16

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AND

JOURNAL OF SCIENCE.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Polit*. lib. i. cap. 1. Not.

VOL. VIII.—SIXTH SERIES.
JULY—DECEMBER 1904.

"Meditationis est perscrutari occulta; contemplationis est admirari perspicua .... Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

———————————

——"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

ALERE FLAMMAM.
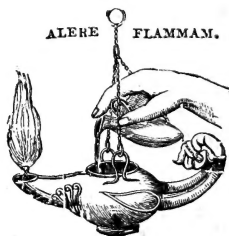
CONTENTS OF VOL. VIII.

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NUMBER XLV.—SEPTEMBER.

NUMBER XLVI.—OCTOBER.

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PLATES.

ERRATA.

Page 267, line 4, *for* (Figs. 5–14) *read* (Figs. 9–20).

  „   „   „ 13, *for* (Figs. 4–12) *read* (Figs. 9–20).

  „ 564, Table VI. *for* electrostatic capacity 1·63 cm. *read* electrostatic capacity 0·63 cm.

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PHILOSOPHICAL MAGAZINE

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[SIXTH SERIES.]

JULY 1904.

I. *The Diffusion of Hydrogen through Hot Platinum.*
By O. W. RICHARDSON, J. NICOL, *and* T. PARNELL [*].

THE fact that platinum is permeable to, and capable of absorbing, hydrogen was first observed by Graham, and has since then been investigated by a number of scientists. Recently, A. Winkelmann [†] (who also made experiments with palladium [‡]) has carefully measured the rate at which hydrogen under different pressures diffuses through thin-walled platinum tubes which are heated and exposed to the atmosphere externally. These experiments show that the rate at which the hydrogen diffuses through the platinum is not proportional to the pressure, as one might naturally have expected it to be, but is very nearly proportional to the square root of the pressure. Winkelmann shows that this result can be explained by supposing the hydrogen to dissociate into atoms which are capable of diffusing through the platinum, whilst the undissociated hydrogen is not.

As there is no other evidence, so far as the authors are aware, that free hydrogen dissociates at high temperatures, it seemed worth trying whether this conclusion would be borne out by further experiments, or whether the facts might not be explained equally satisfactorily on some other hypothesis. With this object in view, the authors set out to examine the variation of the rate of diffusion of hydrogen through hot platinum with the driving-pressure and the

[*] Communicated by Prof. J. J. Thomson, F.R.S.
[†] Drude's *Ann.* vol. viii. p. 388.
[‡] *Loc. cit.* vol. vi. p. 104.

temperature of the metal over as wide a range as possible. The method used was to measure the decrease of pressure in a vessel containing hydrogen, into one part of which a platinum tube was sealed. The tube was heated electrically to a constant temperature. The pressure of the hydrogen at the other side of the walls of the tube was maintained at zero, either by leaving the tube exposed to the air and allowing the hydrogen to burn, or by connecting with a mercury pump.

Of the practical difficulties connected with measurements of this kind, a large number are described by Winkelmann in the papers cited. A most troublesome source of error, which we ultimately managed to get rid of, is the gradual alteration in the state of the metal surface due to long continued heating. Instead of being smooth and featureless as when originally drawn, a platinum tube which has been heated for some time to a high temperature, at any rate in an atmosphere of hydrogen, gradually develops a crystalline structure, becomes full of cracks and excrescences, and, as a rule, ultimately breaks. Naturally this recrystallization of the metal is accompanied by a change in its permeability to hydrogen. Winkelmann found that in his experiments continued heating invariably increased the rate of diffusion of hydrogen through the tubes. After much trouble from this cause, especially through the tubes breaking, we found that the effect could be almost entirely avoided by taking care that the alternate heating and cooling produced no strain on the platinum tube.

In measuring the rate of diffusion of hydrogen through a platinum tube there are obviously two alternative modes of procedure. We may either let the hydrogen in from outside, or out from inside the tube. The latter method is more conducive to simplicity in the general design of the apparatus, since the escaped hydrogen may simply be allowed to burn and there is no need to surround the hot tube with an air-tight containing vessel. It has, however, one very obvious disadvantage. Since the hot platinum only lets hydrogen through, all the traces of impurity originally present gradually get collected in the small tube, so that one runs a considerable risk of getting variations due to the hydrogen becoming impure. For this reason we thought it would be interesting to make preliminary experiments, at any rate, using the other method. Since only rough measurements were made with it, we shall not describe the apparatus used in this part of the investigation, but shall content ourselves with stating that the rate of diffusion was found to vary with the pressure very nearly as the square root, and to increase very quickly with the temperature. So far as they go, these

results are in agreement with those which had previously been found by Winkelmann on the pressure variation and with those which we obtained later, where the second method was used, in both cases.

Experimental Arrangements.

We shall now describe the apparatus used for the experiments which yielded the numbers given in this paper. It has been pointed out that, when the method of letting hydrogen out through a small platinum tube is employed, it is absolutely essential to have the gas as pure as possible. Since hydrogen takes up impurities in passing over rubber, it is necessary to have all joints of sealed or ground glass. The hydrogen was prepared by the action of caustic potash on aluminium. The aluminium was hung up inside a wide test-tube which was open at the bottom and dipped into a beaker containing caustic potash solution. The gas escaped by a tube sealed into the other end of the test-tube and passed through a tube containing solid potash, to absorb most of the moisture, to a three-way tap. This tap enabled the main part of the apparatus to be connected with either the hydrogen

Fig. 1.
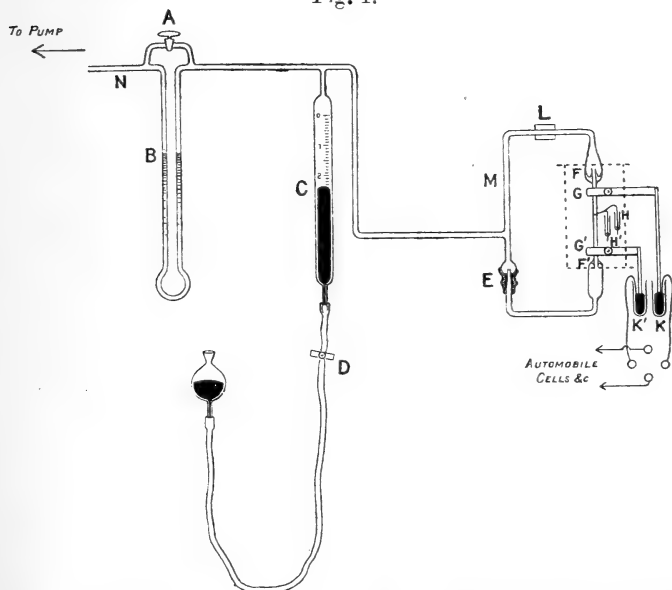
generator or the water-pump. The rest of the apparatus consisted of a Toepler pump, a mercury pressure-gauge, a phosphorus-pentoxide drying-bulb, and the parts shown in fig. 1.

The tube B is a sulphuric-acid gauge whose movements

served to measure the rate of escape of the hydrogen from the heated platinum tube* F F′. In making the measurements the tap in the by-pass A was shut off ; by opening it the parts to the right of A could be connected with the pump and generator, and so could be exhausted or filled with hydrogen.   By raising or lowering the mercury in the burette C, the volume of the imprisoned hydrogen, and hence the rate of movement of the gauge B, could be varied at will. The volume of mercury in the burette could be kept constant, by means of the clip D.   The platinum tube F F′ was 18 cms. long, 2 mms. external diameter, and its walls were 0·1 mm. thick ; it was sealed into the rather wide glass tubes shown at F and F′.   The heating-current entered the tube by the brass clips G G′ which were soldered to stout copper rods dipping into the mercury cups K K′.   The brass clips were not allowed to come into contact with the platinum tube but were separated from it by platinum-foil, in order to prevent the brass from alloying with the hot platinum tube.   The current was supplied by 6 motor-car (secondary) cells, capable of discharging at 42 amperes, and could be regulated by means of an adjustable resistance consisting of two spirals of thick german-silver wire which could be made to slide up and down in two wide glass tubes containing mercury.

It has been pointed out that it is very important that the platinum tube should not be strained by the expansion and contraction produced by heating and cooling.   It was very easy to get rid of straining due to the leads by making their ends dip into mercury cups.   The rigidity of the glass framework F′ E L F might also have been obviated by only sealing the platinum tube into glass at one end and closing the other end by fusion.   It is evident, however, that such an arrangement places the experimenter absolutely at the mercy of the purity of his hydrogen ; for the hot platinum acts as a very efficient filter for the impurities, which are stopped and collect in the tube, out of which they have little chance of diffusing since there is always a current of gas from the rest of the apparatus towards the hot tube.   It is quite easy to see that the percentage of foreign substances in the gas in the tube must be increased during the experiment in the ratio of the volume of gas which has passed out to the volume of the tube.   It was thought that this difficulty would be got rid of to a very large extent, at any rate with comparatively wide tubes like the one used in the present experiments, by having the tube vertical and open to the

* The tubes in these experiments were made of the pure platinum supplied for thermometric work by Messrs. Johnson, Matthey & Co.

hydrogen at each end. The heating would then promote circulation of the gas inside the tube; in fact, we should expect a constant stream of hot gas up the hot tube, equalized by a current down the arm M E. These convection currents would effectually prevent any accumulation of impurities within the tube itself.

The objection to this scheme caused by the putative rigidity of the glass framework was avoided by means of the loose joint E. The upper tube was blown out a little about 1 cm. and slightly constricted about 3 mms. from the end, the edges being pressed outwards with a file. The lower tube was drawn down a little at its end so as to project into the bulbous inflation of the upper tube, and still have plenty of play in the shoulder formed by the constriction lower down. The tubes were then joined by means of thick-walled rubber tubing outside and the joint drowned internally with mercury between the two tubes. This both prevented the hydrogen from coming in contact with the rubber and also made the joint absolutely air-tight. With this arrangement, the upper tube being clamped at L, it was found that the joint allowed several centimetres' play at F′ before the platinum tube was sealed in. It is evident therefore that there could be no appreciable strain due to the much smaller expansion caused by heating.

The temperature of the tube was determined by means of a thermocouple formed of very fine platinum and platinum-rhodium wire. The two wires were welded very carefully by pulling them against the hot tube so that they both made contact at exactly the same point. In this way the galvano-meter deflexion in the thermocouple circuit was found to be almost independent of the direction of the heating current, showing that only a very small fraction of the potential-fall along the tube was shunted through the galvanometer. A small correction was made for this. The thermocouple wires H H′ ran through glass tubes to a point about 1 metre distant, where their ends were soldered to copper wires leading to the galvanometer, both the junctions being placed in test-tubes in a vessel containing cold water, whose temperature could be read. As trouble was at one time experienced owing to the sensitiveness of the D'Arsonval galvanometer changing during the experiments, an arrange-ment was set up by which the thermocouple reading could be compared with that produced by a constant small fraction of the E.M.F. of a storage-cell by simply reversing a commutating switch. The E.M.F. of the cell was checked by means of a sensitive voltmeter from time to time.

order to keep the temperature of the heated tube at all constant, it was necessary to protect it and the adjacent parts from draughts. This was done by building a cylindrical box of asbestos card around them; the position of the box is shown by the dotted lines in the figure. To enable the tube to be seen the asbestos was provided in front with a mica window. Much trouble was also caused at first by the gradual heating of the gas in the tubes F L M E F'. This was ultimately got rid of by wrapping the glass tubes F and F' closely round with spirals of composition tubing, through which a constant stream of water was driven. Under these circumstances it was found that turning on the heating current produced a sudden expansion due to the heating of the gas actually inside the tube, but there was no gradual drift in the sulphuric-acid gauge due to the adjacent glass parts F F' becoming slowly heated.

The thermocouple was standardized directly by determining the melting-points of the pure sulphates of potassium and sodium. To do this a minute quantity of the powdered salt was placed near the middle of the platinum tube and watched carefully through the mica window with a lens. The melting-point was quite sharp, and constant within the error of reading of the galvanometer. Two such determinations were made, one before and the other after the measurements had been carried out, and they agreed absolutely with each other. The melting-points were taken to be $1066°\cdot5$ and $883°\cdot2$ C. respectively [*].

In carrying out the experiments the apparatus was first filled with pure hydrogen. The caustic potash in the generator was first warmed, and a large volume of hydrogen allowed to blow off into the air to make sure that all the air was driven out of the generator. The apparatus was then exhausted, first with the water-pump and finally with the Toepler pump down to about $\frac{1}{50}$ mm. It was then filled slowly to atmospheric pressure with hydrogen from the generator. After standing some time the hydrogen was pumped out and new gas let in. This operation was repeated before the final filling took place. The barometer and the mercury-gauge in the apparatus were then read : the difference of the readings gave the pressure of the hydrogen.

A number of measurements of the rate of diffusion were taken at any one pressure corresponding to a series of temperatures. As a rule the temperatures were those given by deflexions of 80, 100, 120, 140, and 160 mms. on the

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* *Cf.* Heycock and Neville, Journ. Chem. Soc. vol. lxvii. p. 160.]

galvanometer-scale. The deflexion of the thermocouple-galvanometer was kept constant by one observer, by moving the adjustable german-silver resistance, whilst another took the readings of the sulphuric-acid gauge. This was read by means of a millimetre-scale placed immediately behind it on the wooden stand to which both were fastened. By means of a lens there was found to be no difficulty in reading the position of the meniscus to 0·1 mm.; and the times were taken when the meniscus crossed each millimetre, or every second, fifth, tenth, or twentieth, according to the rate at which the hydrogen happened to be diffusing away. It is hardly necessary to state that in taking the readings the tap A was not turned and the sulphuric-acid gauge thrown into action until the temperature had become steady.

After making a series of measurements at different temperatures for any given pressure of hydrogen in the apparatus, the pressure was reduced to about one-half its original value by means of the mercury-pump, and the observations repeated for a series of temperatures at the lower pressure. In this way readings were taken over a range of pressure extending from atmospheric to 2 mms. of mercury. Naturally, as all the pressures were obtained by subtracting the readings of a rather narrow mercury gauge from the barometric height, there was some uncertainty about the readings at the lowest pressures. To get rid of this uncertainty further experiments were made by a slightly modified method which will be described later. At the higher pressures the amount of hydrogen which escaped during any one experiment was small compared with that originally present, so that there was no appreciable alteration of pressure and the leak did not fall off with time. At low pressures, however, this was not the case. For instance, with a total pressure of 10 mms. of mercury, the times required for the sulphuric-acid gauge to move over consecutive runs of 3 mms. were found, in one experiment, to be 27, 29, 29, and 34 seconds respectively; whilst with 2 mms. total original pressure, the time for the first 2 mms. of the gauge was 49 as compared with 63 for the second. In these cases the true value was got by extrapolation.

The object of these experiments being to determine the mass ($\dot{Q}$) of hydrogen which flows out of each unit area of internal surface of the hot tube per second under different conditions of temperature and pressure, it is necessary first of all to obtain a formula connecting this quantity with the gauge-readings and the dimensions of the apparatus. Let R be the numerical value of the product pv for 1 gm. of

hydrogen at $0°$ C. and 76 cms. of mercury. Let p, v, and P, V be the average pressures and volumes of the gas in the portions of the apparatus to the left and right of the stop-cock A (fig. 1) respectively. Let h_1 and h_2 be the initial and final readings of the right-hand side of the sulphuric-acid gauge, σ its internal sectional area, and ρ the ratio of the density of sulphuric acid to that of mercury. Then the total quantity of gas which has diffused out during an experiment is given by

$$-\delta m = -\frac{1}{R}\,\delta(pv) = -\frac{1}{R}\,(p\delta v + v\delta p),$$

where $\delta v = (h_1 - h_2)\sigma$ and $\delta p = 2\rho(h_1 - h_2) + \delta P$, δP being the increase of the pressure on the left-hand side of the gauge produced by the motion of the liquid.

Hence $\qquad\qquad \delta P = -\sigma(h_2 - h_1)\dfrac{P}{V}.$

Substituting we get

$$-\delta m = \frac{1}{R}\,(h_2 - h_1)\left[2\rho v + \sigma p\left(1 + \frac{v}{V}\right)\right],$$

since $p = P$ very nearly. As the effect varies very rapidly with the temperature of the tube, something has to be allowed for the cooling of the platinum tube near the leads. This was done by neglecting one centimetre at each end; thus the tube, which was really 10 cms. from lead to lead, was assumed to have the same effect as a tube 8 cms. long uniformly heated to the temperature of the middle point of the actual tube. This gave 4·52 sq. cms. as the effective superficial area of the hot tube. Putting in this value and values found for ρ, σ, and V, we obtain the reduction formula

$$\dot{Q} = 2\cdot544 \times 10^{-7}\left(\frac{h_2 - h_1}{t}\right)\left[\cdot272v + \cdot0844p\left(1 + \frac{v}{576}\right)\right].$$

The area of cross-section σ was determined by cutting down the gauge and weighing a column of mercury in it of known length. It was important to determine the volume v_0 of the apparatus to the right of the gauge when the mercury was at the zero mark in the burette C_1 with some accuracy. This was done by cutting down the gauge at the point N in the diagram, replacing the sulphuric acid in it by mercury, shutting off the tap A and altering the volume of air in the burette by moving the mercury reservoir. The end N being open to the atmosphere, corresponding readings of the pressure (on the gauge) and of increase of volume, $v - v_0$, (on the burette) were taken. These on reduction gave $v_0 = 46\cdot3$ c.c.

as the mean of two concordant determinations. A similar experiment with the pump, &c., connected at N gave the value of V as 576 c.c.

The aim of the first series of observations at atmospheric pressure was to test whether there was any perceptible change produced in the rate of diffusion by the heating in one experiment. This was done by raising the temperature of the tube in steps of about 140° from 576° to 1136°, making a measurement at each temperature, and then repeating the observations in the inverse order on the downward grade. If there were an effect of the kind looked for, there ought to be a difference between any two observations at the same temperature, the amount of which should increase with the interval between the times at which they were taken. As the rate of escape of the hydrogen is considerable at the highest temperatures, there was a gradual diminution of the total pressure of the hydrogen in the apparatus from 75·9 to 74·1 cms. during the experiment. The numbers were corrected to a pressure of 76·0 cms. by assuming that the flow was proportional to the square root of the pressure, an assumption which will be justified later. The numbers which were obtained are given in the subjoined table.

TABLE I.—P = 76·0 cms.

Temperature Centigrade.	Rate of Diffusion.	($\dot{Q}$).
576°	$4·655 \times 10^{-9}$	$4·60 \times 10^{-9}$
717	$2·05 \times 10^{-8}$	$2·015 \times 10^{-8}$
856	$6·375 \times 10^{-8}$	$6·20 \times 10^{-8}$
995	$1·567 \times 10^{-7}$	$1·542 \times 10^{-7}$
1136	$3·564 \times 10^{-7}$	

The observations were taken in the order indicated by the arrows. It will be seen that the numbers in the right-hand column are all slightly smaller than the corresponding numbers on the left. In no case, however, is the difference greater than the admissible experimental error, and it was not confirmed by later experiments.

After having made a sufficient number of preliminary observations to thoroughly test the working of the apparatus, a series of measurements was made, in the manner already

explained, covering a range of temperature from 576° C. to 1136° C., and of pressure from 76·0 to 0·2 cms. of mercury. The results of all the measurements which were made, using this method, are given in Tables I. and II.

TABLE II.

Tempera-ture (Cent.).	Pressure, cms. of mercury.	$\dot{Q}$.	Tempera-ture (Cent.).	Pressure, cms. of mercury.	$\dot{Q}$.
717	76·56	$1·841 \times 10^{-8}$	995	14·8	$6·32 \times 10^{-8}$
856	76·56	$5·89 \times 10^{-8}$	1136	14·8	$1·54 \times 10^{-7}$
995	72·0	$1·488 \times 10^{-7}$	717	14·3	$7·56 \times 10^{-9}$
1136	71·4	$3·218 \times 10^{-7}$	856	14·3	$2·64 \times 10^{-8}$
717	37·7	$1·171 \times 10^{-8}$	995	14·3	$6·51 \times 10^{-8}$
856	37·7	$4·09 \times 10^{-8}$	1136	14·3	$1·47 \times 10^{-7}$
995	37·7	$1·084 \times 10^{-7}$	717	7·1	$5·64 \times 10^{-9}$
1136	37·7	$2·356 \times 10^{-7}$	856	7·1	$1·884 \times 10^{-8}$
646	27·4	$4·98 \times 10^{-9}$	995	7·1	$4·39 \times 10^{-8}$
717	27·4	$1·080 \times 10^{-8}$	1136	7·1	$9·85 \times 10^{-8}$
787	27·4	$1·89 \times 10^{-8}$	717	2·96	$3·47 \times 10^{-9}$
856	27·4	$3·41 \times 10^{-8}$	856	2·96	$1·16 \times 10^{-8}$
926	27·4	$5·72 \times 10^{-8}$	995	2·96	$2·72 \times 10^{-8}$
995	27·4	$9·37 \times 10^{-8}$	1136	2·96	$5·91 \times 10^{-8}$
1065	27·4	$1·39 \times 10^{-7}$	995	1·12	$1·594 \times 10^{-8}$
1136	27·4	$2·06 \times 10^{-7}$	1136	1·12	$3·41 \times 10^{-8}$
856	14·8	$2·52 \times 10^{-8}$	1136	0·2	$1·28 \times 10^{-8}$

With the exception of those corresponding to pressures nearly atmospheric, all the values of $\dot{Q}$ have been corrected, in the manner already described, for the small variation of pressure caused by the loss of hydrogen during any one experiment. Thus for the four readings where the pressure is stated in the above table to be 37·7 cms. the actual pressures were 37·7, 37·6, 37·4, and 36·7 cms. respectively, whilst the actually reduced values of $\dot{Q}$ were smaller than those tabulated in the ratio of the square root of the actual pressures to the square root of 37·7.

By inspection of the numbers in the preceding tables, it is easy to see that the rate of diffusion increases very rapidly with the temperature of the metal, and also that it falls off with decreasing pressure much less rapidly than if it were proportional to the pressure. For constant increments of temperature, the ratio of the values of $\dot{Q}$ for the initial and final temperature falls off as the temperature increases. Thus

the numbers in Table I. yield the following values of this
ratio:—

t_2.	t_1.	t_2-t_1.	$\dot{Q}_2/\dot{Q}_1$.
717	576	141	4·4
856	717	139	3·09
995	856	139	2·495
1136	995	141	2·29

The values of t_2-t_1 above are not quite constant, but are
sufficiently so to demonstrate the above property. In this
respect, the rate of diffusion resembles the vapour-pressure of
a liquid. The relation between the quantity ($\dot{Q}$) of hydrogen

Fig. 2.

Temperature Centigrade.
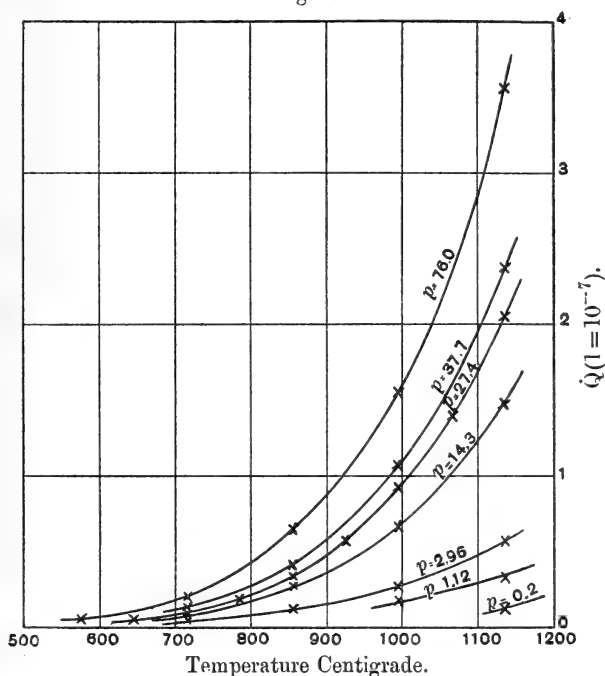
diffusing through the tube per second, on the one hand, and
the pressure and temperature on the other, is made still
clearer by means of the two accompanying diagrams
(figs. 2 & 3).

We come now to the theoretical interpretation of the results which have been obtained. Winkelmann showed that

Fig. 3.
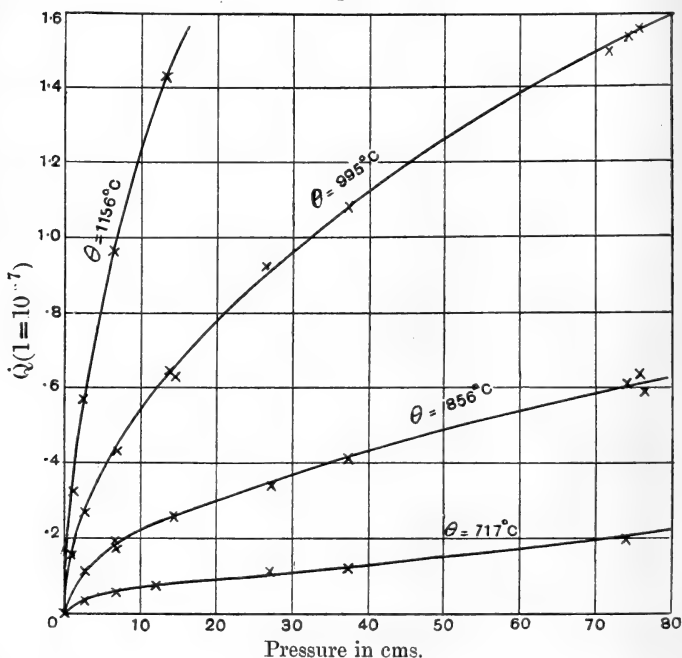
his results could be explained by supposing that the hydrogen dissociated outside the platinum and that only the atoms could get through. On this supposition, he was able to obtain the dissociation constant of the hydrogen. In the units used in his paper (and in the present one), he gives the value of this as 0·64 in two experiments and 0·81 in the other. He points out, however, that there is a considerable choice in the value of this constant which may be taken to fit his results. This might be due to the fact that he takes only three observations to test a formula with two undetermined constants, but as a matter of fact any value of K which does not make the dissociation too big over the comparatively limited range of pressure in his experiments will give the values found. We consider that our experiments demonstrate clearly that a value for the dissociation constant as high as 0·64 is quite inadmissible, and that the external dissociation, for the existence even of which we have no definite experimental evidence, must be much smaller than Winkelmann supposed.

In a recent paper [*], one of the authors has treated the problem of a dissociating gas diffusing in solution, and in the one-dimensional case has arrived at the following expression for the number N of gramme-molecules of gas per second diffusing through a slab of solvent of thickness d:

$$N = \frac{1}{d} \left[\frac{\mu_n}{n} \left(\frac{k_\iota}{A k_0} \right)^{\frac{1}{n}} \lambda_0 + \mu \frac{\lambda_0{}^n}{k_0 A} \right],$$

where n is the number of submolecules produced by the dissociation of each molecule :

μ_n, μ = coefficients of diffusion of the dissociated and undissociated gas molecules, respectively:

k_0 = the dissociation constant outside and k_ι inside the solution ;

$1/A$ is the partition coefficient of the undissociated gas, and λ_0 is a real root of the equation of dissociation

$$c_0{}^n + k_0 c_0 = k_0 P_0,$$

where P_0 is the total concentration of the external gas and c_0 is the partial concentration of the dissociated molecules. All the concentrations are molecular.

Since in the present experiments we are dealing with flow through the walls of an annular cylinder, we ought not to use the one-dimensional solution. It is, however, just as easy to obtain the solution for this case. In the paper cited, it is shown that the concentrations satisfy the equation :

$$\nabla^2 \left(\mu C + \frac{\mu_n}{n} c \right) = 0,$$

where C, c are the concentrations of the undissociated and dissociated gas respectively. Since everything is a function of r only ∇^2 becomes

$$\frac{d^2}{dr^2} + \frac{1}{r} \frac{d}{dr}.$$

The conditions to be fulfilled are : at the external boundary $(r = r_1)$ C = c = 0, and at the internal boundary $(r = r_2)$

$$\mu C + \frac{\mu_n}{n} c = \mu \frac{C_0}{A} + \frac{\mu_n}{n} \frac{c_0}{a} ;$$

C_0, c_0 being the values of C and c outside, and $1/a$ the partition coefficient of the dissociated gas. The necessary

solution is therefore

$$\mu C + \frac{\mu_n}{n} c = \left[\mu \frac{C_0}{A} + \frac{\mu_n}{n} \frac{c_0}{a} \right] \frac{\log r - \log r_1}{\log r_2 - \log r_1},$$

and the quantity diffusing in unit time per sq. cm. at a point determined by r is

$$-\left[\mu \frac{dC}{dr} + \frac{\mu_n}{n} \frac{dc}{dr} \right] = \frac{\left[\mu \frac{C_0}{A} + \frac{\mu_n}{n} \frac{c_0}{a} \right]}{\log r_1 - \log r_2} \cdot \frac{1}{r} \cdot$$

This solution reduces to the other case, of course, on making the radii large compared with their differences. As a matter of fact, with the thin-walled tube we used the difference for the two sets of formulæ was less than the expected error of our absolute measurements, so the first formula, being somewhat simpler, was used in interpreting the results.

Applying these formulæ to the equation $H_2 = 2H$, $n = 2$ and λ_0 becomes the positive root of the quadratic

$$c_0^2 + k_0 c_0 = k_0 P_0.$$

This is

$$\lambda_0 = \frac{k_0}{2} \left[\left(1 + 4 \frac{P_0}{k_0} \right)^{\frac{1}{2}} - 1 \right].$$

It will be noticed that in the general case there are two terms in the diffusion formula, one being proportional to the external concentration of the dissociated, and the other to that of the undissociated gas. The relative importance of these two terms depends on the ratios of the coefficients of diffusion (μ_n, μ) and the solubilities (A^{-1}, a^{-1}). There is a relation between the latter, viz., $a^n/A = k_0/k$. In the case where the fraction of external gas dissociated is small compared with the whole amount $\lambda_0 = C_0 = (k_0 P_0)^{\frac{1}{2}}$ and the rate of diffusion becomes

$$\frac{1}{d} \left\{ \frac{\mu_2}{2} \left(\frac{k_0}{A} \right)^{\frac{1}{2}} P_0^{\frac{1}{2}} + \frac{\mu}{A} P_0 \right\}.$$

In this case therefore there are two terms in the formula, one proportional to the external pressure and the other to its square root. If there were an appreciable amount of dissociation in the hydrogen outside, the first term would increase more rapidly with the pressure than as the square root, and the second more rapidly than the first power.

In order to obtain information on these points, the power of the pressure to which the diffusion was proportional was

tested by plotting $\log \dot{Q}$ against $\log P$. The result is shown on the accompanying diagram (fig. 4). The points marked with dots were obtained from the numbers in Tables I. and II.,

Fig. 4.

Scale of $\log \dot{Q}$.

Scale of $\log P$.
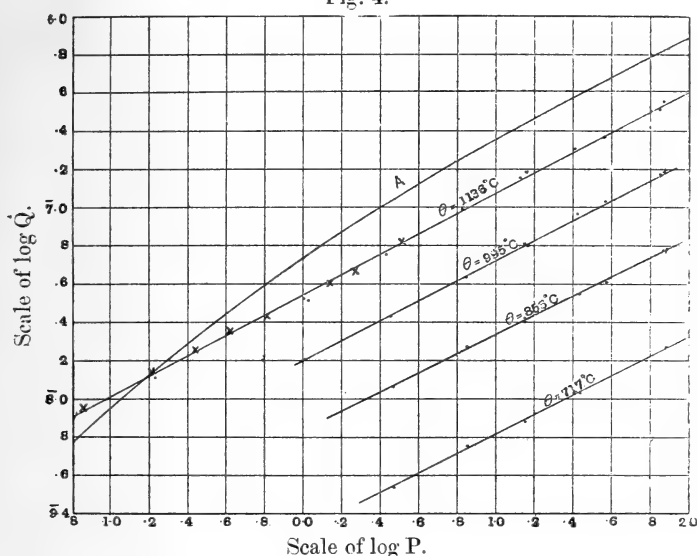
those marked with crosses from another series of experiments which will be described later. The points on these curves represent absolutely every observation taken at the specified temperatures by the method which has been described above. It will be seen that they all fall, within the experimental error, on straight lines whose slope appears to be independent of the temperature. The rate of diffusion is therefore a power of the pressure which may be obtained from the slope of the lines in question. The magnitude of this power for the various temperatures is: for $\theta = 1136°$, ·531; $\theta = 995°$, ·521; $\theta = 856°$, ·500; and $\theta = 717°$, ·508. The mean of all the values is ·515. Since none of the values differs from ·5 by more than the expected error, this appears to be the true value; the fact of the average being 3 per cent. above this is probably due to some constant source of error which has not been corrected for.

To the order of accuracy of our experiments then we conclude that the pressure variation of the diffusion is given by a formula of type $\dot{Q} = AP^{\frac{1}{2}}$, where A is independent of pressure. This is not in agreement with the result of Winkelmann, who makes the diffusion proportional to the concentration of the dissociated hydrogen outside, the amount of

dissociation being that given by $k_0 = 0.64$. In order to compare the two we have plotted the values of $\log \dot{Q}$ on Winkelmann's theory against $\log P$ taking the dissociation constant $k_0 = 0.64$. The result is any line on the diagram parallel to curve A (fig. 4). It will be seen that the two cases are practically identical at pressures greater than 10 cms. It is only at lower pressures that any discrepancy between Winkelmann's view and our results becomes evident. The discrepancy is not due to the experiments having been made at different temperatures; for we find, on calculating back from Winkelmann's value of the amount of hydrogen diffusing through each sq. cm. of his tube per second, that his experiments were made as nearly as possible at 1000° C., whereas our results hold from 717° C. to 1136° C. As a matter of fact Winkelmann's observations, which only extend from about 10 cms. to 70 cms., are all on the flat part of the curve and therefore agree with ours. It is only the interpretation which is different.

In order to substantiate this point it was thought advisable to make further experiments at low pressures, where alone the difference between the two views becomes apparent. This was more especially the case, as the low pressures in the previous experiments were obtained by subtracting the readings of the mercury manometer from the barometric height, and were therefore liable to considerable errors at pressures less than a centimetre. In these experiments the same apparatus (fig. 1) was used as before, but a new mode of procedure was adopted. The apparatus was filled with pure hydrogen in the manner previously described, and was then exhausted with the water-pump till only about 3 cms. pressure of hydrogen remained. The water-pump and the tap A were then shut off, and the parts to the left side of the sulphuric-acid gauge exhausted with the mercury-pump to a pressure of about $\frac{1}{20}$ mm. During this operation the sulphuric-acid column on the left side of the gauge B rose until the difference in the levels on the two sides was about 200 mms. Thus, since the pressure on the left side of the gauge is for all practical purposes zero, we have a volume of gas whose pressure is given by the difference of level in the sulphuric-acid gauge. In this way the readings of the acid gauge were made to give both the rate of escape and the actual pressure of the gas. This arrangement was found to be very satisfactory and naturally very sensitive.

Before giving the observations it is necessary to obtain a formula connecting the gauge-readings with the rate of escape of the gas. Using where not otherwise explained the

notation of pp. 7, 8, we have the equations

$$\dot{m} = \frac{1}{R}(p\dot{v} + v\dot{p}),$$

$$p = \rho(x_1 - x_2), \qquad \dot{p} = \rho(\dot{x}_1 - \dot{x}_2),$$

$$v = V_0 - \sigma x_2, \qquad \dot{v} = -\sigma\dot{x}_2,$$

$$x_1 + x_2 = 2c, \qquad \dot{x}_1 = -\dot{x}_2$$

where dots represent differentiation with respect to t, x_1 and x_2 are the readings of the right- and left-hand side of the gauge respectively, V_0 is the volume to the right of the gauge including the gauge-tube (supposed to extend uniformly to the zero of the scale), and c is the final scale-reading of either side of the gauge. These equations yield for the rate of increase and for the mass of gas left

$$\dot{m} = -2\frac{R}{\rho}\dot{x}_2[V_0 + \sigma c - 2\sigma x_2] \quad . \quad . \quad . \quad . \quad (1)$$

and

$$m = -2\frac{\rho}{R}x_2[V_0 + \sigma c - \sigma x_2] + \text{const.} \quad . \quad . \quad (2)$$

Thus from a knowledge of x_2 and $\dot{x}_2$ we are enabled to calculate the values of $\dot{m}$ and hence of $\dot{Q}$, which is the value of $\dot{m}$ per unit area of internal surface of the platinum tube. We can therefore obtain $\dot{Q}$ as a function of x_2 and of p, which is $= 2\rho(c - x_2)$.

The results of the first series of observations made in this way are given in Table III., the actual scale-readings σ_2 being plotted against the time in fig. 5. The way in which $\dot{Q}$ depended on the pressure was tested by means of these results in two ways. In the first place, by drawing tangents to the curve in fig. 5, $\dot{x}_2$ could be obtained for any value of x_2, and hence $\dot{Q}$ could be calculated from formula p. 8. Since these observations were made at $1158°$ C. they had to be corrected to $1136°$ C. to compare with the earlier observations. This was done by means of the law for the temperature variation of the effect which will be obtained later. The logs of the values of $\dot{Q}$ for $1136°$ C. thus obtained have been plotted against log P in fig. 4, where they are indicated by crosses. It will be seen that within the experimental error they all fall on the straight line given by the previous observations, showing that the rate of diffusion is at any rate approximately proportional to the square root of the pressure.

As it is difficult to draw tangents to a curve like that in fig. 5 very exactly, it was thought that a better test of the

Fig. 5.

Scale of t in minutes.

Scale of $y(=\sigma-x_2)$ in millimetres.
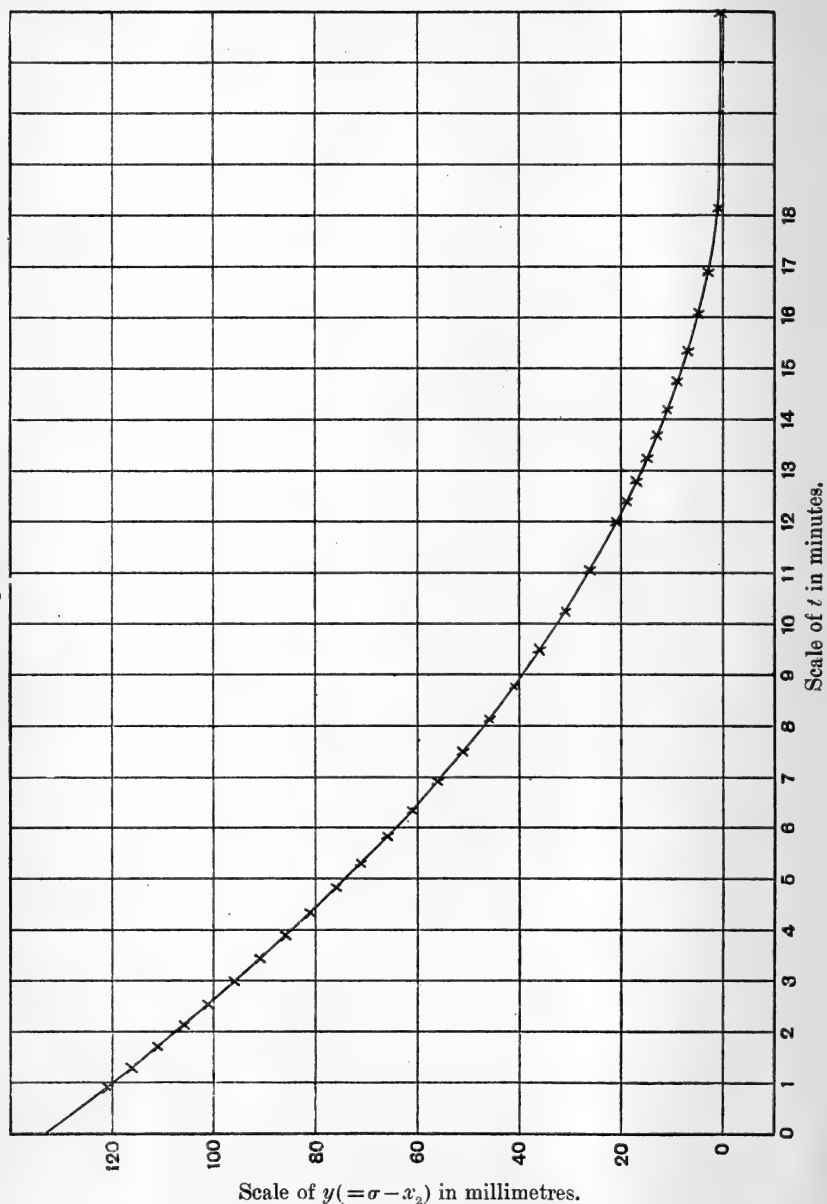
accuracy with which the experimental results were expressed by the square-root law would be obtained by putting $\dot{m}$

proportional to $p^{\frac{1}{2}}$ in equation (1) and testing the experimental numbers by the formula so obtained. To compare with previous results we divide $\dot{m}$ by the area (S) of the internal surface of the platinum tube and obtain instead of (1)

$$\dot{Q} = 2k\rho\dot{x}_2[V_0 + \sigma(c - 2x_2)],$$

where $k = 1/RS$. If in this we put

$$\dot{Q} = Ap^{\frac{1}{2}} = A(2\rho[c - x_2])^{\frac{1}{2}}$$

and integrate, we obtain

$$\frac{At}{k}\left(\frac{1}{2\rho}\right)^{\frac{1}{2}} = B - 2(V_0 - \sigma c)(c - x_0)^{\frac{1}{2}} - \tfrac{4}{3}\sigma(c - x_2)^{\frac{3}{2}}, \quad . \quad (3a)$$

where B is an integration constant. As a matter of fact B determines the time required for all the hydrogen to escape, which is given by the equation $T = (2\rho)^{\frac{1}{2}}kB/A$.

The accuracy of formula (3) has been tested by means of three series of numbers corresponding to different temperatures. The temperature in the first series—those given in Table III.—was $1158°$ C. The first column in the Tables (III., IV., and V.) contains the value of $c - x_2 (= y)$, and the second that of $2(V_0 - \sigma c)y^{\frac{1}{2}} + \tfrac{4}{3}\sigma y^{\frac{3}{2}}$. The third column was obtained by subtracting two of the numbers in column II. from one another and dividing by the corresponding integral of time. In order to obtain as large a difference as possible the sixth row in column II. was subtracted from the first, the seventh from the second, and so on. The resulting values are proportional to the value of A, and their constancy therefore forms one test of the square-root pressure law. The fourth column was obtained by multiplying the mean value of the numbers in the third column by t. Addition of corresponding numbers in the second and fourth columns should then give independent values of B. The values of B thus found are exhibited in column V., and their constancy forms a second test of the square-root law.

TABLE III.

I.	II.	III.	IV.	V.
$y = c - x_2$.	$95y^{\frac{1}{2}} + \cdot 112y^{\frac{3}{2}}$.		$296\cdot5\,t$.	B.
12·1	335·2	...	0	335·2
10·6	313·2	...	21·9	335·1
9·1	289·6	...	45·0	334·6
7·6	264·2	...	69·9	334·1
6·1	236·2	...	97·0	333·2
4·6	204·7	3·00	128·8	333·5
3·1	164·0	3·07	166·0	330·0
2·1	138·0	2·96	196·3	334·3
1·5	116·4	2·94	218·5	334·9
·9	90·2	2·90	246	336·2
·3	51·4	2·92	284	335·4

It will be seen that the agreement of the numbers in both columns III. and V. is extremely good. The extreme difference in column III. is only 5 per cent., whilst the agreement in column V. is still better, the greatest difference amounting to about 2 per cent. The mean value of A from this series of observations was found to be $3 \cdot 924 \times 10^{-8}$ gms. per sq. cm.

The next series of observations (Table IV.) was for a slightly higher temperature ($1176°$ C.) and a much smaller range of pressure (13 mms. of mercury as compared with 32 mms. before). The agreement here is still satisfactory though not so good as before. This is because the limited range of the observations increases the relative errors.

TABLE IV.

I. $y = c - x_2$.	II. $95y^{\frac{1}{2}} + \cdot 112y^{\frac{3}{2}}$.	III.	IV. $346\,t$.	V. B.
4·6	204·7	...	0	204·7
3·1	164·0	...	37·0	201·0
2·1	138·0	...	67·5	205·5
1·5	116·4	3·44	89·0	205·4
·9	90·2	3·27	115·2	205·4
·3	51·4	3·67	149·1	200·5

These numbers give the mean value of A at $1176°$ C. as $4 \cdot 58 \times 10^{-8}$.

The next table (Table V.) gives the values at $1021°$ C. This shows the best agreement of the whole series, for the extreme difference in A is only about 2 per cent., whilst that of B is about 4 per cent. The difficulty of regulating the temperature made errors of this amount unavoidable. The numbers give the mean value of A at $1021°$ C. as $1 \cdot 77 \times 10^{-8}$ gms. per sq. cm. per sec.

TABLE V.

I. $y = c - x_2$.	II. $95y^{\frac{1}{2}} - \cdot 112y^{\frac{3}{2}}$.	III.	IV. $1336\,t$.	V. B.
10·6	313·2	...	0	313·2
9·1	289·6	...	24·45	314·1
7·6	264·2	...	59·35	3236
6·1	236·2	...	80·15	3164
4·6	204·7	...	119·2	3239
3·1	164·0	1·352	147·5	3115
2·1	138·0	1·322	177·7	3157
1·5	116·4	1·330	207·7	3241
·9	90·2	1·334	226·5	3167
·3	51·4	1·340	272·3	3237

An interesting point which is exhibited by formula (3) arises out of the fact that the rate of escape of the hydrogen varies as the square root of the pressure. Confining ourselves to the case where the volume v may be regarded as invariable, we have in general

$$\dot{Q} = -kv\dot{p} = Ap^{\frac{1}{2}},$$

whence

$$At = B - 2kVp^{\frac{1}{2}} = 2kV(p_0^{\frac{1}{2}} - p^{\frac{1}{2}}),$$

if $p = p_0$ when $t = 0$.

We see from this formula that it only requires a finite time for the pressure of the hydrogen to fall absolutely to zero, and indeed the time taken is exactly twice as long as if the rate of escape were always equal to that at the initial pressure. As an illustration, we may calculate the time required for 1 cm. pressure of hydrogen to escape from a vessel whose volume is 1 c.c. when the hot platinum is 0·1 mm. thick, 1 sq. cm. in area, and at a temperature of 1158° C. At this temperature $A = 3·924 \times 10^{-8}$ and k per unit area $= 1/R$, whilst $R = 8·82 \times 10^5$ c.c. × cms of mercury. Putting these numbers in the formula $T = \dfrac{2Vp_0^{\frac{1}{2}}}{RA}$, we find the time required is 58 seconds. Since the time varies as the volume of the apparatus and as the square root of the pressure, we see that with 100 c.c. at 0·1 mm. the time required to reduce the pressure to zero would be very nearly 10 minutes. In this way we are led to what should be an efficient way of obtaining a dead hard vacuum, viz., fill the apparatus, which is provided with a platinum tube such as that used in these experiments, with pure hydrogen several times, exhaust to a low pressure, and finally let the last traces diffuse out by heating the platinum tube.

These experiments formed a very efficient test of the purity of the hydrogen used. Initially there was a pressure on the right-hand side of about 200 mms. of sulphuric acid After the hydrogen had all diffused out by heating the platinum tube, the stopcock A was again turned to see if the pressure was the same on both sides. In no case was any motion of the sulphuric acid visible, though a motion of $\frac{1}{10}$ mm. would have been detected. We conclude, therefore, that the impurities in the hydrogen amounted to less than one part in two thousand.

Since we have now shown that over a range of pressure from 76·0 to 0·1 cm. and of temperature from 717° C. to 1176° C. the value of $\dot{Q}$ varies as the square root of the pressure, the further consideration of the formulæ on pp. 13 & 14

becomes materially simplified. In the first place, we may take our result to mean that the percentage of gas disso- ciated externally is negligibly small, and in the second place there is no experimental evidence of the existence of the term $\frac{\mu}{A} P_0$ proportional to the pressure. The formula for $\dot{Q}$, therefore, which we require is simply

$$\frac{\dot{Q}}{2} = \frac{1}{d} \frac{\mu_2}{2} \left(\frac{k_\iota}{A}\right)^{\frac{1}{3}} P_0^{\frac{1}{2}}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

when μ_2 is the coefficient of diffusion of the atomic hydrogen and k_ι the dissociation constant *inside* the platinum. $\dot{Q}$ is divided by 2 since it has been measured in grammes, whereas the right-hand side is in gramme-molecules. In the original deduction of this formula A was not the inverse of the coeffi- cient of absorption of the undissociated hydrogen as usually defined *, but was the ratio of the internal and external con- centrations ; under those circumstances P_0 was not the pressure but the concentration of the external gas. If we make P_0 the pressure and $1/A$ the coefficient of absorption, the ratio of the two will be the same as before and will be unaffected by changes of temperature : in order to distinguish the new meanings we will write the new quantities P and A_0 respectively. Now the coefficient of diffusion μ_2 must evi- dently be proportional to the mean velocity of agitation of the molecules, and therefore varies as $\theta^{\frac{1}{2}}$, where θ is the absolute temperature. It is rather more difficult to specify the way in which the solubility $1/A_0$ of the undissociated hydrogen will vary with the temperature. It is not probable that the variation will be very rapid. At high temperatures it seems likely *à priori* to resemble the case of a gas dis- solving in a liquid without chemical action at ordinary temperatures. For such cases it has been shown by Bohr † that the temperature variation of the absorption coefficient is given by

$$\frac{1}{A_0} (\theta - n) = K_1,$$

θ being the absolute temperature and n and K constants. At high temperatures this becomes much the same as $\theta/A_0 = K'$, so that we shall assume that A_0 varies as θ. Since both μ_2 and $A_0^{\frac{1}{2}}$ vary approximately as $\theta^{\frac{1}{2}}$ it follows

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* *Cf.* Winkler, *Ber.* vol. xxii. p. 1773.

† Wied. *Ann.* 1897, vol. lxii. p. 644. There are a number of mistakes in this paper owing to the sign — having been replaced by the sign ÷.

that $\dot{Q}$ should vary with θ very approximately as k_ι. Now the way in which k_ι varies with the temperature can easily be obtained. It is given by the equation *

$$\frac{d(\log k_\iota)}{d\theta} = \frac{1}{R\theta^2}(q_\iota + pv'), \quad \cdots \quad (4)$$

where q_ι is the heat of dissociation of hydrogen when the dissociation takes place inside platinum, and v' is the change of volume when 1 gm.-molecule of hydrogen dissociates completely at pressure p. Expressing q_ι in small calories, $pv' = R = 2$ † very approximately, so that we may write the equation

$$\frac{d(\log k_\iota)}{d\theta} = \frac{q}{2\theta^2} + \frac{1}{\theta}, \quad \cdots \quad (5)$$

whence

$$\left. \begin{array}{l} k_\iota^{\frac{1}{2}} = C\theta^{\frac{1}{2}}e^{-\frac{q_\iota}{4\theta}} \\[2mm] \frac{1}{2}\log\frac{k_\iota}{\theta} = C' - \frac{q_\iota}{4\theta} \end{array} \right\} \quad \cdots \quad (6)$$

We have seen that the number of gramme-molecules which flow through unit area per second is given by

$$\frac{\mu_2}{2d}\left(\frac{k}{A_0}\right)^{\frac{1}{2}}P^{\frac{1}{2}},$$

where P is the number of gramme-molecules per c.c. of hydrogen at $0°$ C. under the pressure π (in cms. of mercury) observed during the experiments. To reduce to the mass $\dot{Q}$ per sq. cm. per sec., we have to multiply by the molecular weight of hydrogen (2), and by the factor $7{\cdot}68 \times 10^{-3}$ to reduce to pressure readings. We then get

$$\dot{Q} = 7{\cdot}68 \times 10^{-3} \times \frac{\mu_2}{d} \times \left(\frac{k_\iota}{A_0}\right)^{\frac{1}{2}} \times \pi^{\frac{1}{2}},$$

or since $d = {\cdot}01$ cm. in the experiments,

$$\dot{Q} = \frac{\mu_2}{1{\cdot}3}\left(\frac{k_\iota}{A_0}\right)^{\frac{1}{2}}\pi^{\frac{1}{2}}. \quad \cdots \quad (7)$$

Eliminating k_ι between (6) and (7) we obtain :—

$$\log_{10}\frac{C\mu_2}{A_0^{\frac{1}{2}}} = \log_{10}\dot{Q} - \tfrac{1}{2}\log_{10}\theta + \frac{q_\iota}{9{\cdot}2\theta} - \log_{10}\pi^{\frac{1}{2}} + {\cdot}115 \quad (8)$$

The quantity on the right-hand side is thus seen to be a

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* See for instance J. J. van Laar, *Arch. Néerl.* [ii.] v. p. 490.

† Van't Hoff, 'Lectures on Theoretical Chemistry,' vol. i. p. 21.

constant for all temperatures and pressures. A series of values of $\log_{10} \dfrac{C\mu_2}{A^{\frac{1}{2}}}$ will be given below.

To determine q_ι the heat required to dissociate 1 gm.-mol. of hydrogen inside the platinum, we must take two corresponding values of $\dot{Q}$ ($\dot{Q}_1$, $\dot{Q}_2$) and of θ (θ_1, θ_2) and eliminate the quantities which are independent of temperature in equation (8). We then obtain the formula

$$q_\iota = 9 \cdot 2 \frac{\log_{10} \dot{Q}_1 - \frac{1}{2} \log_{10}\theta_1 - \log_{10}\dot{Q}_2 + \frac{1}{2}\log_{10}\theta_2}{1/\theta_2 - 1/\theta_1},$$

Values of q_ι for different pressures and different ranges of temperature are given in Table VI. The mean value of 15 determinations yields $q_\iota = 36,500$ small calories.

The values of q_ι and of $\log_{10} \dfrac{C\mu_2}{A_0^{\frac{1}{2}}}$ corresponding to the numbers in Tables I. to V. have been calculated, and are shown in Table VI. The first column gives the pressure π in cms. of mercury, the second the absolute temperature, and the third the values of q_ι calculated from the values of $\dot{Q}$ given in the earlier tables. The values of q_ι were calculated for approximately constant differences of temperature at any one pressure ; thus the first value (33,800) corresponds to $1129 - 849$, the second (34,600) to $1268 - 990$ and so on. The fourth column gives the right-hand side of equation (8) for all values of π and θ. The fifth gives the mean of these values for each pressure, whilst the sixth gives the corresponding values of $C\mu_2/A_0^{\frac{1}{2}}$.

Inspection of the numbers in column III. shows that they are all fairly constant. The two lowest values correspond to the experiments in Tables III.–V. As in these experiments the temperatures were not determined so accurately as in the others, they cannot be regarded as of equal value in determining q_ι. We may also leave out the other value (31,900) marked with an asterisk on the ground that it is much less than any of the others. The mean of the fifteen remaining determinations gives $q_\iota = 36,500$ whilst the mean for the whole eighteen was 36,800.

In obtaining the numbers in column IV. by evaluating the right-hand side, the mean value 36,500 cal. was taken for q_ι. It will be seen that the resulting values of $\log_{10}C\mu_2/A_0^{\frac{1}{2}}$, although very constant at the middle of the range, have a distinct tendency to fall off at the lower pressures. This is brought out more clearly by the mean values of $\log_{10}C\mu_2/A_0^{\frac{1}{2}}$ at any one pressure given in column V., and by the corresponding

TABLE VI.

I. π.	II. $\theta°$ abs.	III. q_ι.	IV. $\log_{10} \dfrac{C\mu_2}{A_0^{\frac{3}{2}}}$.	V. Means of IV.	VI. $C\mu_2/A_0^{\frac{3}{2}}$.
76·0	849	......	$\bar{6}$·055		
	990	......	$\bar{7}$·994		
	1129	33,800	$\bar{7}$·966		
	1268	34,600	$\bar{7}$·946		
	1409	37,000	$\bar{7}$·975	$\bar{7}$·987	$9·7 \times 10^{-7}$
37·7	990	......	$\bar{7}$·906		
	1129	......	$\bar{7}$·930		
	1268	38,100	$\bar{7}$·941		
	1409	37,400	$\bar{7}$·946	$\bar{7}$·931	$8·53 \times 10^{-7}$
27·4	919	......	$\bar{7}$·931		
	990	......	$\bar{7}$·941		
	1060	......	$\bar{7}$·907		
	1129	......	$\bar{7}$·921		
	1199	37,900	$\bar{7}$·924		
	1268	36,900	$\bar{7}$·948		
	1338	38,300	$\bar{7}$·988		
	1409	38,500	$\bar{7}$·958	$\bar{7}$·940	$8·71 \times 10^{-7}$
14·3	990	......	$\bar{7}$·927		
	1129	......	$\bar{7}$·951		
	1268	36,800	$\bar{7}$·931		
	1409	36,600	$\bar{7}$·952	$\bar{7}$·940	$8·71 \times 10^{-7}$
7·1	990	......	$\bar{7}$·953		
	1129	......	$\bar{7}$·957		
	1268	34,900	$\bar{7}$·913		
	1409	35,050	$\bar{7}$·927	$\bar{7}$·938	$8·67 \times 10^{-7}$
2·96	990	......	$\bar{7}$·931		
	1129	......	$\bar{7}$·936		
	1268	35,100	$\bar{7}$·894		
	1409	31,900 *	$\bar{7}$·847	$\bar{7}$·902	$7·98 \times 10^{-7}$
1·12	1268	......	$\bar{7}$·875		
	1409	35,800	$\bar{7}$·870	$\bar{7}$·873	$7·47 \times 10^{-7}$
	1294	......	$\bar{7}$·875		
	1431	40,300 *	$\bar{7}$·906		
	1449	43,000 *	$\bar{7}$·936	$\bar{7}$·906	$8·06 \times 10^{-7}$

values of $C\mu_2/A_0^{\frac{1}{2}}$ in column **VI.** This is probably bound up with the fact, mentioned on page 15, that the experiments did not make $\dot{Q}$ vary accurately as $p^{\frac{1}{2}}$, but as some slightly

higher power varying from ·5 to ·53. If we reduced the above experiments by assuming that $\dot{Q} \propto p^{\cdot 52}$ over the whole range, the variations in column **VI.** would all be reduced to within the error of experiment. But as was pointed out on page 15, it seems more reasonable to suppose that the slight deviation from the square-root law is due to some uncorrected error. There are many possible causes for such an effect : for instance, the variation of the convection currents in the hydrogen inside the platinum tube might cause its temperature to vary slightly with the pressure when the temperature outside was kept constant. The mean of all the 34 values of $\log_{10} C \mu_2 / A_0^{\frac{1}{2}}$ given in column **IV.** is $\bar{7}\cdot 934$, whence the geometric mean of all the values of $C \mu_2 / A_0^{\frac{1}{2}}$ is $8\cdot 59 \times 10^{-7}$. We thus obtain for the complete formula for the mass of hydrogen per second diffusing through each square centimetre of the platinum disk thickness d maintained at temperature θ when the gradient of the square root of the pressure between its faces is $\pi^{\frac{1}{2}}/d$,

$$\dot{Q} = 6\cdot 60 \times 10^{-9} \frac{\pi^{\frac{1}{2}}}{d} \, \theta^{\frac{1}{2}} e^{-\frac{9\cdot 125}{\theta}}.$$

The constancy of the numbers in the preceding tables shows that this formula holds with considerable accuracy over a range of temperature from $576°$ C. to $1176°$ C., and over a range of pressure from $76\cdot 0$ cms. to 1 mm. There are signs of deviation at low temperatures and at the extremes of pressure, but these may be due to unavoidable experimental errors.

The quantity A which occurs in the above formulæ is the partition coefficient for undissociated hydrogen H_2, and is to be carefully distinguished from the quantity which would be measured by an experimental determination of the solubility of hydrogen in platinum. It is, however, easy to obtain a formula for the ratio between the total internal and external concentrations of hydrogen inside and outside platinum on the view developed in this paper. If we neglect the concentration of the external dissociated hydrogen compared with that of the undissociated, we have, in the notation previously adopted :

$$C_1 = \frac{1}{A} C_0 \text{ and } c = (k_\iota C_\iota)^{\frac{1}{2}} = \left(\frac{k_\iota C_0}{A} \right)^{\frac{1}{2}},$$

whence the total internal concentration reckoned in grammemolecules per c.c.

$$= C_\iota + \tfrac{1}{2} c = \frac{C_0}{A} \left(1 + \tfrac{1}{2} \left[\frac{A}{C_0} k_\iota \right]^{\frac{1}{2}} \right).$$

We have seen that k_i increases rapidly with the temperature, so that at high temperatures the second term would become the predominant one. Thus on this view the total solubility of hydrogen in platinum would increase rapidly with the temperature. According to Roscoe and Schorlemmer's ' Chemistry ' (vol. i. p. 139), platinum at a red heat absorbs 3·8 times its volume of hydrogen, whereas at 100° C. it only absorbs ·76 times its volume. This is therefore a confirmation of the preceding hypothesis.

An interesting application of these results may be made with the object of elucidating the mechanism of the negative ionization produced by hot platinum in an atmosphere of hydrogen, which has been investigated by H. A. Wilson[*]. Wilson showed that the negative ionization produced by hot platinum was increased to thousands of times its former value by immersion in an atmosphere of hydrogen. The part due to the hydrogen appeared to be proportional to the gas-pressure over most of the range, and varied with the temperature according to a formula $(a\theta^{\frac{1}{2}}e^{-b/\theta},\ a$ and b constants) first given by one of the authors[†], and shown to apply generally to the negative ionization from hot metals. Wilson also showed by experiments on the lag of the electrical leak behind changes in the pressure and temperature of the wire, that the ionization was probably not produced by the external hydrogen, but by the hydrogen dissolved by the platinum. This was in agreement with the argument, given by one of the authors in the paper just cited (p. 546), based on the magnitude of the negative leak from hot carbon, that the ionization in case of that substance at any rate could not be due to the external gas.

We have seen that the preceding experiments indicate that the concentration of the dissociated hydrogen in the platinum is proportional to the square root of the external pressure. The ionization therefore cannot be a function of the dissolved atoms singly, but must be either due to their interaction (recombination or production by dissociation), or it may be due to some action of the dissolved molecules singly. If, following Wilson, we may assume that above a certain temperature the total solubility of hydrogen in platinum begins to decrease, we can explain all the results by supposing that the negative ions are produced by the collisions of hydrogen atoms in the surface layer, or in the body of the platinum. We see at once that the concentration N of the hydrogen atoms inside is $(k_i C_i)^{\frac{1}{2}} = \left(\dfrac{k_i C_0}{A_0}\right)^{\frac{1}{2}}$.

Now, if w is the mean velocity, the number of collisions per second is proportional to $N^2\overline{w}$. But $A_0 \propto \theta_1 k_t = C^2 \theta e^{-q/2\theta}$ and $\overline{w} \propto \theta^{\frac{1}{2}}$, so that the ionization would vary as

$$\theta^{\frac{1}{2}} e^{-\frac{q_t}{2\theta}} P.$$

This formula agrees well with the results of Wilson, who found for q_t at 133 mms. the value 36,000, whereas the number given in this paper is 36,500. The weak point in this argument lies in the assumption that the total solubility of the hydrogen falls off with increasing temperature, since we have seen that it is exceedingly probable that at the temperatures at which the experiments in the present paper were made the solubility rises very rapidly with temperature. Reference to Wilson's paper[*] will, however, show that such an assumption is necessary to explain the lag of the electric leak behind changes of temperature. We shall not dwell further on this somewhat intricate subject as we hope to make further experiments bearing on the matter.

So far as we have been able to discover, the first suggestion that hydrogen undergoes dissociation in passing through hot metals was made by Ramsay[†], to explain some facts in connexion with the diffusion of hydrogen through palladium. The same opinion was also put forward somewhat later by Hoitsema[‡]. Winkelmann (*loc. cit.*) appears to have been the first to apply this idea to the case of platinum.

The principal results of this investigation may be summed up as follows:—

The rate of diffusion of hydrogen through the walls of a hot platinum tube which is maintained at zero pressure (or exposed to the air) on one side is very approximately proportional to the square root of the pressure of the hydrogen on the other side over a range of pressure from 1 to 760 mms.

This result can be explained by supposing that the hydrogen is dissociated and that the dissociated atoms pass freely through the platinum. Such an assumption probably involves the presence of a certain amount (which may be vanishingly small) of both molecules and free atoms both inside and outside the platinum.

There is no clear evidence of the existence, either of any appreciable fraction of dissociated gas, even at the lowest pressures, outside the platinum or of any appreciable fraction of the gas passing through in the combined state.

The results cannot be explained on any theory which makes the number of diffusing particles proportional to the number

[*] Phil. Trans. A. vol. ccii. p. 266.
[†] Phil. Mag. vol. xxxviii. p. 206 (1894).
[‡] *Zeitschr. f. Physik. Chem.* xvii. p. 40 (1895).

of molecules of hydrogen per c.c. outside, as, for instance, by supposing an action between the hydrogen molecule and the palladium, wherein one atom was taken up and the other rejected. A theory in which the palladium combined with a molecule of hydrogen, and then detached the atoms separately, would yield results similar to those given by the theory here adopted, and therefore probably explain the results. In fact, such a theory would be thermodynamically equivalent to the one here given.

On this hypothesis a formula for the number of gramme-molecules of gas diffusing per c.c. per second, has previously been found by one of the authors. In the case where the diffusion of the undissociated gas is very small and the dissociated fraction of the external gas is negligible, this becomes

$$\frac{1}{d}\frac{\mu_2}{2}\left(\frac{k_\iota}{A_0}\right)^{\frac{1}{2}}P^{\frac{1}{2}};$$

where d is the thickness of the walls, μ_2 the coefficient of diffusion of the atoms through the platinum, k_ι the dissociation constant of the hydrogen inside the platinum, A_0 the solubility of molecular hydrogen in platinum, and P the external concentration of the gas at 0° C. under the pressure in the experiments. $k_\iota^{\frac{1}{2}}$ is shown to be $= C\theta^{\frac{1}{2}}e^{-q_\iota/4\theta}$, where C is a constant, θ the absolute temperature, and q_ι the heat of dissociation of the hydrogen inside the platinum.

A mean of 15 concordant determinations of the heat of dissociation (q_ι) of hydrogen inside platinum gives 36,500 calories. For the mean value of the constant $\mu_2 C/A_0^{\frac{1}{2}}$ we find $8\cdot59 \times 10^{-7}$. Putting in these numbers, the mass $\dot{Q}$ of gas diffusing per sec. through each square cm. of a platinum disk d cms. thick when the pressure on one side is π cms. of mercury and on the other zero, the absolute temperature being θ, is given by

$$\dot{Q} = 6\cdot60 \times 10^{-9}\frac{\pi^{\frac{1}{2}}}{d}\theta^{\frac{1}{2}}e^{-\frac{9\cdot125}{\theta}}.$$

This formula has been shown to hold with considerable accuracy over a range of pressure from $0\cdot1$ to $76\cdot0$ cms. and of temperature from 576° C. to 1176° C.

Finally the experiments point to the view that the ionization produced by hot platinum in an atmosphere of hydrogen is due to collisions taking place between the atoms of hydrogen dissolved in the platinum.

In conclusion, the authors wish to record their gratitude to Professor Thomson for his advice and suggestions during the course of this work, which was carried out in the Cavendish Laboratory.

II. *On Normal Piling, as connected with* Osborne Reynolds's
   Theory of the Universe. *By* Prof. J. D. Everett,
   F.R.S.*

PROF. O. REYNOLDS'S theory was set forth in an
elaborate paper communicated to the Royal Society in
Feb. 1902, and accepted for the Transactions. It is published
as a separate volume of about 250 quarto pages, under the title
"The Sub-Mechanics of the Universe." It is briefly summarised
in the Rede Lecture for 1902, which is published as a small
book, with excellent pictorial illustrations, under the title "On
an Inversion of Ideas as to the Structure of the Universe"
(Camb. Univ. Press). Its fundamental supposition is that
the universe consists of equal small and perfectly hard
spherical grains, arranged for the most part in the manner
usually adopted for piles of shot, hence called by Prof.
Reynolds NORMAL PILING. As a knowledge of the structure
of such piles is essential to an intelligent discussion of the
theory, and is unfamiliar to most students of physics, I propose
to commence by giving a full description of it.

When 4 equal spheres touch one another, their centres are
at the 4 corners of a regular tetrahedron. The tetrahedron
has 6 edges, each joining the centres of a pair of spheres ;
and if the 4 spheres are a portion of a normal pile, each of
these pairs belongs to a continuous line of spheres in contact,
extending all through the pile. The whole pile can thus be
resolved in 6 different ways into parallel lines of spheres in
contact. Every sphere in the interior of the pile belongs to
6 of these lines, and is therefore touched by 12 spheres, at
the opposite ends of 6 diameters. Its centre is the meeting-
point of the vertices of 8 of the tetrahedrons; and the gaps
between these tetrahedrons would be exactly filled by
6 square pyramids ; all the 8 edges of a square pyramid
being of the same length as an edge of a tetrahedron.

The pile can be split up into planes of spheres, either
parallel to the base of a square pyramid, or to any face of a
tetrahedron. In the former case the spheres in the plane
layer are in square formation, so that each touches 4. In the
other case the spheres in the plane tier are in triangular
formation and each touches 6, whose centres are at the
corners of a regular hexagon. This last is the arrangement
which gives the closest possible packing of spheres *in plano*.
The tiers which have this formation are parallel to a face of
the fundamental tetrahedron ; and as a tetrahedron has
4 faces, we have our choice of 4 ways of splitting up a normal
pile into tiers of closest formation.

A pile can be built up in the form of a regular tetrahedron, and each of its 4 faces will then be composed of spheres in triangular formation. Or it can be built up in the form of a square pyramid; in which case the 4 sloping faces will be in triangular formation, while the base and all planes of spheres parallel to the base will be in square formation. The vertical planes of spheres parallel to the diagonals of the square base will also be in square formation.

A normal pile can thus be split up into planes of spheres in 7 different ways, there being 3 sets of parallel planes containing spheres in square formation, and 4 sets containing spheres in triangular formation.

Perhaps the clearest idea of the relative position of spheres in normal piling is obtained by imagining a solid chequer built up of equal cubes alternately black and white, so that every section taken parallel to the cube-faces is like a chess-board. Then, if we imagine all the cubes of one colour (say all the blacks) to be annihilated, and each white cube to be replaced by a sphere having the same centre, and of diameter equal to the diagonal of a cube-face, these spheres will just fit together as they stand, and will constitute a normal pile. A section through cube-centres, taken parallel to a cube-face, is in square formation; and a section taken perpendicular to a body diagonal is in triangular formation.

If we think of a normal pile as placed with a set of squarely formed layers horizontal, each sphere rests on 4 in the layer below, and in its turn supports 4 in the layer above, these last 4 being vertically over the first 4. Besides touching these 8 in adjoining layers, it also touches 4 in its own layer, making 12 in all.

On the other hand, if we think of the pile as placed with one of its sets of triangularly-formed tiers horizontal, each sphere rests on 3 in the tier below, supports 3 in the tier above, and also touches 6 in its own tier, again making 12 contacts in all.

The following is another representation of normal piling:— Divide space into equal cubes by 3 sets of parallel planes. Then the centres of the spheres are to be at the cube-corners, and at the middle points of the cube-faces; the common sphere-diameter being made equal to the distance of the middle point of a face from a corner of the face.

Another representation (the one adopted by Prof. O. Reynolds) consists in dividing space into cubic compartments as before, with the same sphere-diameter, but placing the sphere-centres at the cube-centres, and at the points of bisection of

the cube-edges. This representation has the advantage that each cube, considered by itself, shows the positions of the 12 spheres which touch a single sphere placed at the centre.

Another representation is obtained by dividing space into parallelepipeds whose edges are equal and parallel to three edges of the fundamental tetrahedron. The corners of the parallelepipeds will be the positions of the sphere-centres.

Coming now to the subject of closeness of packing :—It is possible to pack spheres in a box in a symmetrical arrangement in which each sphere touches 6 instead of 12, the points of contact being the ends of three diameters at right angles. The space thus occupied by a given number of spheres will be $\sqrt{2}$ times as great as that occupied in normal piling. It is also possible to adopt a symmetrical arrangement in which each sphere touches 8, the points of contact being given by drawing lines from the centre of the sphere to the corners of the circumscribed cube. This will give a density of packing intermediate between the other two.

It would, however, be a mistake to suppose that normal piling is the only system which gives the maximum of compactness. Every system of piling that has the maximum compactness must consist of parallel tiers in triangular arrangement; and each sphere in a given tier must touch 3 in each adjacent tier, besides 6 in its own; but these conditions allow, in adding each successive tier, a choice between two positions. In normal piling the choice is always made in the same way. In the system which comes next in order of simplicity, and which may conveniently be called *antinormal* piling, the choice is made alternately in one way and in the other. The departure from normal piling cannot begin till we come to the third tier : and if there are $2+n$ tiers the number of different arrangements is 2^n, all giving the maximum of compactness.

In normal piling the choice is governed by a fixed tetrahedron ; any two spheres that touch must be in a line parallel to one of its edges. The alternative choice is represented by the edges of a second tetrahedron base-to-base with the first. The base is parallel to the tiers, and must not be turned about ; but as regards the 3 edges which meet in the vertex, the change is the same as if they were rotated through 60° in the plane of the tiers. Each sphere in a tier is surrounded by 6, giving 6 gaps 60° apart. Three spheres touching each other can be laid over 3 of the gaps, 120° apart ; and the question is, which of the 2 sets of 3 thus available shall we choose.

In normal piling there are 6 directions of lines of touching spheres, 3 of them being in the tier planes, and parallel to

the edges of the base of our tetrahedron of reference. The other 3 are parallel to the edges which meet in the vertex of the tetrahedron; and as soon as we depart from normal piling these lines of spheres are broken. Moreover, in normal piling there are, in addition to the assumed set of parallel tiers, 3 other sets parallel to the 3 faces of the tetrahedron that meet in the vertex. In departing from normal piling, we twist these faces alternately one way and the other through 60°, and thus destroy these 3 sets of parallel tiers.

As regards the 3 sets of parallel planes containing sphere-centres in square formation, they have all disappeared. In place of the regular equidistant planes of this kind, we have to draw planes 3 times as close, if we wish to include all the sphere-centres; the distribution of centres in each plane being in alternate stripes of greater and less closeness, the average closeness being of course one third of what it was in normal piling.

If, instead of attending to the centres of the spheres, we attend to the tangent planes at every point of contact of 2 spheres, we find that, in normal piling, each sphere is inscribed in a dodecahedron, whose faces are equal and similar rhombuses, the point of contact being in each case the centre of the rhombus. Space is thus partitioned into equal and congruent cells. The dodecahedron has 24 edges consisting of 4 sets of 6 parallels; and if we take a section across one of these sets, each of the two halves has the shape of a bees' cell. If we rotate one half through 60°, the halves will still fit together; and in their new position they form the dodecahedron in which each sphere is inscribed in anti-normal piling.

According to Prof. O. Reynolds's theory, the material universe (including æther as well as ordinary matter) consists of equal spherical grains, exactly alike, and infinitely hard, so that when they collide the rebound is instantaneous, without loss of kinetic energy.

For the most part, their free paths are infinitesimal compared with their diameters; and their packing differs only infinitesimally from normal piling, so that each grain is securely hedged in by its 12 neighbours, with extremely little room to disport itself between them. But though this is the prevailing condition, there are exceptional spots where there is a crack or loose joint in the piling, affording opportunity for grains to pass across it, and thus partially change their neighbours. "Surfaces of misfit" is a name frequently applied to these places of weakness; and according to the theory they are closed surfaces, approximating to the form of

spherical shells, the enclosed nucleus of grains being in normal piling. The surface of misfit, together with the enclosed nucleus, is called a *negative inequality* ; and the magnitude of the negative inequality is reckoned by the number of grains which are deficient.

According to the theory, these surfaces of misfit travel through the grains after the manner of solitary waves. The mode of propagation is, that the impulses propagated through external grains towards the loose joint are stronger from one side than from the other, and there is a preponderating transfer of grains across the gap in obedience to the stronger impulse, causing the gap to travel in the opposite direction, as a geometrical consequence ; just as a bubble travels upwards through water in virtue of the water moving downwards.

Each of these negative inequalities is an atom of ordinary matter: what we call the momentum of moving matter being merely the manifestation of the real momentum of grains moving in the opposite direction. The apparent mass of an atom is measured by the number of grains that are wanting in its surface of misfit. Calculations are given to show that a moving surface of misfit tends to continue moving with a uniform velocity of translation and with unchanged magnitude.

Again, calculations are given to show that two of these surfaces of misfit have a tendency to approach each other. The argument is to the effect that their mutual approach increases the average compactness of arrangement, and is therefore promoted by the perpetual battering from outside which is equivalent to hydrostatic pressure. Furthermore, the calculation shows that the mutual force between the two atoms varies inversely as the square of their distance. Gravitation is thus explained as a partial relief of pressure in front, allowing a *vis à tergo* to prevail.

This calculation of attraction inversely as square of distance is based on a first approximation, which is sufficient when the distance is a large multiple of the diameter of an atom. It neglects the square of the small ratio of the diameter to the distance. But when the two atoms are very close, this square must be taken into account. At a certain small distance it gives much stronger attraction, thus explaining cohesion and surface-tension ; and for still closer approach it gives repulsion.

Again, electricity is explained by a particular kind of disarrangement called a *complex inequality*. It is conceivable

that, in the rubbing together of portions of matter built up in the manner above described, a small cluster of grains may be torn away from its place, and shifted to another neighbouring place. Two opposite disarrangements are thus produced. At the place from which the cluster has been removed, surrounding grains are pushed inwards to fill the gap. At the place to which the cluster has been removed, surrounding grains are pushed outwards to make room for it. Each of these two opposite disarrangements produces dilatation, by rendering the packing abnormal; though Prof. Reynolds calls one of them a *positive inequality*, and uses language which conveys the impression that the intrusion of the cluster of grains produces an increase of compactness.

By reasoning which is difficult to follow, the attempt is made to prove that these two dissimilar centres of disturbance will exhibit mutual attraction. They are regarded as constituting the two opposite kinds of electricity.

Another kind of complex inequality is introduced to explain magnetism. Suppose a spherical cluster of grains to receive a forcible twist, large enough to make portions near its equator (where the motion is largest) change their external neighbours; while portions near the poles have not moved far enough to change their neighbours. These latter portions of the sphere will tend to return to their original position, while portions which have passed a little beyond the position of unstable equilibrium will tend to move further away. Rotational stresses will thus exist between the spherical cluster and the surrounding grains, the stresses being opposite-ways round in different portions. The strains accompanying these stresses involve dilatation; and attractions and repulsions will be exhibited, governed by the criterion that the tendency is for less space to be occupied. The reasoning is not worked out in detail; but the brief statement is made that permanent magnetism is thus explained.

The propagation of light is ascribed to transverse displacement of the granular medium by successive impacts; and Röntgen rays, to normal displacement, similarly propagated. Calculations are given to show that, when proper values are assigned to the diameters, masses, velocities, and mean free paths of the grains, the diminution of the energy of a wave, to the fraction $\frac{1}{e^2}$ of its original amount, will occupy some millions of years in the case of transverse waves, and less than a millionth part of a second for normal waves, the distance traversed by the normal waves in undergoing this diminution being 2800 metres,

The same assumptions, as to the grains, which give these results, give also the correct velocity of light, and the correct value of the gravitation constant.

Prof. Reynolds nowhere hints at the existence or possibility of closest piling other than the normal kind. I do not think I am going too far in saying that he identifies a tendency to closest packing with a tendency to that particular kind of closest packing which he names " normal piling."

It is necessary to face the question, whether this ignoring of other modes of closest piling vitiates the arguments by which the theory is supported.

It is legitimate, in framing a hypothesis as to the structure of the universe, to assume the particular structure which best answers the purpose of yielding an explanation ; and where there is a choice between several hypotheses, the simplest is the best, provided it proves sufficient. Of all the possible modes of closest piling, normal piling is incomparably the simplest.

The theory does not expressly assert that the grains, coming together fortuitously, have settled down into this simple arrangement ; but nevertheless the question does occur to an inquiring mind whether fortuitous clashing might be expected to lead, in the long run, to such a result. It is near akin to the question whether equal grains of shot, shaken together in a bag, will arrange themselves in normal piling. Prof. Reynolds gives experiments showing that shot so treated do arrange themselves in closest packing, but adduces no evidence as to the particular kind of closest packing that they assume.

Looking at the matter from a theoretical point of view, we have, on the one hand, the fact that, in the casual laying of 3 successive tiers in closest order, it is an even chance whether the piling is normal. In the casual laying of 4 tiers, the chance is only 1 in 4 ; for 5 tiers it is 1 in 8, and for $2 + n$ tiers it is 1 in 2^n.

On the other hand, we have the fact that a normally piled cluster has 6 sets of parallel lines of balls, and these will form efficient battering-rams in collisions with other clusters which can only have 3.

If, instead of collision, we suppose a statical push, as in a football maul, there is still the same advantage. Balls ranged in parallel lines, pushing end on, will be better able to retain their formation than balls with only oblique support behind them.

Again, as has been pointed out by Mr. Barlow [*] in a different connexion, if we suppose that, in the first instance,

[*] W. Barlow, " A Mechanical Cause of Homogeneity, &c." Sci. Proc. Roy. Dublin Soc. vol. viii. pt. vi. (1897) p. 535.

the balls, under the influence of the tendency to compression, form themselves into small clusters in different kinds of closest piling, there will be misfits at the junctions between such clusters, involving a waste of space; and adjacent clusters will tend to unite and become continuous, so as to occupy less room. Such union is most easily effected when the tiers of the two clusters are nearly parallel to begin with. If each of the two clusters has only one set of tiers (as in the case of a cluster not in normal piling), these tiers are very unlikely to be nearly parallel. If each of them has 4 sets of tiers (as in normal piling) the probability is immensely increased.

The tiers here spoken of are those in triangular formation. And the case is strengthened if we include tiers in square formation; for of these each normal cluster has 4, and the other clusters have none.

For these reasons, clusters in normal piling will unite together much quicker than other clusters.

They will thus become larger than other clusters, and their increased size will add to the effectiveness of their 6 sets of battering-rams.

With these advantages in the struggle for existence, it appears to me feasible to maintain that, in the fortuitous clashing of a universe of grains, with free paths gradually diminishing, normal piling would eventually become the prevailing system.

Prof. O. Reynolds's theory as it actually stands deals, not with remote history, but with the universe as it is; and it supposes the great majority of the grains to be at present locked in their places, with no opportunity for change except at the surfaces of misfit. At these surfaces, the motion is so restricted that the stability of the formation does not seem to be seriously threatened.

I have not made any attempt to verify the elaborate statistical calculations with which Prof. Reynolds's paper abounds; and in the parts of the paper that I have read carefully, several conclusions are drawn which to me are not obvious, but appear very questionable.

My present purpose is not controversy, but explanation; and the style of the paper is so excessively technical that a good deal of explanation seems necessary before intelligent controversy can begin. I have chiefly aimed at an explanation of the geometrical conditions which underlie the system supposed; thereby clearing the way for a more searching criticism, and helping towards the working out of the very fruitful suggestions which the theory contains.

III. *On the Force on a Magnetic Particle in a Magnetic Field.* *By* Prof. A. ANDERSON, *M.A., LL.D.*[*]

A SMALL particle of soft iron, or a small magnet, when placed in a magnetic field is acted on by a force in the direction in which the intensity of the field increases most rapidly. This is, in general, only true when the field is conservative.

Consider a magnetic particle, P (fig. 1), whose axis of

Fig. 1.
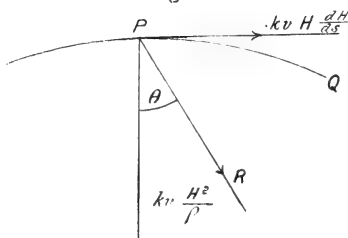
magnetization is along a line of force, PQ. It is easy to show that the forces acting along the tangent and normal are, respectively,

$$M\frac{dH}{ds} \quad \text{and} \quad M\frac{H}{\rho},$$

where M is the moment of the particle, H the intensity of the field, and ρ the radius of curvature of the line of force at P.

If the particle be of small susceptibility, these expressions may be written

$$kvH\frac{dH}{ds} \quad \text{and} \quad kv\frac{H^2}{\rho},$$

v denoting the volume, and k the susceptibility.

Let the resultant, R, of these forces make an angle θ with the normal. Then, if this direction is that along which the intensity of the magnetic field increases most rapidly, the rate of increase perpendicular to R is zero, or

$$\frac{dH}{ds}\cos\theta - \frac{dH}{dn}\sin\theta = 0.$$

And hence

$$\frac{H}{\rho} = \frac{dH}{dn}.$$

* Communicated by the Author.

Let PQ, P′Q′ (fig. 2) be two indefinitely close lines of

Fig. 2.
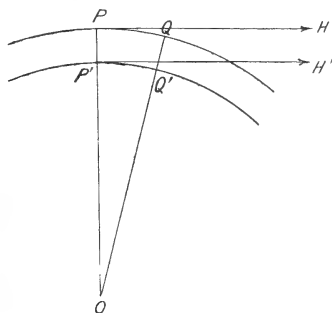
magnetic force, and let H be the magnetic intensity at P and H′ that at P′, PP′ and QQ′ being normals to the line of force at P and Q. Then, since

$$\frac{H}{\rho} = \frac{dH}{dn},$$

it easily follows that

$$\frac{H}{P'Q'} = \frac{H'}{PQ}.$$

The work done in carrying unit pole round the circuit PQQ′P′P is, therefore, zero; from which it follows that the field of force is conservative.

As an example of a non-conservative field, take the case of a current of electricity in a straight wire or cylindrical liquid column of circular cross section. The intensity of field inside, on the supposition that the current is uniformly distributed over the area of the cross section, is $\frac{2Cr}{a^2}$, where r is the distance from the axis and a the radius of the wire. The force on a particle of soft iron inside is $\frac{4kvC^2r}{a^4}$, but it is in the direction in which the magnetic intensity *diminishes* most rapidly. Inside a wire conveying a current of electricity a magnetic potential does not exist, and the field is non-conservative.

We may now go a step farther and find the direction of the force on a magnetic particle in a plane field of given magnetic intensity, assuming that there exists a distribution of electric currents at right angles to the plane. Let the direction of the current be the positive direction through

PQQ'P'. We have, easily,

$$\frac{\mathrm{H}}{\rho} - \frac{d\mathrm{H}}{dn} = 4\pi w,$$

where w is the current density per unit area.

Let PX (fig. 3) making an angle ϕ with the normal PO

Fig. 3.
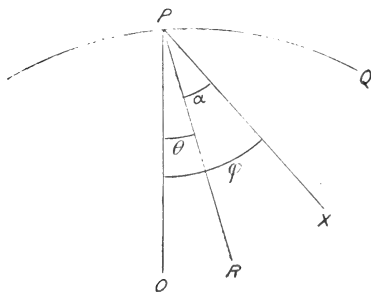
to the line of force at P, be the direction in which the magnetic intensity increases most rapidly, and PR, making an angle θ with PO, the direction of the force on a magnetic particle at P. Also, let the angle XPR$=\alpha$.

Then

$$\tan \theta = \frac{d\mathrm{H}}{ds} \Big/ \frac{\mathrm{H}}{\rho},$$

and

$$\tan \phi = \frac{d\mathrm{H}}{ds} \Big/ \frac{d\mathrm{H}}{dn};$$

from which it follows that

$$\tan \alpha = \frac{4\pi\rho w \sin \phi \cos \phi}{\mathrm{H} - 4\pi\rho w \sin^2 \phi}.$$

If $\phi = 0$, $\alpha = 0$, which is the case for a particle **outside a** straight wire carrying a current.

If $\phi = \pi$, $\alpha = \pi$, which is the case for a particle **inside a** wire carrying a current.

To solve the general case, let u, v, w be the components of current density at any point, α, β, γ the components of magnetic intensity, λ, μ, ν the direction cosines of the direction in which the magnetic intensity increases most rapidly, and λ', μ', ν' the direction cosines of the force on the particle.

Then

$$\frac{\lambda}{\alpha\dfrac{da}{dx}+\beta\dfrac{d\beta}{dx}+\gamma\dfrac{d\gamma}{dx}}=\frac{\mu}{\alpha\dfrac{da}{dy}+\beta\dfrac{d\beta}{dy}+\gamma\dfrac{d\gamma}{dy}}=\frac{\nu}{\alpha\dfrac{da}{dz}+\beta\dfrac{d\beta}{dz}+\gamma\dfrac{d\gamma}{dz}},$$

and

$$\frac{\lambda'}{\alpha\dfrac{da}{dx}+\beta\dfrac{da}{dy}+\gamma\dfrac{da}{dz}}=\frac{\mu'}{\alpha\dfrac{d\beta}{dx}+\beta\dfrac{d\beta}{dy}+\gamma\dfrac{d\beta}{dz}}=\frac{\nu'}{\alpha\dfrac{d\gamma}{dx}+\beta\dfrac{d\gamma}{dy}+\gamma\dfrac{d\gamma}{dz}}.$$

These equations, with the relations

$$4\pi u=\frac{d\gamma}{dy}-\frac{d\beta}{dz},\quad 4\pi v=\frac{d\alpha}{dz}-\frac{d\gamma}{dx},\quad 4\pi w=\frac{d\beta}{dx}-\frac{d\alpha}{dy},$$

are sufficient to find the angle between the force on a magnetic particle and the direction in which the magnetic intensity increases most rapidly, the field being due to permanent magnets, and to a given distribution of currents.

It can easily be seen that the relation

$$\begin{vmatrix} \lambda, & \lambda', & \beta w-\gamma v \\ \mu, & \mu', & \gamma u-\alpha w \\ \nu, & \nu', & \alpha v-\beta u \end{vmatrix}=0$$

holds between the components of magnetic intensity and current and the two directions considered. Hence if a line be drawn perpendicular to both the current and magnetic intensity, a magnetic particle will be urged in a direction lying in a plane through this line and the direction in which the magnetic intensity increases most rapidly.

Queen's College, Galway.
    March 30, 1904.

Postscript.

The complete solution may be put as follows :—

Let λ, μ, ν be the direction cosines of the direction of greatest increase of the magnetic intensity H, λ', μ', ν' those of the force F on the magnetic particle ; then

$$p\lambda=\frac{1}{2}\frac{dH^2}{dx},\quad p\mu=\frac{1}{2}\frac{dH^2}{dy},\quad p\nu=\frac{1}{2}\frac{dH^2}{dz},$$

$$p'\lambda'=\frac{1}{2}\frac{dH^2}{dx}+4\pi(v\gamma-w\beta),$$

$$p'\mu'=\frac{1}{2}\frac{dH^2}{dy}+4\pi(w\alpha-u\gamma),$$

$$v'\nu'=\frac{1}{2}\frac{dH^2}{dz}+4\pi(u\beta-v\alpha).$$

Thus $p = H\dot{H}$, where $\dot{H}$ is the maximum rate of increase of magnetic intensity.

Draw a line perpendicular to both the current density and magnetic intensity, its direction being that of translation of a right-handed screw rotated from current to magnetic intensity, and let its direction cosines be λ'', μ'', ν''. Then if C be the current density, and ϕ the angle between C and H,

$$p'\lambda' = p\lambda + 4\pi CH \sin\phi\,\lambda'',$$
$$p'\mu' = p\mu + 4\pi CH \sin\phi\,\mu'',$$
$$p'\nu' = p\nu + 4\pi CH \sin\phi\,\nu'';$$

$$\therefore\ p'^2 = p^2 + 8\pi CHp \sin\phi(\lambda\lambda'' + \mu\mu'' + \nu\nu'') + 16\pi^2 C^2 H^2 \sin^2\phi;$$

or if ψ denote the angle between (λ, μ, ν) and $(\lambda'', \mu'', \nu'')$,

$$p'^2 = H^2[\dot{H}^2 + 8\pi C \sin\phi \cos\psi\dot{H} + 16\pi^2 C^2 \sin^2\phi].$$

The force F, as has been shown above, lies in the plane through (λ, μ, ν) and $(\lambda'', \mu'', \nu'')$. Let it make an angle ψ' with $(\lambda'', \mu'', \nu'')$; then it follows at once that

$$p'\cos\psi' - p\cos\psi = 4\pi CH \sin\phi.$$

Thus

$$\cos\psi' = \frac{\dot{H}\cos\psi + 4\pi C \sin\phi}{[\dot{H}^2 + 8\pi C \sin\phi \cos\psi\dot{H} + 16\pi^2 C^2 \sin^2\phi]^{\frac{1}{2}}}.$$

When $C = 0$, or $\phi = 0$, $\psi' = \psi$, the direction of F in both these cases being that in which the magnetic intensity increases most rapidly.

If $v = $ volume of the particle and k the susceptibility supposed small, it is easily shown that

$$F = kvH[\dot{H}^2 + 8\pi C \sin\phi \cos\psi\,\dot{H} + 16\pi^2 C^2 \sin^2\phi]^{\frac{1}{2}},$$

which reduces to the well-known expression

$$kvH\dot{H}, \text{ when } \phi \text{ or } C \text{ vanishes.}$$

The expression for $\cos\psi'$ leads to the very simple result that F is the resultant of two forces, one, equal to $kvH\dot{H}$, along the direction in which the magnetic intensity increases most rapidly, and the other, equal to $4\pi kvHC \sin\phi$, along the line perpendicular to the current and magnetic intensity.

IV. *On the Theory of Diminishing Entropy.*
By S. H. Burbury, *F.R.S.**

1. **W**ITH regard to the first difficulty which I found in Willard Gibbs's Chapter XII., I gladly accept Dr. Bumstead's conclusion that the best way of meeting it is to define the density by finite elements of volume. I hope that Dr. Bumstead will consider the question what element of volume should, in given cases, be chosen for the definition.

2. Adopting this course, we dispense with the theorems by which D for Willard Gibbs's ensemble, or ρ for the coloured liquid of his illustration, were proved to be constant in time, these theorems being based on the unworkable definition of density as the limiting ratio of quantity to the containing volume. Our familiar experience that stirring the coloured liquid tends to reduce it to a uniform condition is thus accepted as an experimental law, and we have made our mathematics consistent with it. But from this point of view the title of Chapter XII., " On the motions of systems and ensembles of systems through *long* periods of time," seems wholly misleading. For the tendency to uniformity does not depend on length of time in any especial manner. If $\frac{d\mathbf{E}}{dt}$ be the rate of change of entropy with the time, it is not necessary that t should be *long* in order to make $\frac{d\mathbf{E}}{dt}$ finite.

3. Willard Gibbs's explanation that this tendency depends on the order in which we proceed to the limits, if I rightly understand it, leaves the result after all indeterminate. I may be allowed to illustrate it, as I understand it, thus. Let x be a quantity for which I have to call a value, and I may call it as great as I like without limit. Let y be another quantity for which Dr. Bumstead has to call a value, and he may call it as small as he likes without limit. Then what about the product xy? If I have to call first, then no matter what I call x, Dr. Bumstead can make xy infinitely small. But if he calls first, then no matter what he calls y, I can make xy infinitely great. I think a solution of the problem in this form could hardly be considered satisfactory.

4. My second difficulty was and is as to the value of η to be attributed to a system. Here Dr. Bumstead says I have misapprehended Willard Gibbs's teaching, which has to do, not with single systems, but with large averages. I think however that, doubtless owing to my lack of perspicuity,

Dr. Bumstead has misapprehended me. I will restate my difficulty in Willard Gibbs's *ipsissima verba*. Twice at the head of p. 150 (which is the point where the difficulty arose) he speaks of " the average value of η for " a group of systems. Now it is impossible for an average " value for " the group to exist, unless there exists a definable " value for " each system in the group. I give an example. The average " chest measurement for " the students now in Yale University is, say, 36 inches. That implies that there exists a definite system of measurement for each individual student. We cannot avoid giving the definition by saying that our anthropometric investigations are " statistical," and have only to do with large averages, not with individual measurements. What then does Willard Gibbs mean by the " value of η for " a system ?

5. At time t' a certain group of systems occupy the elementary extension in phase DV'. Their number is $Ne^{\eta'}DV'$ by definition of η Chapter I. Then I suppose at that instant η' is " the value of η for " each system of the group, and is therefore the average value for the group. Another group at the same instant occupy DV_1', and their number is $Ne^{\eta_1'}DV_1'$. Now Willard Gibbs says η' is " an arbitrary function of the phase," meaning, as Dr. Bumstead understands it, an arbitrary function of the phase at the instant t'. At another instant t'', which may, so far as we have yet gone, be before or after t', the systems which at t' were all in DV'_1 are scattered over more than one elementary extension in phase. What at the second instant is " the value of η for " any system of the group ? One possible answer is, that each system of the group always retains the same η which was " the value for it " at t'. But if that were adopted no system ever changes its η. There can, therefore, be no question of the mean value $\bar{\eta}$ increasing or diminishing for the ensemble.

Another possible answer is that by an " arbitrary function of the phase " is meant an arbitrary function of the phase in which the system is for the time being. According to this view, the number of systems in any elementary extension in phase is constant. The ensemble is then in statistical equilibrium. Willard Gibbs cannot have meant either this answer or the other. I think he has not quite explained his meaning.

The Theory of Diminishing Entropy.

6. The remaining difficulty is that the argument of pp. 150, 151 will work either forwards or backwards. Entropy may, for all that appears, either increase or diminish. The

difficulty arises only in those cases in which the motions of separate parts of the bodies under consideration are dynamically reversible. That being the case, is it possible for a motion of the aggregate of such separate parts to be dynamically irreversible? This question cannot be decided by the mere use of a mathematical formula which is wholly independent of time, such as Willard Gibbs's Theorem IX. We have a choice of two ways of dealing with the problem.

7. One way, which Willard Gibbs in the passage quoted by Dr. Bumstead recommends us to follow, is to admit that our mathematics, in Willard Gibbs's language mathematical fictions, give us no information whether the motion is forwards or backwards, that is towards or from uniformity, but that experience, in Willard Gibbs's language the real world, teaches us that it is towards uniformity. We must, then, call experience to our aid as the interpreter of our mathematics. That is one way. It is perfectly logical, and satisfactory so far as the teaching of experience is complete.

8. I can call to mind only one writer by whom this method has been followed expressly and with decision. Prof. Ladislas Natanson, namely, obtains the equation $\dfrac{d\mathrm{E}}{dt} + \dfrac{d\mathrm{K}}{dt} = 0$ for a fluid, E denoting kinetic energy of visible motion of masses, K kinetic energy of those molecular motions which we cannot directly measure. And he says our mathematics give us no information whether in this equation $\dfrac{d\mathrm{E}}{dt}$ is positive and $\dfrac{d\mathrm{K}}{dt}$ negative, or *vice versâ*, but we must, he says, accept the teaching of experience, that $\dfrac{d\mathrm{E}}{dt}$ is negative, as interpreting our mathematical result.

9. Another way is to make an assumption in aid of our mathematics. The mathematical theorem, *e. g.* Theorem IX. of Willard Gibbs's Chapter XI., although it cannot alone determine the direction of motion, may with the aid of an assumption determine it. This method is really the appeal to experience in another form, because the assumption itself, unless consistent with experience, is illegitimate. But as this method has been followed by Boltzmann in his H Theorem, which is the type of theorems of this kind, we must consider it fully.

10. Boltzmann's assumption leads to this result, that the number of pairs of molecules which by their mutual encounter pass out of a certain class in time dt is proportional to the number of pairs which are in the class for the time being. Let $f\,du_1 \ldots dw_n$ denote the number per unit of

volume of molecules of the class m whose velocities at a given instant lie between the limits

$$u_1 \ldots u_1 + du_1 \ldots w_n \ldots w_n + dw_n.$$

And let $f' \, du_1' \ldots dw_n'$ denote the number per unit volume of the class m' whose velocities lie between the limits

$$u_1' \ldots u_1' + du_1' \text{ &c.}$$

Then the number per unit volume of pairs (m, m') whose velocities respectively lie within the limits aforesaid is

$$f \cdot f' \, du_1 \ldots dw_n'.$$

That statement involves no physical assumption. Then he says the number of these pairs for which in time dt m collides in a given way with the m' of the same pair is proportional to $f \cdot f' \, du_1 \ldots dw_n'$. That statement involves a physical assumption of the most important and far-reaching character, namely (see Art. 11), that the chance of a molecule having velocities within the assigned limits is at every instant independent of the positions and velocities of all the other molecules for the time being. For if not, the number of collisions between m and m' is not necessarily proportional to the product of $f \, du_1 \ldots dw_n$ and $f' \, du_1' \ldots dw_n'$, because there may exist a stream by virtue of which m and m' have on average velocities of the same sign, and that affects the frequency of such collisions. It may conceivably be true that there are no such streams, but it is not axiomatic. On this assumption Boltzmann proves rigorously that Maxwell's law of distribution of velocities, namely, that the chance of the molecules $m_1 \, m_2 \ldots m_n$ having at any instant velocities between the limits u_1 and $u_1 + du_1 \ldots w_n$ and $w_n + dw_n$ is proportional to $e^{-h\Sigma m(u^2+v^2+w^2)} \, du_1 \ldots dw_n$, is a sufficient condition for stationary motion; and he further proves, by the H theorem, that it is a necessary condition, because H diminishes irreversibly until that distribution is attained.

11. It will be observed that Maxwell's law expresses the condition of independence of Art. 10, and cannot, therefore, be true unless the condition of independence is true. Boltzmann's assumption, therefore, whatever its apparent form, as it is sufficient to prove Maxwell's law, necessarily involves the condition of independence.

12. Now it has been shown (see the paper by Mr. J. H. Jeans on the Kinetic Theory of Gases, Phil. Mag. May 1903), and I think it is unquestionably true, that in a system of molecules which have finite dimensions, or act on each other at finite distances, the continued existence of the condition of independence is inconsistent with the continuity of

the motion, and therefore impossible. It follows, since
Maxwell's law of distribution of velocities asserts the con-
dition of independence and cannot exist without it, that
Maxwell's law and Boltzmann's H theorem are alike impos-
sible, at least as accurate propositions. It does not follow
but that results obtained by their use may be true for rare
gases to sufficient approximation.

13. Inasmuch as Willard Gibbs has made great use of
this same expression $Ae^{-h\Sigma m(u^2+v^2+w^2)}$ in the form $D = e^{\frac{\psi-\epsilon}{\theta}}$,
it is right to point out that the objection, which is I think
fatal to it in Boltzmann's theorem, is wholly inapplicable to
it as used by Willard Gibbs. Because Willard Gibbs's
systems, unlike Boltzmann's molecules, do not collide with,
or in any way influence, one another. That is evidently
required by the argument of Chapter I.

14. As a further example of an assumption in aid of our
mathematics, we might assume the following. Consider the
two equal elementary extensions in phase DV_1 and DV_2.
Assume that the number of systems which in time dt pass
out of DV_1 into DV_2 is proportional to the number which for
the time being are in DV_1. That is, if x_1 denote the density
in phase for DV_1, the number in question is $\kappa_{12} x_1 DV_1 dt$,
where κ_{12} is a positive constant. Similarly the number
which pass from DV_p to DV_q is $\kappa_{pq} x_p DV_p dt \ldots$ and
$\kappa_{pq} = \kappa_{qp}$ is a positive constant. That seems a reasonable
assumption. If we now make $H = \Sigma x \log x$, the summation
including all the DV's, $\dfrac{dH}{dt} = \Sigma \log x \dfrac{dx}{dt}$, and this is re-
ducible, as in Boltzmann's H theorem, to a series of terms
of the form $\kappa(x - x') \log \dfrac{x'}{x}$, and is therefore negative. Our
assumption has enabled us to remove the ambiguity of sign
in the direction of motion.

15. So Planck, in his *Irreversible Strahlungsvorgange*,
makes a similar assumption, viz., that the energy which his
resonator emits into space per unit of time is proportional
to the energy which for the time being it has.

16. Again, in the diffusion of gases, let there be in a
vessel a mixture of oxygen and nitrogen at uniform pressure
and temperature of the combined gases, but the proportions
of the respective gases being initially different at different
points. Experience teaches that the mixture left to itself
tends to become uniform, so far as this, that any space *large
enough to be measured* will ultimately contain oxygen and
nitrogen in the same proportions, from whatever part of the

containing vessel it is taken.   As regards *less than measurable spaces* we do not know by experience that this is the case, nor have we any right to infer from analogy or otherwise that it is so.

We might assume in explanation of this our experience—and it would be a reasonable assumption—that the number of oxygen resp. nitrogen molecules which pass out of any measurable space S per unit of time is proportional to the number which for the time being are within S.   And that assumption would be made to lead to the application of Boltzmann's logarithmic theorem above mentioned.

17. The assumption that we have just made in the diffusion problem Art. 16, although it follows as a consequence of the impossible condition of independence, may be true while that condition is untrue.   Again, the condition of independence, although it cannot be true at every instant without violating the continuity of the motion, may be true at some one initial instant, if, for instance, the molecules have velocities attributed to them at that instant at haphazard. And the consequences which follow from the condition of independence may be expected to continue in force in a greater or less degree, and so entropy to diminish, for some time after the initial instant.   After a sufficient lapse of time this would in an isolated system cease to be true.   But if the system under consideration is not isolated, but continually receiving disturbances from without due to causes independent of its own state of motion for the time being, the effect of these will be to restore in whole or in part the initial haphazard character of the motion.   Now any actual material system generally does from time to time experience external disturbances which will have this effect.   So the condition of independence, though inadmissible as an accurate law, may be expected to have great influence in fact.

18. I think the conclusions that may fairly be drawn at present are as follows :—

(*a*) If a material system consist of parts, the motions of which are separately reversible, it is impossible to prove that the motion of the aggregate is irreversible without making some physical assumption to be used in support of the pure mathematical theorem, whether that be the logarithmic theorem employed by Boltzmann and by Willard Gibbs (Theorem IX.) or any other mathematical theorem independent of time.

(*b*) Such assumption, to be admissible, must be mathematically possible, and must be at least not inconsistent with experience.

(*c*) Boltzmann's assumption, the condition of independence, is not mathematically possible, at least as an accurate proposition.   I think there exists at present no theorem satisfying the necessary conditions.

19. Does not the theory of a general tendency of entropy to diminish take too much for granted?   To a certain extent it is supported by experimental evidence.   We must accept such evidence as far as it goes and no further.   We have no right to supplement it by a large draft on the scientific imagination.

V. *The Magnetic Susceptibility of Alloys of Bismuth and Tin.   By* S. C. LAWS, *B.A., B.Sc., St. John's College, Cambridge,* " 1851 *Exhibition" Scholar* *.

O N the conclusion of some experiments on the Thomson effect in alloys of bismuth and tin, it was thought desirable to make further use of the specimens employed in that investigation in order to obtain some information concerning the magnetic behaviour of the alloys.   The present paper contains the results of experiments made to determine the magnetic susceptibility of these alloys.

The method employed involves the measurement of the mechanical force acting on a cylinder of the substance, arranged along the axis of a coil of wire, when a current passes through this, the cylindrical rod of metal being situated partly within and partly without the coil.   Boltzmann † has shown how to calculate this force in terms of the susceptibility *k*, the dimensions of the coil and cylinder, and the current passing.   The expression obtained becomes much simpler when the length of rod within the coil is equal to that projecting outside ; accordingly, this has always been arranged throughout the experiments.

Von Ettingshausen ‡ has previously utilized this method for determining the magnetic susceptibility of bismuth.   In his experiments the rod of metal was arranged horizontally at one end of a light horizontal beam suspended by a fine metal wire and provided with a counterpoise at the other end ; the force acting on the metal cylinder when a current was sent through the coil was then known from the torsion produced in the suspending wire.

Von Ettingshausen also used three other distinct methods

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* Communicated by Prof. J. J. Thomson, F.R.S.
† Boltzmann, *Wien. Ber.* lxxx. p. 687 (1879).
‡ Von Ettingshausen, Wied. *Ann.* xvii. p. 287 (1882).

for determining the susceptibility of bismuth, but regards this as the best of the four.

For my purpose this method has also the advantage that the specimens are required in the form of cylindrical rods, so that precisely the same specimens are used in these experiments as had been employed in the investigation referred to above ; also the amount of apparatus required is comparatively small and the experiments could be quickly carried out.

In the experiments the coil was 40 cms. in length and consisted of 4158 turns of No. 16 B.W.G. copper wire arranged in 21 layers upon an ebonite cylinder 2·70 cms. in external diameter, and having stout flanges of vulcanised fibre screwed on to its ends.

In order to measure the force acting on a rod this was suspended from one arm of a sensitive balance, the coil being arranged so that the rod hung freely along the axis and in such a position that, when the current was sent through the coil, the rod assumed a position with half its length projecting beyond the coil. In order to secure this adjustment the coil was mounted on a tripod provided with levelling screws. Above the coil, and moving with this, was fixed a scale by means of which it could be ascertained when the rod occupied the required position (fig. 1).

Care was also taken to arrange the coil vertical, with the rod situated along its axis.

When a current was sent through the coil it was found that each of the specimens examined showed a decrease in weight, so that they were all diamagnetic.

As this decrease of weight was never more than 3 milligrammes, methods had to be adopted to render visible a much smaller deflexion of the beam of the balance than could be indicated with the ordinary pointer.

Accordingly, the double suspension method due to Lord Kelvin, and used by Poynting in his experiments on the gravitation constant[*], was employed. For this purpose the pointer belonging to the balance was removed and replaced by one made of brass and bent forwards at a short distance from its free end at right angles to the beam of the balance.

One end of a single short fibre of raw silk was fixed to this horizontal free end, the other end of the fibre being attached to the end of a fixed horizontal brass rod situated a few millimetres in front of the end of the pointer.

From the silk fibre was suspended, by means of two light copper hooks, a light plane mirror, 1 cm. in diameter.

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[*] Poynting, Phil. Trans. 182 A, p. 572 (1891).

By means of a fixed plane mirror the light from an illuminated scale, situated at a distance of 2 metres from the balance, was reflected to the suspended mirror, from which, after again undergoing reflexion at the surface of the fixed mirror, it was reflected into a telescope situated immediately above the middle of the graduated scale.

In this way, any slight motion of the end of the pointer produced a rotation of the suspended mirror about a vertical axis, and consequently a motion of the scale across the field of the telescope.

By raising the position of the centre of gravity of the balance the sensitiveness could be increased, until, by moving a rider through 1 cm., that is, by increasing the weight on one side of the balance by 1 mgm., a deflexion of 800 mm. of the telescope scale was obtained ; in this position the balance had a period of swing of 15 seconds.

To damp the motion of the suspended mirror, and especially to get rid of the rapid oscillations due to the natural period of the suspended system, this was provided with a set of vanes immersed in a vessel of paraffin-oil. The vanes were made of aluminium and were situated below the mirror, to the back of which they were rigidly attached by means of a metal wire. To avoid any heating of the air inside the coil and consequent upward currents, which would themselves produce an apparent diminution of the weight of the rods, a metal jacket was fitted into the space inside the coil, leaving only a cylindrical space along the axis, 10 cms. long and 1 cm. in diameter, at the upper end of the coil (fig. 1, p. 52).

Through this jacket a stream of cold water was kept running with the result that, though with a current of 10 amperes the outside of the coil became quite hot, yet in the space within the water-jacket no increase of temperature could be detected. The current used was obtained from a battery of 54 storage-cells (B, fig. 1), and could be adjusted by means of an adjustable resistance R.

By means of the reversing-key K, the circuit could be broken or the current sent in either direction through the coil, the magnitude of the current being indicated by the Weston ammeter A.

The method of carrying out an experiment was, then, as follows :—

The coil was arranged in a vertical position at such a distance below the balance that no force was exerted on this when a current was passed through the coil, and the rod suspended vertically by means of a thread from one arm of the balance ; this thread passed through a hole in the case

E 2

of the balance and through the shelf on which the balance rested. The height of the coil was then adjusted so that the

Fig. 1.
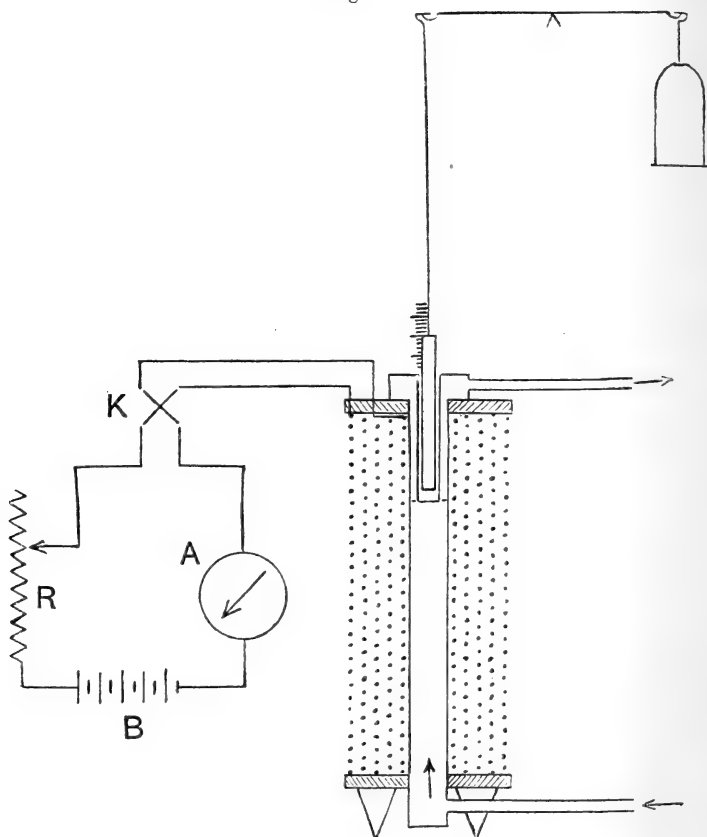
middle of the rod—all the rods were 15·6 cms. in length— occupied such a position that it rose to the level of the end of the coil when the current was put on. The observer then seated himself at the telescope and observed the reading of the scale occupying the centre of the field of the telescope.

In practice the suspended mirror was never stationary, but by observing in the telescope the scale-readings at the ends of a series of successive oscillations, the amplitude of which did not exceed 1 cm., the position of the zero was readily obtained.

The current of the required magnitude was then sent through the coil, when, owing to the mechanical force

exerted on the cylinder, the equilibrium of the balance was disturbed. In the space of one or two minutes the balance again came approximately to rest, and the new equilibrium position was observed. The circuit was then broken and a second observation taken of the zero position when no current passed in the coil. By subtracting from the second reading the mean of the first and third, the deflexion produced by the current in the coil was obtained. The current was then sent through the coil in the opposite direction and observations taken as before, and so on.

As an example of such observations, the following data were obtained with the bismuth cylinder when a current of 6 amperes was sent through the coil :—

Scale-reading when current is :—		Deflexion due to current of 6 amperes.
(A) zero.	(B) 6 amperes.	
+65		
	−59·5	120
56		
	64	118·5
53		
	68	119·5
50		
	70	119
48		

In the same way the balance was standardized from time to time by observing the deflexion of the scale seen through the telescope produced by the motion of the rider through a known length of the beam of the balance.

The force acting on the rod due to the current in the coil was then calculated on the assumption that this was proportional to the deflexion produced by it. As far as the graduations of the balance-beam would allow—that is, down to forces of $\frac{1}{5}$ mgm. weight—this assumption was verified experimentally, and as the smallest forces measured were not less than $\frac{1}{20}$ mgm. weight, there is no reason for doubting the truth of this assumption.

Experiments were performed with currents in the coil varying from 1·4 to 10·2 amperes, the sensitiveness of the balance being adjusted as far as possible so as to give a convenient deflexion for each value of the current.

Having in this way obtained the absolute value of the force acting on the rod, this quantity is substituted in Boltzmann's expression referred to above (p. 49), and the value

of k calculated.   For the force P acting on the metal cylinder situated half within and half without the coil, Boltzmann gives the value *

$$P = \frac{\pi^3 k N^2 \nu^2 C^2 \rho^2 m}{b - \beta} \left[8\epsilon - \frac{4}{3} \epsilon \frac{(l^2 + m^2)(b^3 - \beta^3)}{(l^2 - m^2)^2 (b - \beta)} - \frac{8}{3} \frac{l(b^3 - \beta^3)}{(l^2 - m^2)^2} + \rho^2 \eta \right],$$

where k is the magnetic susceptibility, $2m$ the length, and ρ the radius of the rod ; l represents the length, b the external and β the internal radius of the coil ; N is the number of turns per cm. in each layer of the coil and ν the number of layers ; C is the current passing through the coil.
    Also

$$\epsilon = \log \frac{b + \sqrt{m^2 + b^2}}{\beta + \sqrt{m^2 + \beta^2}}$$

and

$$\eta = \frac{b^3}{m^2(m^2 + b^2)^{\frac{3}{2}}} - \frac{\beta^3}{m^2(m^2 + \beta^2)^{\frac{3}{2}}}.$$

The results obtained are shown below :—

Bismuth.—Radius of rod $= \cdot 2908$ cms.

Current in coil $=$ C.	Field inside coil due to current $=$ H.	Deflexion due to current $= \delta$.	Force (in milli-grams) on cylinder due to current $=$ P.	Value of $k \times 10^6$.
amps.				
1·4	183	34·3	·057	13·8
2	261	70·6	·118	13·9
3	392	76·5	·270	14·2
4	523	133	·470	13·9
5	653	212	·750	14·1
6	784	119	1·05	13·8
7	915	162	1·43	13·8
8	1046	211	1·87	13·8
9	1176	267	2·36	13·8
10	1307	332	2·93	13·9

The mean of the calculated values of k shown in the last column is $13·9 \times 10^{-6}$.
    Using the same method, von Ettingshausen found for three samples of bismuth the values $14·0 \times 10^{-6}$, $14·5 \times 10^{-6}$, and $13·5 \times 10^{-6}$, whilst by three distinct methods the mean values he obtained were $13·6 \times 10^{-6}$, $15·3 \times 10^{-6}$, and $13·4 \times 10^{-6}$[†].
    Curie also has measured the magnetic susceptibility of

    * Boltzmann, *Wien. Ber.* lxxxiii. p. 581 (1881).
    † Von Ettingshausen, *loc. cit.* p. 272.

bismuth, and has found for his specimen the value 13.2×10^{-6} [*], whilst Wills, who employed fields ranging from 1620 to 10450 lines per sq. cm., found that k remained constant throughout this range, his mean value being 12.5×10^{-6} [†]. Hence we see that the results given in this paper agree very well with those of previous observers.

Alloy No. 1.—Containing 1·23 per cent. tin.
Radius of rod $= \cdot 2493$ cms.

C.	H.	δ.	P.	$k \times 10^6$.
1·7 amps.	222	37·5	·052	11·6
2·4	314	74	·104	11·5
3·2	418	53·8	·186	11·7
4	523	83	·288	11·5
4·8	627	119	·412	11·5
6	784	85	·644	11·5
7	915	117	·812	11·6
8	1046	152	1·15	11·5
9	1176	192·3	1·45	11·5
9·8	1281	228	1·72	11·5

It will be observed that for this alloy the value of k is constant throughout the range of field strengths, H, employed.

The mean value thus obtained is $k = 11.54 \times 10^{-6}$, which is about 17 per cent. less than that obtained for pure bismuth.

Alloy No. 2.—Containing 3·01 per cent. tin.
Radius of rod $= \cdot 2446$ cms.

C.	H.	δ.	P.	$k \times 10^6$
1·8 amps.	235	22·5	·043	8·90
2·5	327	42·7	·083	8·86
3·2	418	69·4	·136	8·86
4	523	103·8	·211	8·77
4·8	627	102·6	·302	8·71
6	784	205·0	·475	8·77
7	915	87·3	·652	8·88
8	1046	109·5	·851	8·84
9	1176	162·4	1·09	8·92
10	1307	170·4	1·32	8·81

In the case of this alloy, too, it appears that the magnetic susceptibility is independent of the magnetizing force.

* Curie, *Comptes rendus,* cxvi. p. 137 (1893).
† Wills, Phil. Mag. xlv. p. 446 (1898).

The mean value obtained for k is $8\cdot83 \times 10^{-6}$, which is about one-third less than that obtained for bismuth.

Alloy No. 3.—Containing 10·0 per cent. tin.
Radius of rod = ·2616 cms.

C.	H.	∂.	P.	$k \times 10^6$.
1·8	235	22·6	·039	6·96
2·6	339	37	·081	7·01
3·3	431	54·5	·130	6·94
4	523	89	·195	7·12
4·8	627	128	·281	7·11
5·8	758	65·7	·338	6·86
7	915	95·8	·578	6·86
8	1046	127	·768	6·96
9	1176	161·5	·972	7·00
10·2	1333	207	1·25	6·98

These experiments indicate that for this alloy also, the magnetic susceptibility is independent of the magnetizing force.

The mean value found for k is $6\cdot96 \times 10^{-6}$, which is exactly half that found for bismuth.

Alloy No. 4.—Containing 23·6 per cent. tin.
Radius of rod = ·2769 cms.

C.	H.	∂.	P.	$k \times 10^6$.
2·6	339	58	·074	5·67
3·4	444	101	·128	5·78
4·0	523	138	·176	5·70
4·5	588	173·5	·221	5·66
5·2	680	82	·300	5·71
6·5	849	128·8	·469	5·74
7·5	980	161	·624	5·77
8·3	1085	197	·765	5·76
10·2	1333	297	1·17	5·80

Here as in all other cases it will be observed that the values of k remain constant throughout the range of magnetizing forces used.

The mean value obtained for k is $5\cdot73 \times 10^{-6}$, which is about 18 per cent. less than that obtained for the preceding alloy containing 10·0 per cent. tin.

From the above experiments we are able to conclude that the addition of tin to bismuth, or to an alloy of bismuth and

tin, is always accompanied by a diminution of the diamagnetic properties of the metal.

As fig. 2, which represents the relation between magnetic susceptibility and composition, shows, this effect is most marked in cases where the proportion of tin is small, the susceptibility of an alloy containing 10 per cent. tin being only one-half that of pure bismuth.

When the proportion of tin is greater than 10 per cent., the further addition of tin is accompanied by a diminution of the magnetic susceptibility, which is proportional to the amount of tin added.

In all cases the magnetic susceptibility k is independent of the strength of the magnetic field in which the metal is placed.

Fig. 2.
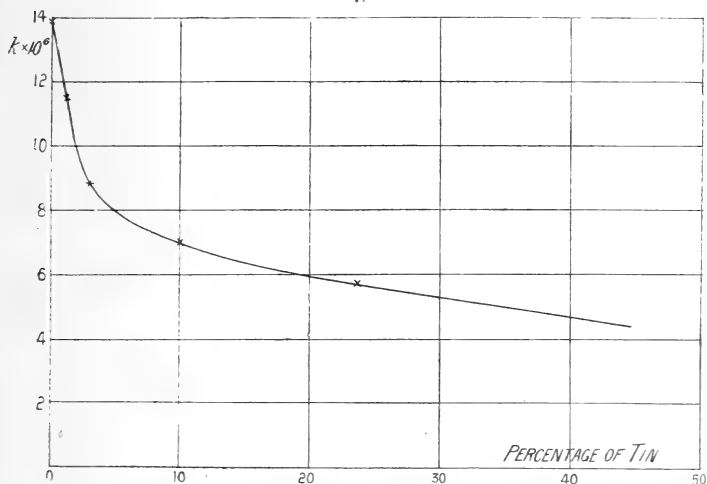
The electrical and magnetic properties of these alloys have now been extensively investigated, and it may be observed that the nature of the results obtained in these experiments is quite general. That is to say, when a very small amount of tin is added to pure bismuth a considerable change in all the physical properties is produced. On the other hand, the addition of bismuth to pure tin is always accompanied by a gradual change in the physical properties, the metal so long as it does not contain a considerable excess of bismuth behaving as though it were a simple mixture of the two constituents.

Finally, I desire to express my thanks to Prof. J. J. Thomson for his ever ready advice and suggestions.

Cavendish Laboratory,
    April 26th, 1904.

VI. *Atmospheric Friction on Even Surfaces.*
By A. F. ZAHM, *Ph.D.**

THE aim of the experiments here described is to determine the friction of free air flowing swiftly over even surfaces, and the law of its variation with the speed of flow, the length and quality of surface. The work differs from that of Maxwell and others on viscosity, in some essential features. The air flows in uniform current past a thin plane at speeds ranging up to forty feet a second. The primary purpose of the research was to establish a basis for calculations in aeronautics ; but it is hoped that the measurements are sufficiently accurate to be of interest also in the general dynamics of fluid motion.

It is well known, from the investigations of Froude and others, that the resistance of a fair-shaped vessel in water is due largely to surface friction ; and, by analogy, it was surmised that in the atmosphere a like relation should obtain. But the measurements of several prominent experimenters led them to affirm that the tangential resistance of air is negligible, even for bodies of fair outline ; while the present research tends to prove that such friction is fully as great for air as for water, in proportion to their densities. It therefore seems important that the main laws of this resistance should be carefully determined.

To measure the tangential force of the air on even surfaces, various skin-friction planes were suspended inside a wind tunnel, by means of two fine steel wires attached to the top

Fig. 1.—Friction Plane suspended in Wind-tunnel.
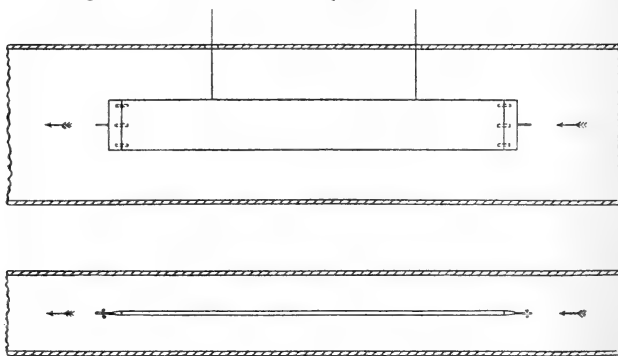
of the laboratory, as shown in fig. 1. The tunnel itself, standing on the floor of the laboratory, measures 40 feet

long by 6 feet square, and has a five-foot suction-fan at one end, a cheese-cloth screen, or two, at the other end, to straighten the inflowing air. A boy, with a rheostat and tachometer, holds the fan at any desired speed, accurately to a fraction of one per cent., thus giving an even flow of air of like constancy. As the wind-friction moves the plane endwise, the displacement is determined by the motion of a sharp pointer attached to one suspension-wire, and travelling over a fine scale lying on top of the tunnel. The swing of the plane can be measured accurately to five thousandths of an inch, and the force on the plane is exactly proportional to the scale-readings. The wind-speed is usually measured by a carefully standardized * pressure-tube anemometer, though other kinds have been employed for comparison. The tunnel is sometimes narrowed, as shown in fig. 1, to increase the speed.

The planes employed were similar to those commonly used to determine the skin-friction of water. The first was a pine board 4 feet long, $25\frac{1}{2}$ inches wide, and 1 inch thick, carefully trued and varnished, and suspended in the wind tunnel by steel wires twenty-five thousandths of an inch thick. It was provided, fore and aft, with seven-inch pine ends whose " lines " were circular arcs tangent to the board, the ends being held on by dowel pins. Each end terminated in a sharp edge from the centre of which a steel pin protruded along stream between guides, to steady the plane against wabbling. As the doweling was carefully executed, straight planes of any length could be made by adding extra boards ; the lengths most employed being 2, 4, 8, 12, 16 feet.

The method of using the planes to determine the surface-friction was as follows :—The total force was measured at various velocities, with the ends first on the sixteen-foot board, then successively on the other lengths ; and finally with nothing between them. Subtracting this last force from each of the others gave the friction on each of those five lengths. It may not be absolutely true that the end resistance was the same for each of those lengths ; but the error of this assumption is regarded as very slight for several reasons : (1) the end resistance is but a small part of the total ; (2) the stream-lines are so slightly disturbed that the flow about the ends must be practically the same in all cases ; (3) the results harmonize very well with those obtained by other methods.

* " Measurement of Air Velocity and Pressure." Physical Review, December 1903.

Table I., taken from the laboratory note-book, exhibits the observed and computed values for the two-foot friction board. The mouth of the tunnel was screened with cheese-cloth to steady the flow of air, in order to obviate wabbling in so small a board. The velocity was thus reduced, it is true, but sufficient values are given to make a reliable diagram. With the sixteen-foot plane speeds up to forty feet a second were employed, with like results.

The following data may be prefaced :—surface of the two-foot plane without end-pieces, 8·83 square feet ; weight of plane and ends 17 pounds ; one inch swing of plane =0·0862 pounds wind-force; barometric pressure 29·80 inches ; mean temperature of experiment 24°·2 C.

TABLE I.

Surface Friction by 24 in. × 25·5 in. Pine Board with sharp ends.

Speed of Fan.	Swing of Plane.	Force causing Swing.	Force on Ends.	Net Friction.	Pressure-tube Anem.	Wind Speed.	Friction per Sq. Ft.
rev. min.	in.	lbs.	lbs.	lbs.	gm. sq. cm.	ft. sec.	lbs.
200	0·050	0·00431	0·00233	0·00198	22·8	6·38	0·000224
250	0·080	0·00690	0·00365	0·00325	37·0	8·12	0·000368
300	0·120	0·01034	0·00553	0·00481	58·0	10·18	0·000545
350	0·158	0·01362	0·00734	0·00628	78·5	11·80	0·000710
400	0·205	0·01767	0·00949	0·00818	103·5	13·52	0·000925
450	0·260	0·02240	0·01188	0·01052	132·0	15·34	0·001188
500	0·310	0·02586	0·01379	0·01207	155·0	16·61	0·001366

The third column gives the whole force on the friction-board with its ends and suspension-wires ; the fourth column gives the force on the ends and wires alone, which, deducted from the whole force, gives the friction on the sides of the two-foot length. Dividing this net friction by 8·83, the area of the true friction surface, gives the values in the last column.

Similar tables were obtained for the other friction boards, of lengths 4, 8, 12, 16 feet respectively. When the values from the five tables are plotted on logarithmic cross-section paper, they give five separate straight lines all having the same inclination as the one shown in fig. 2 (plotted from Table I.), in which the slope tangent is 1·85. This means that, for all the velocities and lengths of surface employed in this research, the skin-friction is expressed by an equation of the form :

$$F = av^{1·85}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\alpha)$$

a being a numerical constant, v the wind speed. Hence if

the net friction on each board is known for any one velocity, it can readily be computed for any other velocity.

Fig. 2.—Relation between Wind-speed and Friction.

In practice the computations illustrated in Table I. were obviated, for all the tables, by a simple expedient. The observed anemometer-readings and swing of the plane were plotted while the measurements were in progress, giving five straight lines all of the same slope. Then a point was chosen on each line representing a wind-speed of 10 feet per second, and the corresponding friction per square foot of surface noted. From these values the numerical equations between F and *v* can at once be written. The observed values are given in the subjoined table :—

TABLE II.

Skin-friction at 10 ft. per sec. for various Lengths of Surface.

Length of Friction Board.	2.	4.	8.	12.	16.
Average Friction, lbs. per sq. ft.	0·000524	0·000500	0·000475	0·000467	0·000457

Knowing, then, the friction, at the same speed, on planes of five different lengths, there remained to determine the

law of its variation with length of surface. To that end the values in Table II. were plotted on logarithmic cross-section paper, as shown in fig. 3. The diagram is a straight line whose equation is :

$$f = 0.000551 \, l^{-0.07},$$

in which f is the average friction in pounds per square foot

Fig. 3.—Relation between Length of Board and Average Friction.
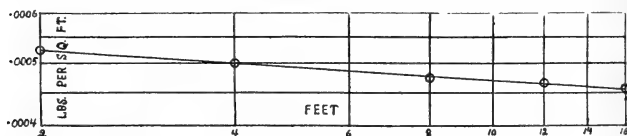
at a speed of 10 feet per second, and l is the length of surface in feet. At one foot per second, therefore, the coefficient is $0.000551 \div 10^{1.85} = 0.00000778$. Hence at any speed, v feet per second, the average friction per square foot is :

$$f = 0.00000778 \, l^{-0.07} v^{1.85}. \quad \ldots \quad (\beta)$$

Assuming the two laws thus far derived to be true for the planes and wind-speeds employed, we may readily express the total friction on a plane of any length, from 2 to 16 feet, moving at any speed from 5 to 40 feet per second. Thus, by equation (β) the total friction F, on a surface one foot wide and l feet long is :—

$$F = fl = 0.00000778 \, l^{0.93} v^{1.85}, \quad (v = \text{ft./sec.}).$$

Of course this value of F must be doubled for a material plane of length l, since a material plane has two sides.

It may now be inquired what other circumstances alter the surface-friction. Perhaps the chief of these are the atmospheric changes of density and the unevenness of surface.

No effort was made to determine the relation between the density and skin-friction of the air, partly for want of time, partly because too great changes of density would be needed, with the apparatus in hand, to reveal such relation accurately. Doubtless the friction increases with the density, since it is due to the inertia of the fluid near the friction surface. Of course in steady motion at low velocity, such as studied by Maxwell, the conditions are different. He found that when one plane moves edgewise near and parallel to another plane, at a constant speed below one twelfth of an inch per second, the friction is independent of the pressure, and proportional to the absolute temperature, for such atmospheric conditions as prevail near the earth's surface.

Some measurements were made with the four-foot friction-board covered with various materials, to observe the effect of quality of surface upon the tangential resistance. Practically the same friction was observed whether the board was covered with dry varnish or wet sticky varnish, or sprinkled with water, or covered with calendered or uncalendered paper, or glazed cambric, or sheet zinc, or Old English draughting paper, which feels rough to the touch. But when the plane was covered with coarse buckram, having sixteen meshes to the inch, the friction, at 10 feet a second, was ten to fifteen per cent. greater than for the uncovered surface; and the friction increased as the velocity raised to the power 2·05, or approximately as the square of the speed.

The fact that such a variety of materials exhibit practically the same friction seems to indicate that this is a shearing force between the swiftly gliding air and the comparatively stationary film adhering to the surface, or embedded in its pores. If, as seems to be true, there is much slipping, this means that the internal resistance of the air is less at the surface than at a sensible distance away. As the shearing strength of a gas is due to the interlacing of its molecules, owing to their rapid motion normal to the shearing plane, it may be that the diminution of shearing resistance near a boundary surface is due to the dampening, within the film, of the component of molecular translation normal to the surface.

To summarize the results attained thus far it may be said that, within the ascribed limits of size and wind-speed:

(1) The total resistance of all bodies of fixed shape and aspect is expressed by an equation of the form

$$R = a v^n, \qquad (\alpha)$$

R being the resistance, v the wind-speed, a, n numerical constants.

(2) For smooth planes of constant length and variable speed the tangential resistance may be written

$$R = a v^{1·95}. \qquad (\beta)$$

(3) For smooth planes of variable length l and constant width and speed the friction is

$$R = a l^{·93}. \qquad (\gamma)$$

(4) All even surfaces have approximately the same coefficient of skin-friction.

(5) Uneven surfaces have a greater coefficient of skin-friction, and the resistance increases approximately as the square of the velocity.

The equation $R = av^n$ was found to express very accurately the resistance of all shapes tested, at speeds from 5 to 40 feet a second. For normal planes, spheres, cylinders, and blunt bodies generally, except very small ones, n equals 2, very approximately; for thin, tapering bodies n may have any value from 2 to 1·85. But in every case, if the form and aspect of the model remain fixed, a and n are found to remain invariable for all the speeds employed. This was manifested by plotting the speed and resistance on logarithmic cross-section paper, and observing that the diagram was invariably a straight line for all the models tested.

These conclusions, though apparently valid for the speeds employed, which ranged from 5 to 40 feet a second, may not all be extended to considerably higher velocities. The well-known formula for blunt bodies, $R = av^2$, fails when employed for a wide range of speeds, as shown by Duchemin[*] and by the present writer [†]; the formula $R = av^2 + bv^3$ being much more accurate. So, likewise, the equation for friction may have its limitations. Even at the ordinary speeds of transportation, at which the Newtonian formula obtains for blunt bodies, not too small, it fails for sharp ones, owing to the element of friction. It seems, therefore, that for such speeds the resistance might better be expressed as the sum of two terms, one giving the head resistance the other the component of surface-friction. Thus

$$R = av^2 + bv^{1 \cdot 85}.$$

This formula has, indeed, been applied satisfactorily to a number of easy shapes, the computations agreeing very closely with the data of observation.

Such were the results obtained in a wind of uniform velocity and direction. When, however, the current is turbulent, a and n are found to vary considerably. But, since the flow of a turbulent wind cannot be specified, the measurements made in one such current cannot well be applied to determine the resistance of a different one. For that reason it seemed better to make the measurements in a uniform wind, where, moreover, the instruments give steadier readings.

On comparing the above results with those obtained by Froude for water, it is found that the equations are very similar. The exponents are nearly the same, and the coefficients are to each other roughly as the densities of air and water. Using varnished friction-boards, Froude finds $n = 1 \cdot 85$

    [*] " Les Lois de la Resistance de l'Air sur les Projectiles."
    [†] " On the Resistance of the Air at Speeds below One Thousand Feet a Second," Phil. Mag. May 1901.

for a surface 8 to 20 feet long, and $n = 2 \cdot 00$ for a plane 2 feet long; I find $n = 1 \cdot 85$ for all lengths from 2 to 16 feet. By Froude's measurements the friction varies as the power $0 \cdot 83$ of the length for varnished planes 2 to 20 feet long; I find it to vary as the power $0 \cdot 93$. With a varnished board 2 feet long, moving 10 feet a second, the ratio of our coefficients of friction for air and water is $1 \cdot 08$ times the ratio of the densities of those media under the conditions of experiment.

But in some respects Froude's results are quite unlike mine. For several surfaces he finds the skin-friction to vary as the square of the velocity, or nearly so, which is the relation I observed in a turbulent current, and when the friction-board was vibrating slightly. He finds the friction of calico about twice that of a varnished surface; I find that glazed cambric has about the same friction as a varnished surface. But if the cambric is roughened so as to expose a fine down, the friction is very much increased.

The fact that, for some surfaces, the coefficients of friction for air and water are roughly as their densities is of considerable interest. It is well known that the head resistances of the two fluids are directly as their densities; and if their friction coefficients also bear that ratio, the total resistances must be approximately as their densities. Hence the data obtained from water-resistance measurements on such surfaces may be fairly well applied to estimate the air resistance on various-shaped models.

It is not, however, self-evident that the surface-friction of any two fluids is proportional to their densities, and should not be taken for granted. It happens to be roughly true for varnished wooden surfaces in air and water; but is wholly untrue for calico surfaces. In default, therefore, of an adequate physical theory of surface-friction, the magnitude in any given case can be determined only by direct experiment.

To complete the theory of the skin-friction board two steps further remain to be taken. First, the equations of motion for a viscous fluid must be integrated, to find the velocity at all points in the disturbed region about a thin material plane. Then the speed of flow must be measured at all points next the plane and at some distance away. The writer expects soon to map the stream-lines and measure the velocity. If, then, the equations can be integrated, so as to give the speed as a function of the space coordinates, the computed and observed values can be directly compared. It is hoped that some one may obtain sufficiently general solutions of the equations to be of practical value; particularly for the simpler

case in which the plane is indefinitely wide, and in which the edge conditions are negligible. The integrals, if sufficiently general, will be of great importance to the science of surface-friction, and may at once be applied to the mass of accurate data that, for a generation, has been accumulating in the laboratories of the marine engineers.

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Fluid Friction on Even Surfaces.
Note by Lord RAYLEIGH.

IN connexion with such experiments as those of Froude and Zahm respectively on flat surfaces moving tangentially through water or air, it is of interest to inquire how much can be inferred from the principle of dynamical similarity. Dynamical similarity includes, of course, geometrical similarity, so that in any comparisons the surfaces must be similar, not only in respect of their boundaries, but also in respect of their roughnesses, at any rate until it is proved that the restriction may be dispensed with. Full geometrical similarity being presupposed, the tangential force per unit area, F, may be regarded as a function of a the linear dimension of the solid, ρ the density of the fluid, v the velocity, and ν the kinematic coefficient of viscosity. It is assumed that the compressibility of the fluid does not come into account. As in a similar problem relating to the flow of fluid along pipes [*], the method of dimensions shows that, if it be a function of the above quantities only,

$$F = \rho v^2 . f(av/\nu), \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where f denotes an arbitrary function, whose form must be obtained, if at all, from other considerations. If F be independent of ν, f is constant, and F is proportional to the density and the simple square of the velocity. Conversely, if F be not proportional to v^2, f is not constant, and the viscosity and the linear dimension enter.

If the general formula be admitted, several conclusions of importance can be drawn, even though the form of f be entirely unknown. For example, in the case of a given fluid (ρ and ν constant), F is strictly proportional to v^2, provided a be taken inversely as v. Again, if the fluid be varied, we may make comparisons relating to the same surface (a constant). For if v be taken proportional to ν, f remains unaffected; so that F is proportional to ρv^2 simply. For air the kinematic viscosity is about 10 times greater than for

* Phil. Mag. xxxiv. p. 59 (1892) ; Scientific Papers, iii. p. 575.

water, so that to obtain comparable cases the velocity through air would need to be 10 times greater than through water, a remaining unchanged. This condition being satisfied, the frictions per unit area would be as ρv^2, or since water is about 1000 times as dense as air, the actual friction would be about 10 times greater for the water than for the air.

According to Dr. Zahm's experiments, where the velocity alone is varied, the form of f is within certain limits determined to be

$$f(x) = A x^{-15}.$$

Within the same limits (1) gives as the complete expression for F,

$$F = A \rho v^{1\cdot 85} (v/a)^{\cdot 15}. \quad . \quad . \quad . \quad . \quad (2)$$

May, 1904.

VII. *The Penetrating Radium Rays.* By J. A. McClelland, *M.A., Professor of Experimental Physics, University College, Dublin* [*].

THERE has been some difficulty in deciding as to the nature of the very penetrating or γ rays given by radium; whether they are electromagnetic pulses like Röntgen rays, or a flight of charged particles like the α and β rays. In some respects the γ rays act like the α and β rays, which we know to consist of charged particles; while, on the other hand, no deflexion of the rays in a magnetic field has been detected; and, further, we should expect to get Röntgen rays from a body like radium which is emitting charged particles travelling with a great velocity.

The chief reason for considering the γ rays to consist of charged particles, lies in the fact that the absorption of these rays by different substances is approximately proportional to the density of the substance; a law obeyed by α and β rays and by cathode rays, but not obeyed by Röntgen rays. It should be stated here that since the experiments described in this paper were commenced, Mr. Eve [†] has found that when only very penetrating Röntgen rays are used, the absorption produced by different substances becomes more nearly proportional to the density of the substance, a result which tends to remove the chief difficulty in deciding that the γ rays are of the same nature as Röntgen rays.

The method I have used to decide whether or not the γ

rays are charged particles is to endeavour to detect directly the charge carried by the rays, if any.

First Experiment.

A block of lead was taken and a hole drilled to a small depth in the centre of one of its faces; 50 milligrams of a good sample of radium bromide inclosed in an air-tight vessel with a very thin mica top was placed in the hole. The rays from the radium could thus escape upwards from the hole, the block being so large that no α and β rays and very few γ rays could escape in any other direction. This lead block was carefully insulated on paraffin, screened by an earthed conductor from outside electrical forces, and joined to a sensitive Dolezalek electrometer. The electrometer was first connected to earth, then insulated, and the charge it gradually got was observed. Observations were made (1) when the hole in the lead block was covered with only a very thin sheet of foil, so that α, β, and γ rays could escape; (2) when the hole was covered with such a thickness of foil that only β and γ rays could escape; (3) when the hole was so covered that only γ rays escaped.

It is obvious that care must be taken, in screening the block of lead with an earthed conductor, to guard against effects due to contact electric forces. The air between the lead block and the surrounding screen being ionized, a current will exist between the block and the screen if they are not of the same metal; and this will produce a deflexion of the electrometer when insulated. This was avoided by wrapping the block and the wire leading to it with tinfoil, and making the surrounding screen also of tinfoil.

When the effects of contact electric forces are eliminated, the effects observed are as follows:—

(1) When α, β, and γ rays are allowed to escape, the lead block gets a negative charge.

(2) When β and γ rays escape, the block gets a positive charge.

(3) When only γ rays escape, the block gets a negative charge.

The results in cases (1) and (2) are what we should expect. The α rays carry away positive electricity, and the β rays carry away negative electricity. The β rays are only produced in the final stages of disintegration, so that we should expect the negative electricity carried away by the β rays to be less than the positive carried away by the α rays. We thus get a negative charge on the block when both α and β

rays escape, but a positive charge when only β rays escape. The result obtained when only γ rays escape appeared at first sight to show that these rays carried a positive charge. This, however, is not necessarily so. It was found that when the radium was taken out of the block and placed outside the screen, so that the air surrounding the block was still ionized, the lead block got a negative charge. This is due to the known result that a conductor exposed to ionized air gets a small negative charge owing to the fact that the negative carriers travel with a slightly greater velocity than the positive carriers under the same electric force. The ionized air immediately surrounding the lead block gives up to the block more negative than positive carriers, the block thus getting a negative charge and the surrounding gas retaining an excess of positive.

It was easy to show that all the negative charge observed when only γ rays escaped could be accounted for in this way. The experiment was therefore somewhat inconclusive in its present form as regards the γ rays ; it is described chiefly because it affords a simple method of demonstrating the charge carried by the α and β rays.

One precaution is necessary in showing the charge on the α rays in this way. These rays are very easily absorbed by the air, and thus at a small distance above the block there will be an excess of positive electricity producing an electric field tending to drive positive ions to the lead block. This can be got over by placing an earthed sheet of foil a short distance above the block. When this is done and the effects of contact electric force eliminated, the successive changes of sign of the charges got by the block containing the radium occur as described above. The charge in each case of course reaches a maximum when the rate of gain of charge is equal to the rate of loss through the surrounding conducting air.

It is scarcely necessary to give any numbers as they are not required for any calculations in this paper. In showing the negative charge carried by the β rays, initial deflexions of 100 scale-divisions in 10 seconds could easily be obtained with a Dolezalek electrometer. For the α rays it was necessary, for the reason given above, to place an earthed plate at a short distance above the block containing the radium. The conducting gas between the plate and the block prevented the block from charging rapidly, but an initial negative deflexion of 50 divisions in 15 seconds could be obtained. The numbers are merely given to show how this simple arrangement demonstrates the charge on the α and β rays.

Second Experiment.

The experiment described above is not conclusive as to whether the γ rays carry an electric charge because of the effect due to the surrounding ionized gas. This effect could of course be largely eliminated by carrying out the experiment in a high vacuum. It was more convenient, however, to modify the apparatus in another way (fig. 1).

Fig. 1.
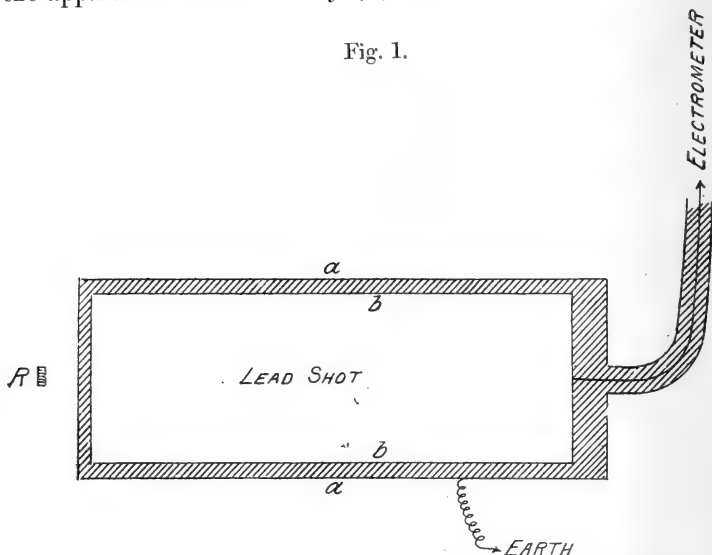
A tin cylinder *b*, 20 cms. long and 8 cms. diameter, was placed inside a second tin cylinder *a*, 25·5 cms. long and 10 cms. diameter, and the annular space between flooded with liquid paraffin, which solidified, insulating the inner cylinder from the outer, which was to earth. The inner cylinder was filled with shot so that it absorbed a pencil of γ rays passed along it. The 50 milligrams of radium bromide producing the γ rays were contained in a small vessel R placed in a hole in a block of lead, so as to limit its radiation to a diverging pencil with its axis parallel to the axis of the shot-cylinder, and thus prevent the rays from falling on other parts of the apparatus.

A wire from the inner cylinder leads to the electrometer placed some yards away, the wire being led along the axis of an earthed metal tube filled with paraffin. The inner cylinder and the wire leading from it were thus not exposed to air ionized by the radium.

The vessel *a* was thick enough to stop all the α rays ; the

β rays could be stopped when desired by placing a sheet of lead between the radium and the vessel a.

In this way the charge got by the shot-cylinder could be measured when it is absorbing both β and γ rays, and also when absorbing γ rays only.

When the shot-cylinder absorbs both β and γ rays, it rapidly gets a negative charge; when it absorbs only γ rays no charge could be detected.

This experiment shows that the γ rays do not carry any charge sufficient to be detected by the above apparatus.

The sensitiveness of the apparatus, and therefore the value of the experiment, can best be estimated by considering the deflexion obtained with the β rays. A pencil of β rays diverges from R wide enough to include practically the whole end of the cylinder b; this pencil produces a deflexion of 38 scale-divisions per minute when absorbed by the shot-cylinder. The whole charge carried by the β rays contained in this pencil does not reach the shot-cylinder, part being absorbed by the cylinder a and by the mica vessel containing the radium. The tin vessel a was ·3 mm. thick, and this thickness of tin (neglecting the paraffin and mica) would absorb $\frac{1}{5}$ of the β rays (Strutt, 'Nature,' p. 539, 1900). The β rays contained in the pencil considered therefore carry a negative charge corresponding to more than 200 scale-divisions per minute.

The same pencil of γ rays is experimented with and gives no deflexion, certainly not as much as 2 divisions per minute ; this pencil is not all absorbed by the shot because of the great penetrating power of these rays. We know the coefficient of absorption of lead for these rays (fig. 4), and it is easy to calculate from the dimensions of the apparatus given that more than $\frac{1}{3}$ of the γ rays included in the pencil considered would be absorbed by the cylinder of shot ; and we have seen that the deflexion produced is not more than 2 divisions per minute. We have therefore the result that the β rays in a given pencil carry a negative charge corresponding to more than 200 divisions per minute, while the γ rays contained in the same pencil do not carry a charge corresponding on the same scale to more than 5 divisions positive or negative per minute.

The absorption of β rays by the mica vessel and the insulating paraffin has been neglected in this estimate, and this absorption would be considerable ; so that the electricity, if any, carried by the γ rays coming from a sample of radium is certainly not more than 1 or 2 per cent. of the negative electricity carried by the β rays from the same radium.

The γ rays might of course consist of a stream of uncharged particles ; but as the forces causing disintegration are probably of an electrical nature, this is not likely.

Absorption of γ Rays by Different Substances.

Rutherford has measured the absorption of γ rays by different substances, and showed that the absorption is roughly proportional to the density of the substance. The absorption, however, increases more rapidly than the density, so that the ratio of the coefficient of absorption to the density is greater for the denser substances, being for lead about twice what it is for water.

On account of the importance of this law of absorption, I have made some further experiments, which show that when the rays have passed through some thickness of the denser substances, the coefficient of absorption is diminished, so that the law that the coefficient of absorption is proportional to the density is then followed with remarkable closeness.

Apparatus used.

The brass tube *a* (fig. 2) is insulated and fitted with a paraffin cork *c* through which passes the brass rod *b* joined by a wire to an electrometer. This wire is led along an earthed metal tube filled with paraffin. The other end of the tube *a* is closed with a very thin metal cap.

Fig. 2.
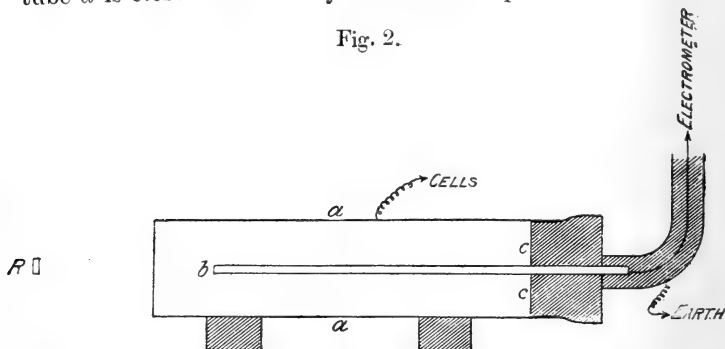
The radium is contained in a small vessel R, which is placed in a block of lead so that a diverging pencil of rays travels along the tube *a*, ionizing the air between *a* and *b*.

The tube *a* is kept at a high potential by joining it to a battery of small storage-cells. The current through the ionized gas is measured by the electrometer, the tube *a* being kept at a sufficient potential to produce the saturation-current.

Sufficient thickness of lead is placed permanently between R and the tube *a* to absorb all the β rays, so that the ionization observed is due entirely to γ rays, and measures the intensity of these rays. The substance to be examined is placed in successive layers in front of the tube *a*. In this way, curves are drawn showing the intensity of the γ rays, after passing through various thicknesses of different substances.

50 milligrams of pure radium bromide were used, placed 10 cms. from the testing-tube *a*.

Observations.

In the first place, the thickness of lead required to stop all β rays was determined by observing the ionization produced in the tube *a*, with nothing between the radium vessel R and the tube, and then putting between successive layers of lead; the vessel R itself stops most of the α rays. Fig. 3 shows that all the β rays are stopped by 4 mms. of lead. In

Fig. 3.
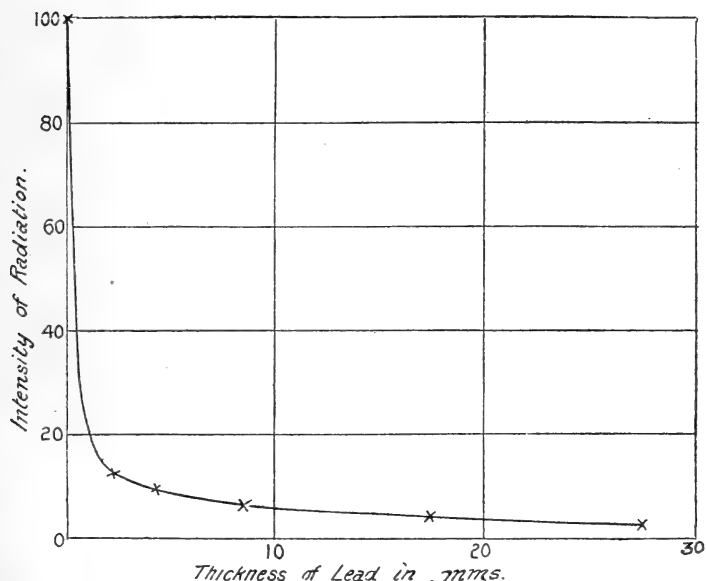
all the following experiments a thickness of 8 mms. of lead was kept in position between the radium and the testing-tube, a thickness which ensured that all β rays were stopped.

Fig. 4 shows the result of experiments with a number of substances, the numbers along the horizontal axis expressing the thickness of the substance traversed in millims., and the numbers along the vertical axis denoting the corresponding intensity of the γ rays in arbitrary units.

Fig. 4.
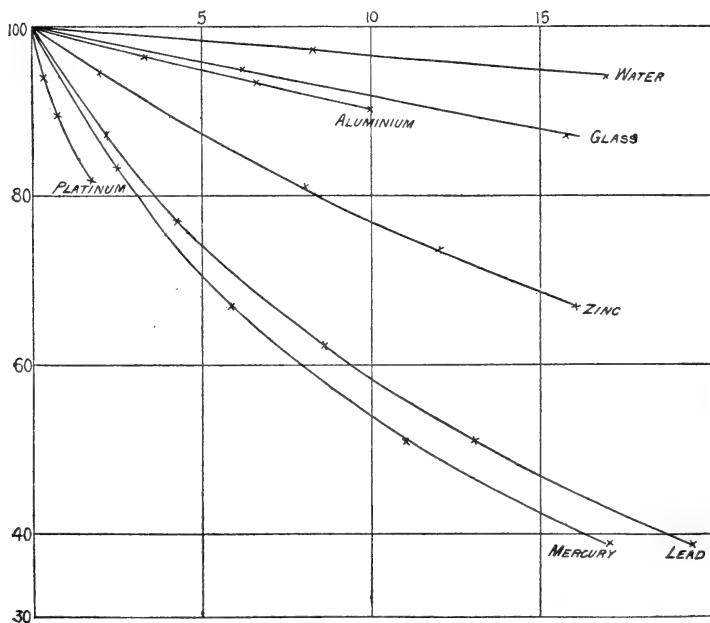
The form of the curves is not accurately given by the equation

$$I = I_0 \epsilon^{-\lambda x},$$

I being the intensity of the rays after passing through thickness x, I_0 the initial intensity, and λ the coefficient of absorption. To represent the curves by an equation of this type we must suppose λ to diminish with the thickness of substance traversed. These remarks apply chiefly to the curves for zinc, lead, mercury, and platinum. The absorption in the case of water, glass, and aluminium is so small, that sufficient accuracy to show any change in the coefficient of absorption along the curves could not be claimed.

The coefficient of absorption has been calculated for each substance :—(i.) for the first 2·5 mms. of substance traversed; (ii.) for the thickness 2·5 to 5 mms.; (iii.) for the part 5 to 10 mms.; (iv.) 10 to 15 mms. The results are shown in Table A.

TABLE A.

	I.	II.	III.	IV.
Platinum	1·167			
Mercury............	·726	·661	·538	·493
Lead	·641	·563	·480	·440
Zinc	·282	·266	·248	·236
Aluminium	·104	·104	·104	
Glass	·087	·087	·087	·087
Water	·034	·034	·034	·034

In this table, for the reason given above, no attempt is made to detect any change in the coefficient for water, glass, and aluminium. It will be observed that for the heavier substances there is a rapid decrease of the coefficient of absorption as greater distances of the substance are traversed by the rays; no such marked change takes place for the lighter substances.

This points to the conclusion that the γ rays are somewhat heterogeneous, and that the difference between the coefficients of absorption for the more penetrating and the less penetrating parts is more marked the greater the density of the substance used to absorb the rays.

Table B gives the result of dividing the coefficient of

TABLE B.

	I.	II.	III.	IV.
Platinum	·054			
Mercury............	·053	·048	·039	·036
Lead	·056	·049	·042	·037
Zinc	·039	·037	·034	·033
Aluminium	·038	·038	·038	
Glass	·034	·034	·034	·034
Water	·034	·034	·034	·034

absorption by the density of the substance. It will be seen than in column I. the numbers vary considerably, the law that the coefficient of absorption is proportional to the density not being followed at all closely. The agreement is, however, much closer in succeeding columns until in column IV. it is very close indeed. For the less penetrating radiation the absorption increases more rapidly than the density; but this

divergence from the law disappears in the case of more
penetrating radiation.

This is similar to what Eve ('Nature,' March 10th, 1904)
has found for Röntgen rays. The ratio of the coefficient of
absorption to the density varies between very wide limits
when rays from a moderately weak tube are used, the ratio
being greater for the denser substances. Eve finds that for
gases, at any rate, this ratio is more nearly a constant when
more penetrating Röntgen rays are used.

Rutherford has determined the coefficient of absorption of
γ rays for various substances ('Nature,' p. 318, 1902). His
numbers are given for comparison for substances which occur
in the above table :—

Substance.	λ.	$\dfrac{\lambda}{\text{Density}}$
Water	·033	·033
Glass	·086	·035
Zinc	·280	·036
Lead	·77	·068
Mercury.................	·92	·068

The numbers for water and glass are almost identical with
those in the above tables ; for lead and mercury they are
greater even than those in column **I.**

As mentioned at the beginning of the paper, the fact that
the absorption of γ rays is proportional to the density of the
absorbing substance was in favour of these rays being charged
particles, like other radiation which follows approximately
the same law. We cannot, however, base much on this, as
according to Eve's work the same law probably holds for
penetrating Röntgen rays ; and apart from their absorption,
the weight of evidence is certainly in favour of the γ rays
being similar to Röntgen rays.

We have rather to consider why charged particles travelling
with great velocity and Röntgen rays should follow the same
law of absorption. Prof. Thomson ('Conduction of Elec-
tricity through Gases,' p. 312) has pointed out the significance
of this law of absorption in the case of charged particles. If
the particle be small compared with the molecule of the
absorbing substance, we must consider collisions as taking
place not with the molecule as a whole, but with the con-
stituents of which it is composed. If, then, all substances are
built up of the same primordial atoms, the absorption by
different substances should be proportional to the number of

such primordial atoms per unit volume, or simply proportional to the density. Thomson also shows that the greater the velocity of the particle—the more penetrating the rays—the more closely will the absorption-density law be obeyed.

It is not at first sight so clear why such a law of absorption should hold for Röntgen rays. If we regard the front of a Röntgen pulse (or a light-wave) as uniform and free from structure of any sort, it would seem difficult to explain such a law. If, however, we regard a Röntgen pulse as possessing a structure (Thomson, 'Electricity and Matter,' p. 63), so that the front of a pulse is not uniform, but that at some places the forces are great while larger regions are free from force, we can at once get an explanation of our results.

Such a conception has been theoretically deduced by Thomson, and used to explain why when Röntgen rays pass through a gas some molecules are ionized while far more are unaffected.

Applying the idea to the case we are considering, we see that if the regions of great force in the pulse are sufficiently small, we must consider their collision with the constituents of the molecule rather than with the molecule as a whole. If in addition the pulse is sufficiently thin—that is sufficiently penetrating—we should deduce the same law of absorption for the Röntgen pulse as for a charged particle travelling with great velocity. That a similar result does not hold for ordinary light-waves is because this last condition is not satisfied.

Note added during Publication.—Two papers have recently appeared by Paschen (*Annalen der Physik*, Nos. 6 and 7, 1904), in which the author claims to show that the γ rays carry a negative charge. In the first paper the radium is surrounded with lead 1·9 cms. thick and carefully insulated in a vacuum, when the lead gets a positive charge. It should be pointed out that under the action of Röntgen rays (according to Curie and Sagnac, Dorn, and Thomson) a metal gives out negatively charged corpuscles, thus itself getting a positive charge. The effect observed by Paschen is thus quite in agreement with the γ rays being of the nature of Röntgen rays.

In the second paper the author claims to show the existence of negatively charged radium rays which are only slightly deflectable in a magnetic field, but this paper gives no direct proof of the γ rays carrying a negative charge.

VIII. *On a Possible Variation of the Solar Radiation, and its probable Effect on Terrestrial Temperatures.* *By* S. P. LANGLEY*.

[Plates I. & II.]

Introduction.

THE purpose of the present communication is primarily to discuss the validity of a surmise we may entertain, founded on observations here, as to certain possible changes in the solar constant. With this there is discussed a possible falling off of solar radiation about the close of March 1903, in connexion with certain recent values of solar radiation computed from observations here, with a comparison of actually observed temperatures for eighty-nine stations of the North Temperate Zone.

I.

Methods of Observation, and Considerations governing the Admission or Rejection of Experimental Data.

The determination of the solar radiation toward the earth, as it might be measured outside the earth's atmosphere, (called the " solar constant ") would be a comparatively easy task, were it not for the almost insuperable difficulties introduced by the actual existence of such an atmosphere, above which we cannot rise, though we may attempt to calculate what would be the result if we could. The determinations of the so-called solar constant, then, chiefly depend on various methods of eliminating the effects of the terrestrial atmospheric absorption, since only after the actual interposition of this we receive the sun's heat.

These determinations depend principally on two classes of observations :—

First, on getting as far above this atmosphere as possible, by the actual ascent of a lofty mountain, and by the observation of the solar radiation there with a subsequent comparison with that nearer its base This method possesses great advantages over any other, but presents obvious difficulties in the execution †.

The second principal class of observations, and the only

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* Communicated by the Author.

Reference may be made to the writer's paper in the Astrophysical Journal for March 1903, and to Mr. C. G. Abbot's paper in " Smithsonian Miscellaneous Collections " (Quarterly Issue), vol. xlv. pp. 74–83 (1903).

† For an example of it the reader is referred to the account of the writer's expedition to Mt. Whitney, Professional Papers, U.S. Signal Service, No. xv.

one considered here, is restricted to a single station, which we must usually take in a low altitude like that at Washington. It consists in making measurements :—(1) By the actinometer of the total radiation observed ; (2) Of the intensity of homogeneous rays in different parts of the solar spectrum for different altitudes of the sun.   (3) From these direct observations we can in theory calculate the total absorption of the atmosphere, from the evidence gathered by the spectrobolometer as to that of the several rays for different altitudes of the sun, and for different thicknesses of air.

As our atmosphere and its absorption is in a condition of incessant fluctuation, it will always be hard to discriminate between changes due to it, and to those, if any, due to an actual change in the radiation of the sun itself, and this discrimination constitutes the particular difficulty of the present research.

The present discussion will lead us to estimate the validity of any surmise as to the actual changes in the solar constant, founded on such observations as those of the second class.

As for the accuracy of the actinometer measures of total solar radiation at the earth's surface, it is easy to obtain measures relatively exact, within about two per cent., or, perhaps, even less.   But the absolute standardization of the instrument offers uncertainty of quite another order, so that I do not think that the absolute magnitudes of the results to be presented here are necessarily within 20 per cent. of the truth, or even more.

The homogeneous rays are observed here by the bolometer, and the bolographic curves from which the atmospheric extinction of radiation is inferred, traced by the movement of the spot of light upon the galvanometer-scale, are now very much more satisfactory than formerly.   They represent an immense gain over the conditions operating when I began the work at Allegheny.   The light spot should move only by an impulse from the sun ; but owing to extraneous causes, it was at first frequently impossible to keep it upon the scale of the galvanometer during so short a time as a single minute. The apparatus now, however, operates so well that such drift and tremor is relatively unknown, and the zero of the galvanometer is found almost unchanged for weeks together, a gain due to many causes and successive improvements during many years, from my own and other hands, but to which none have contributed more than Mr. Abbot of this Observatory. Measurements and reduction of a series of from five to ten bolographs of a single day requires, however, so much labour,

that a single computation of the solar constant takes about one week.

It is of course the third branch of the inquiry, namely, the calculation of total atmospheric absorption from the bolometric measures, which offers the chief and perhaps insurmountable uncertainty. The measures on the extinction of homogeneous rays are assumed to be expressed by a logarithmic formula as follows :—

$$\log d = m \log a + \text{constant},$$

in which d is the ordinate of the bolographic energy-curve corrected for instrumental absorption, m the air-mass (proportional to the secant of the zenith distance of the sun for moderate inclinations), and a the assumed transmission of the atmosphere for vertical incidence. If a is constant during the period of observation, the expression is in the form of the equation of a straight line, the tangent of whose inclination is $\log a$. Accordingly the measures of a single day, for separate wave-lengths, are plotted (Pl. I.) * with $\log d$ as ordinates, and m as abscissæ, and from these plots the values of a are determined. The logarithmic formula is itself immediately derived from the exponential formula of Bouguer ; which for constant barometric pressure and homogeneous rays may be written :—

$$e = e_0 a^m,$$

where e is the energy of the ray observed at the earth's surface, e_0 its energy outside the atmosphere. Since d, the corresponding ordinate of the bolographic curve, is proportional to the energy of the ray, we may write

$$d = d_0 a^m, \quad \text{and} \quad d_0 = \frac{d}{a^m}$$

where d_0 is the ordinate of the bolographic curve corrected for atmospheric transmission.

Thus, assuming that we know a, and knowing m, we may immediately determine corrected ordinates representing the bolographic curve outside the atmosphere. The total area under such a curve is proportional to the total energy of all wave-lengths, so that the ratio of the area of the curve outside the atmosphere to the area of the curve at the earth's surface is the correcting factor to be applied to the solar radiation as observed at the surface of the earth, to give the value of solar radiation outside the atmosphere.

As remarked by the writer at page 147 of the Report of

* The plots here shown are borrowed from the " Smithsonian Miscellaneous Collections."

the Mount Whitney Expedition :—" *If the absorption were invariable at all hours of the day and for all parts of the earth situated at the same height above the sea and subjected to the same air-pressure*, the comparison of high and low sun observations would give us the true energy outside the air, whether the absorbent material were distributed uniformly throughout the atmosphere or were gathered into horizontal layers superposed according to any law whatever.   The character of the atmosphere interposed between us and the sun, however, is constantly varying through the day ; and even if it were at rest, a vertical section would have a different composition from that made at a very great inclination, which would necessarily pass over portions of the earth's surface subjected to conditions very different from those existing at the place of observation."

These observations apply to the methods of reduction now employed, but owing to the increased accuracy of observation, due to the great improvements of apparatus and especially to the writer's introduction of the automatic or so-called bolographic recording devices, it is no longer necessary to follow the sun very long or to very great zenith distances, to obtain differences of ordinates of the bolographs measurable with sufficient exactness.   The determinations are now chiefly made between the hours of 1 and 4 P.M., and it is only the air which transmits the rays during this interval, whose uniform transparency is in question. While the atmosphere extends to an indefinite elevation, the portions within a few miles of the earth's surface contain almost all the variable constituents of it, and the path of the ray in these lower layers does not sweep over a very large extent of the earth's surface between the hours of 1 and 4 P.M.

The following table, computed for the times of the equinoxes, gives the areas of the earth's surface over which the lower air-layers must remain unaltered in transparency during the observations, if good results are to be obtained.

Area over which constant transparency is required.	In the air-layer below an elevation of
0·03 sq. miles.	1/5 mile.
0·86 ,,	1 ,,
3·0 ,,	2 miles
6·8 ,,	3 ,,
12·1 ,,	4 ,,
304·0 ,,	20 ,,

As nearly all the clouds, water-vapour, and other variable constituents of the atmosphere are contained in the layers below a two-mile elevation, it is immediately seen that the region over which approximately constant conditions must, for good observations, continue for three hours, is not many square miles, and this is a demand which can ordinarily be approximately met.

Nevertheless it is obvious that if there was no criterion as to whether constant conditions had prevailed during the measurements, the mere smallness of the period of time and of the area concerned, would be of little weight for inference of the value of the resulting solar constant.

There are various kinds of evidence to be derived from the measures themselves, which aid in forming a judgment of their value for determining the constant, and enable us to reject certain days' observations which the impressions of the eye alone as to the clearness of the air might have inclined us to retain.   These evidences are principally as follows :—

First, the bolographic measurements of a single day on homogeneous rays, when plotted logarithmically as already described, should be represented closely by straight lines, if the air and instrumental conditions are nearly invariable. Examples of good results of this kind are shown in Plate I., which gives for March 25 and 26, 1903, the observations and representative lines for wave-lengths $1\cdot027\,\mu$, $0\cdot656\,\mu$, $0\cdot468\,\mu$, and $0\cdot395\,\mu$ *.

Second, the area of the (calculated) spectrum energy-curve outside the atmosphere ought to be nearly the same, whether estimated from one or another bolograph of a given date.   As examples of this and the two following criteria, the reader is invited to consult the table of solar constant values at a later page.

Third, the several solar constant values computed for a given date ought to agree within the limits of experimental error, though depending on independent bolographs and pyrheliometer measures.

Fourth, if no variation of solar radiation occurs over several months, all the values of the solar constant computed during that interval should agree within close limits, regardless of alterations in the transparency of the atmosphere and the altitude of the sun which may have intervened.   In this

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* The dotted portions of the lines of March 26 are for morning observations.   It is seldom found that the morning observations bear this test of uniformity of conditions of the atmosphere, and they are now omitted, and the transmission coefficients based on the afternoon bolographs alone.

connexion the reader's attention is invited to the values of August, October, and December, 1903.

Fifth, as the great bands of the infra-red are presumed to be solely atmospheric, they should disappear in the energy-curve outside the atmosphere. Accordingly the same result should be reached by the extrapolation in these regions, whether we determine a transmission coefficient at the bottom of the bands themselves, or from the smoothed curve above them. It appears from studies of Mr. Fowle, of the Astrophysical Observatory, that such is the case, except for the very deepest bands, where the determination is subject to many kinds of error all known to tend in the same direction.

The coincidence of all these kinds of evidence does not, however, in the writer's belief, give absolute assurance that the values of the solar radiation may not be underestimated, a defect inherent in the method of observation at a single station.

We cannot go outside the atmosphere and prove that our determinations of its absorption are correct, but at all events the results are the best that can be reached at present from our single and low station. We may next consider their numerical values and what they appear to show.

II.

Experimental Results.

(a) *Atmospheric Transmission.*—In the following Table are given the atmospheric transmission coefficients for rays of certain wave-lengths for separate days of 1903–4, and the mean of numerous similar observations of 1901–2. It is to be remembered that these determinations are made only on the most cloudless days, so that they represent a transparency probably above the normal, though they do not include data of days when cumulus clouds were frequent, such as are often associated with a very deep blue intervening sky.

Attention has already been drawn by the writer[*] to the diminished transparency during the early months of 1903 as compared with 1901–2. The reader will now observe that this condition appears not to have continued so marked during the last three months of 1903, although the transparency did not recover fully its maximum value of 1901–2.

It seems probable that such long-continued alterations of atmospheric transparency, if general over considerable areas of the earth's surface, might directly influence observed

* 'Nature,' November 5, 1903.

TABLE I.

Atmospheric Transmission of Radiation for Zenith Sun.

Wave-length ...	μ 0·40	μ 0·45	μ 0·50	μ 0·60	μ 0·70	μ 0·80	μ 0·90	μ 1·0	μ 1·2	μ 1·6	μ 2·0
Date.	Percentage Transmission for Vertical Rays.										
1903. Feb. 19.	67	64	66	72	76	80	83	85	86	90	92
,, ,, 26.	48	60	66	68	74	83	88	90	93	93	92
,, Mar. 3.	40	48	66	73	79	84	87	89	92	95	96
,, ,, 25.	47	50	57	66	72	76	79	81	84	88	89
,, ,, 26.	52	58	62	68	77	80	81	83	85	89	90
,, April 17.	55	60	69	77	80	82	87	90	94	97	97
,, ,, 28.	39	52	56	64	71	74	76	78	82	88	89
,, ,, 29.	46	49	56	66	72	76	77	80	83	88	90
,, July 7.	42	60	66	69	77	82	85	86	88	89	86
,, Aug. 24.	39	52	60	59	78	84	86	87	89	90	89
,, Sept. 14.	52	76	81	82	87	90	91	91	93	94	94
,, Oct. 14.	64	70	76	80	85	88	91	91	91	(92)	(92)
,, ,, 29.	52	59	65	75	80	82	84	85	88	91	(91)
,, Dec. 7.	58	67	76	81	84	87	89	92	93	95	(94)
,, ,, 23.	67	71	75	80	86	89	90	91	92	94	(94)
1904. Jan. 27.	60	67	73	73	79	84	89	91	91	(91)	(91)
,, Feb. 11.	42	55	58	59	70	78	84	89	92	95	(94)
Mean Results.											
1903. Feb.–Aug.	47·5	55·3	62·4	68·2	75·6	80·1	82·9	84·9	87·6	90·8	91·0
1903–4. Sept.–Feb.	56·4	66·3	72·0	75·7	81·6	85·4	88·0	90·0	91·4	93·1	93·0
1903–4.	51·2	59·8	65·2	70·1	78·1	82·3	85·0	87·0	89·2	91·8	91·6
1901–2.	...	...	76·5	76·9	85·7	89·7	91·0	92·1	93·3	93·0	95·0

temperatures, for if the air becomes more opaque, correspondingly less direct solar radiation remains in the beam to be absorbed at the earth's surface, so that diminished surface-temperatures naturally follow. It is, of course, quite possible that in so complicated a thing as climate other factors may tend to reduce such direct effects, but these are not immediately apparent*.

* An effect of diminished atmospheric transparency upon plant-growth seems equally probable ; for it is generally conceded that certain functions of plant-growth depend on a supply of violet radiations. It will be seen that the defect of atmospheric transparency noted in the early months of 1903 tended to reduce the intensity of the violet end of the spectrum most strongly. It would be of much interest to know if such a deficiency of violet rays may not have appreciably affected vegetation during this period.

As for the causes of the increased opacity of the atmosphere, nothing positive is certainly known, though some have surmised that the many and long-continued volcanic eruptions of 1902 may have distributed more fine dust than usual in the air. It can, on the other hand, be more definitely stated that the defect of transparency was not caused by increased water-vapour; for the inferior transparency was observed on several of the driest days on record, as shown by the form of the great water-vapour absorption-bands in the infra-red spectrum.

(b) *Solar Radiation outside the Atmosphere.*—The following Table (p. 86) includes the best determinations of the solar radiation outside the atmosphere, made at the Smithsonian Observatory from October 1902 to March 1904. A part of these were included in Mr. Abbot's paper above referred to, but as additional computations and data are given for several of the days, they are here repeated*. As was stated at an earlier page, the values are not regarded as absolute measures of the solar constant, but only as forming a series comparable among themselves, from which all known terrestrial variations are excluded, and which may furnish evidence of a suspected variability of the solar radiation.

Column 9, giving the radiation outside the atmosphere, is derived by multiplying the radiation at the earth's surface, given in Column 7, by the ratio of the area of the energy-curve outside the atmosphere to that at the earth's surface, as given in Columns 6 and 5, and correcting to mean solar distance by Column 8. As already said, the agreement, both in Column 6 and Column 9, of the two computations of the same day, furnishes two checks on the goodness of the day's work. Different instrumental conditions may, however, alter the values in Column 6 as between different days.

It will be seen that the days of observation during 1903 fall in two groups, yielding high values and low values of solar radiation respectively, the former including all days prior to March 26, and the latter all days subsequent to that date. A possible return to the higher values is indicated by the observations of February 11, 1904, but we must await subsequent measures to confirm this.

* The values of March 3, April 17, and April 28, included in Mr. Abbot's paper, are here omitted because the observations of those days are of little weight.

TABLE II.—Measures of the "Solar Constant."

1. Date	2. Character of Observations.	3. Hour Angle West.	4. Air Mass.	5. Area of Energy Curve. At Earth's Surface.	6. Area of Energy Curve. Outside Atmosphere.	7. Total Radiation at Earth's Surface. Calories per cm.² per minute.	8. Correcting Factor for Solar Distance.	9. Total Radiation outside Atmosphere Corrected to Mean Solar Distance. Calories per cm.² p. minute.
		h. m.						
1902. Oct. 9.	Fair.	0 06	1·42	⋮	⋮	1·42	0·994	2·19
,, ,, 15.	,,	1 31	1·62	⋮	⋮	1·44	0·989	2·19
,, ,, 22.	,,	3 01	2·41	⋮	⋮	1·30	0·985	2·16
1903. Feb. 19.	Good.	1 01	1·64	243	423	1·35	0·975	2·28
,, Mar. 25.	Excellent.	2 22	2·00	224	431	1·20	,,	2·25
,, ,, 26.	Excellent *	3 01	1·45	186	355	1·19	0·996	2·26
,, Apr. 29.	Excellent.	3 52	2·50	137	365	0·83	,,	2·21
,, July 7.	Fair.	1 44	1·44	209	380	1·16	0·997	2·10
,, ,,	,,	2 57	1·75	200	398	1·05	,,	2·08
,, Aug. 24.	Good.	1 59	1·18	219	377	1·11	1·018	1·94
,, ,,	,,	2 39	1·31	202	370	1·05	,,	1·97
,, Oct. 14.	Good.	0 26	1·07	188	302	1·31	1·030	2·16
,, ,,	,,	3 51	1·51	162	303	1·10	,,	2·11
,, ,, 29.	Good.	3 36	1·18	210	348	1·14	1·023	1·93
,, Dec. 7.	Good.	1 17	1·24	202	345	1·12	,,	1·95
,, ,, 23.	Good.	1 47	1·72	250	408	1·23	0·990	1·98
,, ,,	,,	1 58	1·88	246	408	1·18	,,	1·94
1904. Jan. 27.	Fair.	2 25	1·69	233	415	1·13	0·981	1·97
		0 59	3·34	221	479	0·92	0·970	1·94
		2 45	2·52	242	423	1·16	0·970	1·96
		1 39	3·38	205	418	1·02	,,	2·01
,, Feb. 11.	Fair.	1 41	2·20	205	369	1·18	0·970	2·05
,, ,,	,,	1 49	2·98	169	350	0·99	,,	1·98
,, ,, ,,	,,	1 19	1·81	187	374	1·18	0·972	2·29
		2 29	2·27	168	381	1·02	,,	2·24

* This day has been called "excellent," though in computations of the area of the energy-curve outside the atmosphere there is a larger difference than usual. But this is immediately explainable, if, as appears, the solar radiation itself was rapidly diminishing at the time; for this would result in too small apparent transmission coefficients, so that the second area computed would be larger than the first.

III.

Possibility of Considerable Changes in the Amount of Solar Radiation outside the Atmosphere.

Looking at the general results, these seem then to indicate a possibility that a rapid fall of solar radiation occurred about the close of March*, and that subsequently the radiation continued nearly or quite 10 per cent. less than before. This, if certain, would be important, and we may inquire what causes on the sun could produce such a change, and what effects might be expected to be produced on the earth if it occurred.

The writer showed nearly thirty years ago† that the envelope of the sun profoundly influences by its absorption the radiation received by the earth. While the absorption in the solar envelope is not exactly known, still so much is known that we may infer that if it were absent for a moment, the earth would receive nearly double its present amount of heat. If a variation of 10 per cent. in the transparency of this envelope occurred, nearly 10 per cent. of change in the solar radiation outside the earth's atmosphere would follow.

If a change of solar radiation did occur, there ought to have been a similar fall of terrestrial temperatures afterwards; and we may inquire how great this fall of temperature should be.

The earth may be regarded as a body at a mean temperature of $290°$ absolute ($17°$ C.) maintained at approximately constant temperature by a balance between solar radiation received and terrestrial radiation emitted. It is here assumed that all sources of heat other than the solar radiation are negligible, but if any or all of them are not so, their presence will be to reduce the effect on temperature, of a fall in solar radiation.

Recent studies of German physicists have experimentally verified, for the perfect radiator, Stefan's law that the emission of a heated body is proportional to the fourth power of the temperature‡. Other bodies not perfect radiators depart from this law in the sense that while radiating less absolutely than the perfect radiator, their emission is more nearly proportional to a power of the temperature higher than the

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* It is of interest to note that a marked increase of sunspots occurred on March 21. See Report of the Council, Royal Astronomical Society Monthly Notices, vol. lxiv. p. 357.

† *Comptes Rendus,* Sept. 6, 1875.

‡ O. Lummer, *Rapports présentés au Congrès International de Physique,* 1900, vol. ii. pp. 78–81.

fourth[*]. Suppose T_1 to be the mean temperature of the earth corresponding to a rate of solar radiation S_1, and T_2 that corresponding to S_2. Assume further that the reflecting power of the earth remains unchanged, and that no appreciable heat is received from other sources than the sun. Then

$$\left(\frac{T_2}{T_1}\right)^x = \frac{S_2}{S_1} \text{ where } x > 4.$$

Accordingly, if, as supposed, S_2 is $9/10\ S_1$

$$T_2 > 0\cdot974\ T_1.$$

If $T_1 = 290°$, then $T_2 > 282°\cdot5$, and $T_1 - T_2 < 7°\cdot5$ C.

It may then be stated that if the solar radiation remained for a long period of time at a value which would maintain the earth's surface at a mean temperature of 17° centigrade, and then fell 10 per cent., and so remained indefinitely, the fall of temperature of the earth's surface would be less than 7°·5 centigrade.

But if the solar radiation fluctuated between limits separated by 10 per cent., the fluctuation of terrestrial temperature would be less, according to the frequency of the fluctuations of solar radiation. Again, parts of the earth's surface most closely associated with the oceans by the influences of winds, ocean currents, and rainfall, would be least affected by such solar fluctuations, and would respond most slowly to a permanent alteration of solar radiation.

From the foregoing considerations we may then infer that the effect of a fall of 10 per cent. in the solar radiation should diminish the mean temperature of the earth not more than 7°·5 C., and indefinitely less according to the shortness of the time elapsing before the radiation regained its former value. Stations near the sea, or subject to ocean currents and winds, or to heavy rainfall, would lag far behind stations in the interior of great continents in their temperature fluctuations.

When we come to the study of actual temperatures over the earth's surface, we find that all collections of temperature data for single stations in the interior of great continents, covering long periods of time, exhibit nearly every year such considerable irregular variations from the normal temperatures that we are at no loss to find variations comparable in dimensions with those we are supposing to be caused by a fluctuating solar radiation. But it is only within the last year that we have the series of radiation measures with which to compare temperatures, and we now turn to recent temperatures as published in the *Internationaler Dekadenberichte*

[*] H. Keyser, *Handbuch der Spectroscopie*, vol. ii. pp. 77–82.

of the Deutsche Seewarte for nearly 100 stations, for each ten-day period of 1903, and accompanied by normal temperatures representing the mean for the same ten-day periods of many former years *. From all the stations for which normal values are given, temperature departures have been computed here, and these departures have been averaged by geographical position in seven groups, comprising (1) North America, 20 stations; (2) Insular and South-west Europe and North Africa, 18 stations; (3) North-west Europe, 15 stations; (4) Central Europe, 10 stations; (5) European Russia, 11 stations; (6) Asiatic Russia, 8 stations; (7) High Altitudes in Europe, 7 stations. These seven groups of results are plotted in the accompanying chart (Pl. II.) (in which, to avoid confusion, the lines are alternately continuous and dotted), and as all seem to show the same tendency in more or less marked degree, no hesitation was felt in taking the general mean, giving observations of each station equal weight wherever present in the sum. The general mean for the 89 stations forms the (heavy) eighth line of the chart, and a comparison is invited between its course and that of the ninth and lowest line representing the observations of solar radiation made during 1903 at Washington †. In connexion with this comparison, the reader is reminded that during September an increasing transparency of the atmosphere was noted which may have had the direct effect, discussed at a former page, of increasing terrestrial temperatures thereafter, and thus accounting for the observed rise of the mean temperature at the close of the year. If this be admitted, the general agreement of the observed temperatures, with what radiation measures would lead us to expect, is certainly marked.

As for the total fall of temperature indicated by the charts, that is seen to be a maximum for European and Asiatic Russia, the regions most removed from the influences of the oceans. The fall in the general mean for the land areas of the North Temperate Zone ‡ is, however, over 2° centigrade, or about one-third of the maximum fall possible to be caused

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* The writer is indebted to Prof. Cleveland Abbe and to Dr. W. F. R. Phillips, Librarian of the U.S. Weather Bureau, for their aid in making accessible the publications of temperature data in possession of the Weather Bureau.

† It is to be regretted that only one day of observation with at all satisfactory conditions was secured between April 29 and August 24, and that (July 7) of less weight than the others.

‡ It would be of great interest to extend the discussion of temperature departures to include stations in the interior of South America, Africa, Australia, and Southern Asia, but the data are not now available, and for many regions of these continents may long be wanting.

by a permanent diminution of solar radiation of 10 per cent. It is the absorption of heat by the oceans which slows the terrestrial change (if any) produced by change in the solar constant. Since the indirect effects of the absorption of solar radiation are felt in the oceans to depths of 2000 metres, it may be shown that a change of 2° over the entire surface of the earth would be delayed over a year.

Summary and Conclusion.

A series of determinations of the solar radiation outside the atmosphere (the Solar Constant)* extending from October 1902 to March 1904 has been made at the Smithsonian Astrophysical Observatory under the writer's direction.

Care has been exercised to determine all known sources of error which could seriously affect the values relatively to each other, and principally the varying absorption of the earth's atmosphere. Though uncertainty must ever remain as to the absorption of this atmosphere, different kinds of evidence agree in supporting the accuracy of the estimates made of it, and of the conclusions deduced from them.

The effects due to this absorption having been allowed for, the inference from these observations appears to be that the solar radiation itself fell off by about 10 per cent., beginning at the close of March 1903. I do not assert this without qualification, but if such a change in solar radiation did actually occur, a decrease of temperature on the earth, which might be indefinitely less than 7°·5 C., ought to have followed it.

On comparing the observed temperatures of 89 stations, distributed over the North Temperate Zone, with the mean temperatures of the same stations for many previous years, it is found that an average decrease of temperature of over 2° centigrade actually did follow the possible fall of the solar radiation, while the temperature continued low during the remainder of the year. Stations remote from the retarding influence of the oceans show a much greater variation than that of the general mean.

While it is difficult to conceive what influence, not solar, could have produced this rapid and *simultaneous* reduction of temperatures over the whole North Temperate Zone, and continued operative for so long a period, the evidence of solar variation cannot be said to be conclusive. Nevertheless, such a conclusion seems not an unreasonable inference from the data now at hand, and a continuation of these bolographic

* I wish to renew my caution that the *absolute* values of the solar constant thus given are more likely to be found in defect than in excess.

studies of solar radiation is of increasing interest, in view of their possible aid in forecasting terrestrial climatic changes, conceivably due to solar ones.

All the preceding observations, while under my general direction, have been conducted in detail by Mr. Abbot, to whom my thanks are especially due.

Washington, April 15th, 1904.

IX. *Some Instruments for the Measurement of Large and Small Alternating Currents.* *By* W. DUDDELL, *Wh.Sc.*[*]

[Plate III.]

FOR the measurement of alternating currents over the range from 0·1 to 100 amperes, there are a large number of commercial instruments available which will give fairly accurate results and which are quite easy to use. As soon as one requires to measure a current much outside this range, say $\frac{1}{100}$ ampere or 1000 amperes, then more or less serious difficulties are encountered, even if a moderate accuracy of say 2 or 3 per cent. is required. I propose to describe in this paper some instruments which I have devised, by means of which very small alternating currents and voltages can be measured, and which are also suitable when shunted for very large currents.

In order that an instrument shall read R.M.S. or effective amperes, it is necessary that its deflexion shall be determined by the mean squared value of the current flowing. This leads to a difficulty when it is required to construct instruments for very small currents, which I will illustrate by an example.

Suppose an instrument gives a certain deflexion for 1 ampere and that it is required to alter it to give the same deflexion for a milliampere, then the available deflecting force, which depends on the square of the current, will be reduced in the ratio of one million to one. This is the reason why so little advance has been made in the measurement of very small alternating currents compared to that made in the measurement of direct currents.

The means at our disposal to measure alternating currents may be roughly divided under three heads according to which of the effects produced by the current is taken as a measure of it.

(1) *Electromagnetic Instruments.*—These may be further subdivided into two classes, namely: (*a*) Pure-air core-dynamometers, in which the current to be measured flowing

* Communicated by the Physical Society : read May 6, 1904.

through one coil reacts on the same current flowing through another coil, no magnetic material of any kind entering into the instrument. (*b*) Instruments containing iron either as a core to the two coils, or as the part reacted on by the magnetic force of one coil. In all these instruments the essential part is a coil or coils carrying the current to be measured which produce magnetic fields; and it is in the design of these coils that the main difficulties are encountered. If it is required to measure a small current, then the coils must be wound with fine wire and contain many turns, so that both their resistance and self-induction are high, which in many experiments is very undesirable, if not absolutely prohibitive.

I came across a very good example of the importance of the self-induction of dynamometers for measuring small currents in connexion with some experiments in which I wanted to measure 0·1 amp. at a frequency of 100,000 ∼ per second. A small reflecting dynamometer was available having 8 turns in fixed and 40 turns in moving coil, which gave about 100 scale-divisions deflexion for 0·1 ampere. On attempting to use it no appreciable deflexion could be obtained, although everything appeared to be working satisfactorily; the trouble was finally traced to the dynamometer behaving like an insulator owing to the great importance of its self-induction at this high frequency. Another effect which may occur at high frequencies with coils of many turns and layers, is that the capacity effect from wire to wire and layer to layer may shunt some of the current which should pass round the instrument, and so destroy any accuracy in the reading. In fact, given a good instrument, the evaluation of high-frequency alternating currents is of great difficulty owing to the errors introduced by the self-induction and capacity of the leads, resistances, &c. which may be used in the measuring circuit.

If the resistance and self-induction of the instrument are of no consequence and the frequency is not high, then about 0·001 ampere can be accurately measured by means of a dynamometer without much difficulty.

The measurement of large alternating currents by electromagnetic means offers quite another set of problems. To carry large currents large conductors are necessary, which have to be carefully stranded so as to avoid skin-effects. This stranding presents many serious practical difficulties, as in order to make the current divide equally between the strands it is necessary to make the self-induction and resistance of each strand the same, and also the mutual induction

of all the other strands on it the same.   The suitable arrangement of the strands leads to the use of large cables and large-sized coils, generally consisting of a single turn.   There is a further very serious obstacle to be overcome, namely, eliminating the effect of the magnetic field surrounding the cables which connect the instrument to the supply.   There seems no reason why all these difficulties should not be overcome except the large size and cost of the instrument and considerable loss of power in it.

It is true that transformers can be used to bring the current within the easily measured range, but only at the cost of extra power losses in them and uncertainty in the calibration unless calibrated for the particular frequency and wave-form on which they are to be used.   In most cases, when it is required to measure very small currents step-up transformers cannot be used owing to the power lost in them disturbing the circuit in which it is required to measure the current.   Dynamometers cannot in general be shunted to use them for large currents unless the drop on the shunt is at least $\frac{1}{2}$ volt, owing to the necessity of using a considerable dead resistance in series with the dynamometer itself to swamp its self-induction and temperature-coefficient errors.   Such a drop as $\frac{1}{2}$ volt on the shunt corresponds to a considerable waste of power with large currents; thus with 2000 amperes to be measured the loss of power is 1000 watts, which requires a large shunt to dissipate it.

The above remarks, which refer more especially to pure-air core-dynamometers, apply also to iron core or attracted iron type of instruments, with the further disadvantage of this latter class of instrument being subject to frequency and wave-form errors.

(2) *Electrostatic Instruments.*—In this method the current is measured by measuring the P.D. between the terminals of a known non-inductive resistance through which the current flows.   Using carefully designed non-inductive resistances, this is probably one of the most accurate methods of measuring alternating currents of any value, frequency or wave-form ; its chief limitation lies in the fact that up to the present no electrostatic voltmeter has been put on the market which can be relied on to measure accurately P.D.'s much below 1 volt, and it is better to work with 4 or 5 volts.   When measuring large currents a drop of 1 volt on the shunt is generally quite inadmissible ; and when measuring very small currents the P.D. in the whole circuit is often much less than 1 volt.   It must also not be forgotten that at high frequencies electrostatic voltmeters take an appreciable current which must be corrected for.

(3) *Thermal Instruments.*—Probably instruments depending on the heating produced by the current are as much used as any others for measuring alternating currents. If the resistance of that part of the instrument which is heated by the current is constant, then the mean rate of production of heat is accurately proportional to the mean square of the current. If this rate of production of heat can be accurately measured, then we obtain an ammeter whose accuracy is entirely independent of wave-form or frequency, and whose calibration is the same for both direct and alternating currents, with the further advantage that the instrument is unaffected by magnetic fields, and can be constructed practically non-inductive.

There would seem, at first sight, to be a very large number of ways of accurately determining the rate of production of heat ; but this is not the case if the watts lost in the instrument are kept small, as this corresponds to a small rate of production of heat which is difficult to measure. In practice the methods of measuring the rate of production of heat have been reduced to four, viz. :—The linear expansion of the heated wire. The expansion of a gas or liquid near or surrounding the wire. The measurement of its change in resistance. The measurement of the temperature by means of a thermocouple.

It must be noted that in all these methods what is actually measured is a rise in temperature and not the rate of production of heat, which is really required, the assumption being that when the steady state is attained and the heat losses from the wire or other heated part equal the rate of production of heat, then the heated part will be at a definite temperature above some other part of the instrument. This fact that what is really measured is a difference of temperature, and not the rate of production of heat, is the cause of most of the defects of thermal instruments. The main defects are the following:—

Slowness of action caused by having to wait till the steady state is attained.

Creep of zero caused by change of temperature of surrounding bodies.

Comparatively large power required to work the instrument due to the great inefficiency of converting electrical energy into heat and then reconverting the heat into mechanical energy to move the pointer.

Temperature errors caused either by the resistance of the heated part varying with temperature, or by the means of measuring the rate of production of heat not being independent of temperature.

Easily destroyed by an abnormal current owing to the small mass and normally high temperature of the heated part.

The new thermal instruments now to be described were designed to get over one or more of the above difficulties for special purposes, and do not pretend to have overcome the whole of them.   The first instrument was designed to be very quick in action for a thermal instrument, and was made to work with a small current so that it could be used as a quick-acting voltmeter.   It is essentially a very delicate Ayrton-Perry twisted strip-ammeter which has been improved by the addition of a temperature-compensation device to minimize the zero-creep when the temperature of the whole instrument changes.   The working parts of the instrument are shown in fig. 1.   A B C D is the Ayrton-Perry twisted strip, of which

Fig. 1.
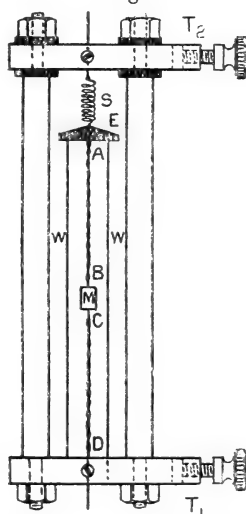
the part A B is twisted in one direction and the part C D in the opposite.   A mirror M and a very thin mica damping-vane are fixed at its centre.   This strip is stretched in a frame formed of a brass block T_1 carrying one terminal and a piece of ebonite E, the sides of the frame being formed of wires W W. This frame itself is stretched by means of a spiral spring S fixed to the other terminal block T_2.

The twisted strip A B C D and the wires W W are made of the same metal ; in fact, the strip A B C D is actually flatted from the wire W W, so that the twisted strip and wires have the same temperature-coefficient of expansion.   If the wires and strip rise in temperature equally, then the whole frame E W T_1 W simply gets longer and no twist of the mirror ensues.   If, however, a current be sent from T_1 to T_2

through the strip, then it heats and twists up, rotating the mirror M. Owing to the fineness of the strip (0·001″ Pt Ag) which is heated by the current, the instrument is very quick in action. The mechanical periodic time is also very short, about $\frac{1}{15}$ second ; so that it is able to follow with a fair accuracy currents which vary over a small range as rapidly as one or two cycles per second. The data of this actual instrument are :—

Resistance 20 ohms.

Current to give 25 cm. at 100 cm. scale-distance, 22 × 10⁻³ ampere.

P.D. to give 25 cm. at 100 cm. scale-distance, 0·44 volt.

So that taking 1 cm. as the smallest measurable deflexion and 0·1 mm. as the smallest detectable movement—

The smallest measurable current is $4·4 \times 10^{-3}$ ampere.

      ,,   detectable   ,, $0·5 \times 10^{-3}$ ampere.

Watts required to produce smallest measurable deflexion, *i. e.* 1 cm., 387 micro-watts.

It is evident that this very simple instrument has many uses. It is very easily set up, requiring no careful levelling, and is quite robust. The present instrument which I made three years ago has often been carried about just as it is in the pocket. The self-induction of the wire is extremely small, and the temperature-coefficient of the resistance of the wire (″Pt Ag) is small also ; so that the instrument can be used as a voltmeter to measure voltages down to about 0·1 volt, and I have used it in series with high resistances to measure voltages up to 10,000 volts. The instrument can of course be shunted to measure large currents ; but if a good deflexion of say 25 cm. is required, the drop in volts on the shunt, 0·44 volt, is serious. Its main defect is that it requires screening from quick vibrations of the order of $\frac{1}{15}$ second, as the damping is not quite sufficient. I find that this can be easily done by standing the instrument on a heavy block which is suspended by means of wires and springs as in the Julius suspension for galvanometers.

The chief practical use to which I have put the instrument has been to observe the quick variations of the R.M.S. voltage in supply-stations produced either by cyclic irregularity of the engine or by phase-swinging between alternators, converters, &c. For this purpose I put the instrument in the place of my oscillograph in the ordinary film-camera recording apparatus, and photograph the movement of the spot.

It is easy by working to a false zero to obtain a sensibility of 1 cm. change in deflexion for 1 per cent. change in the P.D.

By observations of this kind it is often possible to trace the causes of alternators not operating properly in parallel and to find which particular defect and which engine is the cause.

Figs. 2 and 3 (Pl. III.) are two records obtained on a circuit in which there was purposely produced a cyclic irregularity having a known wave-form. In each case the cyclic change on the voltage was about ± 1 per cent. from the mean. In fig. 2 the voltage was changing sinusoidally about 121 $\sim$ per minute, and in fig. 3 it was changing along a square wave-form about 68·5 $\sim$ per minute. In this latter record the shape of the curve of growth and decay of deflexion and the consequent rounding off of the corners of the curves are well shown. In both records 1 cm. = 1 per cent. change in voltage and 10 cm. = 1 second.

The second instrument is much more delicate and sensitive. It was made primarily to measure very small voltages and currents even with very high frequencies up to 120,000$\sim$ per second. At this high frequency the alternator I made would only give an extremely small output ; it was therefore necessary, in order to carry out the experiment, to reduce the power required to work the instrument to its lowest possible value. I have called this instrument a Thermogalvanometer.

The principle of the thermogalvanometer is quite simple. It consists of a resistance which is heated by the passage of the current to be measured, and the radiant heat from which falls on the thermojunction of a Boys radio-micrometer *. As at first constructed, it consisted of a heating-resistance made of three or four turns of 0·001 inch diameter platinum-silver wire wound on a piece of mica, and placed as close as possible to the radiation receiving-plate of an ordinary Boys radio-micrometer, made by the Cambridge Scientific Instrument Company. This instrument was sensitive but exceedingly slow in action, taking over a minute to approximately attain the deflexion corresponding with the current flowing.

A new radio-micrometer was therefore constructed following the instructions given in Professor Boys' paper, but having a very much smaller suspended loop than the radio-micrometer as usually constructed. The radiation receiving-plate was also omitted in order to reduce the quantity of metal to be heated, and the heating resistance was placed directly underneath the lower thermojunction, so that the junction received heat from it both by radiation and convection. The arrangement of the instrument is shown diagrammatically

* Philosophical Transactions of the Royal Society, 1889, vol. clxxx. p. 159.

in fig. 4, and a photo of the actual instrument with the case removed in fig. 5 (Pl. III.).

In the field between the pole-pieces NS (fig. 4) of a permanent magnet is suspended by means of a quartz fibre a single-turn coil or loop of wire l, to the lower ends of which is fixed the thermocouple Bi, Sb. This loop is surmounted by a glass stem g and a mirror M. Below the lower junction of the thermocouple is fixed the heating resistance h, one end of which was connected to the frame of the instrument to avoid electrostatic forces. The action of the instrument is as follows. The current to be measured flows through h and raises its temperature, causing the lower junction of the thermo-couple to rise in temperature above the upper, thus producing a current round the loop, which is deflected by the magnetic field against the tor-sion of the quartz fibre.

The data of the instrument are:—
Resistance 18 ohms.
Current to give 25 cm. at 100 cm. scale-distance, 8×10^{-4} ampere.
P.D. to give 25 cm. at 100 cm. scale-distance, $14\cdot4 \times 10^{-3}$ volt.
So that, taking as before 1 cm. as the smallest measurable and 0·1 mm. as the smallest detectable deflexion,
The smallest measurable current is $1\cdot6 \times 10^{-4}$ ampere.
The smallest detectable current is $0\cdot2 \times 10^{-4}$ ampere.

Watts required to produce smallest measurable deflexion, :0·46 micro-watt.
The deflexions of the instrument were nearly proportional to the square of the current; assuming the instrument correct at 100 divisions

Fig. 4.

deflexion, then higher deflexions required to be increased by amounts gradually increasing up to between 4 and 5 divisions
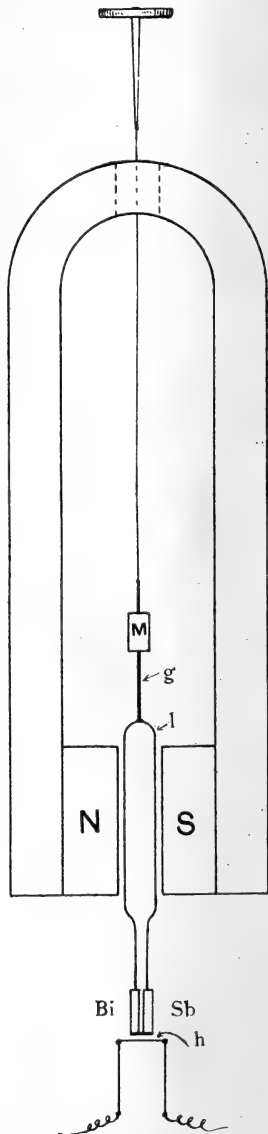
at a deflexion of 400 divisions, in order that the corrected deflexion might be accurately proportional to the square of the current flowing.

To give an idea of the time the instrument requires to take up the permanent deflexion corresponding with any current, Table I. is given below of the deflexions at different times after starting a current of 8×10^{-4} ampere; and after interrupting the same, to show the time required for the instrument to return to zero.

From which it will be seen that the instrument attains after 10 seconds its full deflexion to within 1 part in 500, or as it is a square law instrument indicates the true current to within 0·1 per cent.

TABLE I.

Time in seconds after starting a current of 8×10^{-4} ampere.	Deflexion in scale-divisions.	Time in seconds after interrupting the current.	Deflexion in scale-divisions.
5	500·5	5	1·5
10	501·0	10	1·0
30	501·5	30	0·5
60	502·0	60	0]
120	502·0	120	0
180	502·0	180	0

This instrument, which forms practically a sensitive alternate-current galvanometer, worked very satisfactorily, even with the highest frequencies used of 120,000 ~ per second. As examples of its sensibility, it may be mentioned that on making a suitable noise into a Bell *telephone-receiver* sufficient current is generated to send the spot off the scale; and that if the thermogalvanometer be connected to the line wires of a *microphone-transmitter* arranged in the ordinary way, whistling at a distance of from 15 to 20 feet from the microphone will cause deflexions of the instrument of several hundred scale-divisions. As the instrument will give a deflexion of over two scale-divisions for one millivolt alternating, it might in some cases replace, with advantage, the telephone in the Kohlrausch method of measuring the resistance of electrolytes, as it would avoid the difficulties attendant on estimating the position of a sound minimum.

It is perhaps of interest to compare the current taken by the thermogalvanometer with that which flows through an electrostatic voltmeter when both are used to measure the

same P.D. at high frequencies.   Thus at 100,000 $\sim$ per second
a certain sensitive " Ayrton-Mather " electrostatic voltmeter,
which had applied between its terminals a P.D. of 3·6 volts
and gave a deflexion of 392 scale-divisions, was found to
have an alternating current of 0.12×10^{-3} ampere flowing
through it due to its capacity.   In order to produce the same
angular deflexion on the thermogalvanometer, a current of
about 0.8×10^{-3} ampere was required, so that for the same
sensibility, considered as voltmeters, the thermogalvanometer,
a hot wire instrument, only required less than 7 times the
current taken by a sensitive electrostatic voltmeter, at a
frequency of 100,000 $\sim$ per second.   Later modifications of
the instrument have actually reversed the positions, so that
the thermogalvanometer can be made to take less than one-
third the current required by the electrostatic voltmeter to
give the same deflexion.

The sensibility and the relative calibration depend on the
resistance and exact position of the heater h.   It is evident
that if it is required to measure a very small current, then
the heater must have as high a resistance as possible; con-
versely, to measure a low voltage h should have a low re-
sistance, so that for any given purpose there is a best
resistance for the heater.

The power dissipated by the heater to work the instrument
does not depend much on the resistance of the heater.

Owing to the fact that the heater must be small in order
that the instrument may be quick in action, I have usually
used a straight wire about 3 or 4 mm. long, as shown at h,
fig. 4, or sometimes a single loop.   It was easy enough to
make heaters of this type having a resistance under 5 ohms,
but for higher values great difficulties were experienced.
Straight heaters 3 mm. long, having resistances up to 20 ohms,
were finally made by cutting very narrow strips out of gold-
leaf.   In this way I have made strips of gold 1 inch long
having a resistance over 100 ohms.   The manufacture of
still higher resistances presented great difficulties, until lately
I have succeeded in platinizing glass threads and soldering
them in place.   By this means it is quite simple to make
heaters having resistances up to 1000 ohms for 3 mm. length
and still higher values can be made.   These resistances will
easily carry sufficient current for the present purpose; a
2000 ohm heater stood 2 milliamperes without damage, but
was destroyed by 6 milliamperes.   It remains, however, to
be seen whether they deteriorate with time, up to the present
the results with heaters under 1000 ohms seem satisfactory.

A set of these heaters have been made and put in the above

thermogalvanometer, and the results obtained are given in Table II.

The currents, P.D.'s, and power are those required to give a deflexion ¼ the scale-distance, which forms a very convenient basis on which to compare square law instruments. I have also included the Ayrton-Perry twisted strip instrument in the table for comparison.

TABLE II.

For deflexion of 25 cm. at 100 cm. scale-distance.

Instrument.	Resistance in ohms.	Current in micro-amperes.	P.D. in milli-volts.	Power in micro-watts.
Thermogalvanometer, gold heater	18	800	14·4	11·5
Thermogalvanometer, platinum on glass heater	103	346	35·6	12·3
Do. do.	202·5	275	55·6	15·3
Do. do.	363	231	84	19·4
Do. do.	1071	121	130	15·7
Do. do.	3367	88	296	26·0
Do. do.	13910	31	431	13·9
Ayrton-Perry twisted strip	20	22000	440	9680

The small power taken to deflect this instrument combined with the fact that the self-induction and capacity of the instrument are so extremely minute, and that it obeys a square law, should render it very serviceable for many measurements which I need not detail. One instrument can be easily used for various sensibilities by simply changing the heater; and should at any time a very excessive current be put through the instrument and destroy the heater, it can be readily replaced without touching the delicate suspended system.

With the highest resistance heater so far tested, namely 13910 ohms, a detectable deflexion of 0·1 mm. at one metre is given by a current of 0·6 micro-ampere. This resistance is not the highest which I think can be made, as I succeeded in making one of 25,000 ohms, which unfortunately got broken before the sensibility was tested. The radio-micrometer part can also be easily made more sensitive. I have, therefore, reason to hope that it may be possible to improve this thermogalvanometer so as to detect small fractions of a microampere with it.

The present high sensibility combined with the suitability for use with high-frequency currents, leads me to think that it might be applied to measure the current in the vertical

receiving wire in spark telegraphy, and thus permit some quantitative measurements to be made which are so much wanting in the published literature on this subject.

The controlling force in the thermogalvanometer is extremely small, and it seemed to me for some time a very drastic procedure to pivot such an instrument and apply to it ordinary controlling springs so as to develop a switchboard instrument from it.   Measurements of the power obtainable from different designs of thermojunctions and heaters, led me to conclude that a pivotted instrument might be constructed.   I have now made two such instruments. In order to save time these two first instruments were made by converting ordinary moving-coil direct-current instruments.   So that it is probable that improvements will be effected when the instruments are manufactured from the commencement specially for the purpose.   These two first instruments have turned out very successful.   Both instruments have ordinary pointers about 10 cm. (4 inches) long, and deflect through an angle of about 75° for their full scale-deflexion.   No. 1 is cased in wood with a mirror under the pointer.   No. 2 is iron-cased of the ordinary switchboard type.   The works are the same except that one has a heater designed for a small current and the other for a small P.D.

The data of the instruments is as follows :—

	No. 1.	No. 2.
Current for full scale-deflexion	0·1 ampere	2·0 amperes.
P.D. „ „ „	2 volts	0·15 volt.

These instruments attain to within 1 per cent. of the proper deflexion in 10 seconds after starting the current, so that they are fairly quick in action.   They have the property of reading slightly too high (about $\frac{1}{2}$ per cent.) if the current be suddenly brought to its full value, when the instrument is quite cold and has been out of use for some time, the reading decreasing to within $\frac{1}{4}$ per cent. of the proper value during the first 30 to 40 seconds after starting the current.

The creep of zero is small; in an ordinary room it is practically nil.   If the back of the iron-cased instrument where the thermo-junction is situated is heated in front of the fire till hot to the touch while the front remains quite cold, a zero creep of about 1° can be produced.   If this great difference in temperature should occur in practice, which is highly improbable, it will produce an error in measuring the full current of under 1 per cent.

The power required to work the instrument, namely 0·2 to 0·3 watt, is comparatively low for pivotted thermal instruments.   The low current, 0·1 ampere, taken by No. 1, and

the low P.D., 0·15 volt, required by No. 2, compare very favourably with other similar instruments. If No. 2 is shunted for 1000 amperes the loss will be only 150 watts, and a comparatively small shunt will be required. The same applies to No. 1, if used in series with a high resistance or connected to a transformer as a voltmeter.

Temperature error.—In the two present instruments this error is about 1 per cent. for 15° F., the deflexion decreasing with increase of temperature. It is to be noted that this low value has not been obtained by making the moving-coil circuit have a resistance which is practically independent of temperature, but by so choosing the materials of the thermo-junctions that the thermoelectric power rises as the temperature rises, and so partially balances the increase in resistance of the coil. I have found materials which will make this balance complete, and so do away with the temperature correction, but practical reasons which will be referred to later have weighed against their use. If the instrument is required only to measure small currents and not to be shunted—this is the case of a voltmeter for, say, 50 volts or over—then the temperature error can be easily reduced to a still smaller value by suitably choosing the alloy of which the heater is made.

The iron-cased instrument No. 2 will stand a fairly large excess-current without being injured. It will carry twice the normal current required to give a full-scale deflexion for half-an-hour without being permanently injured or its calibration altered. It has also had $2\frac{1}{2}$ times the normal current put through it for 20 seconds without injury. Of course, after such ill-usage, the calibration is temporarily altered a few per cent., but the instrument has so far always recovered itself in about $\frac{1}{4}$ hour.

The necessity of making the instrument to stand this large overload, limits very seriously the choice of materials and the design of the heater and thermo-junction. If I could be certain that the instrument would not be seriously overrun, the present small temperature error could at once be got over or more sensitive instruments could be built. For practical work, whether on the switchboard or in the laboratory, it is essential that ammeters shall stand safely a considerable excess-current without being injured, for it is better to have a little less accuracy than no ammeter at all at the critical moment when the reading is required.

I must now refer to an objection which is special to this type of instrument, namely, that its calibration is not exactly the same in the vertical as in the horizontal position. I think

that this difficulty can be overcome, and I think that in any case too much importance should not be attached to it. Switchboard instruments can be calibrated in the vertical position, and are not likely to be used in any other.

Laboratory standard instruments will naturally be calibrated and used horizontally as usual, because the pivot friction is less in this position.

The heaters in these instruments are made of platinum alloys to stand the high temperature and not deteriorate. Each of the heaters is quite small, and would get easily into a space $6 \times 6 \times 0.1$ mm. That in No. 1 consists of a short piece of wire bent zigzag, and that in No. 2 of a piece of foil. The self-induction of these heaters is therefore very small, probably much less than that of any leads that can be used between them and the circuit in which the instruments are used. The instruments can be used to measure very high frequency currents.

The question of the permanency of the calibration is of course a vital one. Judging from our knowledge of the permanency of ordinary moving-coil intruments and the great stability of the platinum alloys used for the heaters, I see no reason to expect any serious alteration in these with time. The constancy of the thermo-junction and its thermo-force is open to question, which can only be satisfactorily answered by time. Up to the present the instruments are keeping all right.

The design of sufficiently non-inductive shunts to carry several thousand amperes for use with these and other instruments is receiving attention from Mr. Mather and myself, and the matter is being experimentally investigated. I have no doubt that the shunts can be made, it is merely a question of the best design.

The advantage of being able to use a single instrument like No. 2, which can be easily and accurately standardised with direct current in connexion with a set of standard shunts giving 0.15 volt drop for all currents from 2 amperes to several thousand amperes, will be appreciated by all who have any quantity of testing work to carry out.

The ease with which one can measure any direct current with a single good moving-coil ammeter and a set of shunts, and the facility with which one can check the accuracy of the instrument and shunts, have always greatly appealed to me, and the present instrument is, I venture to think, a step forwards in this direction in the case of alternating currents.

X. *On the Electrical Vibrations associated with thin terminated Conducting Rods.* By Lord RAYLEIGH, O.M., F.R.S.[*]

IN his discussion of this subject Prof. Pollock[†] rejects the simple theoretical result of Abraham and others, according to which the wave-length (λ) of the gravest vibration is equal to twice the length (l) of the rod, in favour of the calculation of Macdonald which makes $\lambda = 2\cdot53\, l$. On this I would make a few remarks, entirely from the theoretical point of view.

The investigation of Abraham[‡] is a straightforward one; and though I do not profess to have followed it in detail, I see no reason for distrusting it. It relates to the vibration about a perfect conductor in the form of an elongated *ellipsoid* of revolution; and the above-mentioned conclusion follows when the minor axis ($2b$) of the ellipse vanishes in comparison with the major axis l. As a second approximation Abraham finds

$$\lambda = 2l\,(1 + 5\cdot6\,\epsilon^2),\quad \cdots \quad (1)$$

where

$$1/\epsilon = 4\,\log\,(l/b).\quad \cdots \quad (2)$$

But a question arises as to whether a result obtained for an infinitely thin ellipsoid can be applied to an infinitely thin rod of *uniform section*. So far as I see, it is not discussed by Abraham, though he refers to his conductor as rod-shaped. The character of the distinction may be illustrated by considering the somewhat analogous case of aerial vibrations within a cavity having the shape of the conductor. If the section be uniform, the wave-length of the longitudinal aerial vibration is exactly twice the length; but if an ellipsoidal cavity of the same length be substituted, then, however narrow it may be, the wave-length will be diminished in a finite ratio on account of the expansion of the section towards the central parts[§]. This example may suffice to show that no general extension can be made from the ellipsoidal to the cylindrical shape, however attenuated the section may be.

When we ask whether the extension is justifiable in the present case, we shall find, I think, that the answer is in the affirmative so far as the first approximation is concerned, but in the negative for the second approximation.

Let us commence with the consideration of the known solution for an infinite conducting cylinder of radius r_1

[*] Communicated by the Author.
[†] Phil. Mag. vii. p. 635 (1904).
[‡] Wied. *Ann.* lxvi. p. 435 (1898).
[§] See 'Theory of Sound,' § 265.

enclosed in a coaxal sheath of radius r_2*. If in Maxwell's notation P, Q, R be the components of electromotive intensity; a, b, c those of magnetic induction; V the velocity of light; we have

$$P,\ Q,\ R = \cos pt \cdot \cos mz \left(\frac{x}{r^2},\ \frac{y}{r^2},\ 0\right),$$

$$a,\ b,\ c = \cos pt \cdot \cos mz \left(\frac{y}{r^2},\ -\frac{x}{r^2},\ 0\right),$$

in which z is measured along the axis and $m = p/V$. These expressions are independent of r_1 and r_2, and thus the nodal distance corresponding to a given frequency of vibration is the same whatever may be the diameters of the cylinders. But although the relation of m to p is unchanged, as r_1 is supposed to be reduced, the corresponding energies increase and that without limit. Apart from the first two factors, the value of (*e. g.*) the resultant electromotive intensity varies as $1/r$; so that the integrated square is proportional to $\log (r_2/r_1)$. We see that when r_1 is reduced without limit, the phenomenon is ultimately dominated by the infinite energies associated with the immediate neighbourhood of the attenuated conductor. Moreover, when r_1 is already infinitesimal, its further reduction in a finite ratio causes only a vanishing relative change in the value of the energy of vibration.

In the problem for which the solution is above analytically expressed the section of the rod is circular and uniform, but the considerations already advanced point to the conclusion that when the section is infinitesimal neither the circularity nor the uniformity are essential. So long, at any rate, as all diameters, whether of the same section or of different sections, are in finite ratios to one another, the relation of nodal interval to frequency remains undisturbed.

The same line of argument further indicates that the conclusion may be extended to a terminated rod of infinitesimal section. For the infinite energy associated with the neighbourhood of the conductor is unaffected except at points infinitely near the ends. It appears therefore that the wave-length of the electrical vibration associated with a straight terminated rod of infinitesimal section, is equal to twice the length of the rod, whether the shape be cylindrical so that the radius is constant, or ellipsoidal so that the radius varies in a finite ratio at different points of the length. And

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* See, for example, Phil. Mag. xliv. p. 199 (1897); Scientific Papers iv. p. 327.

this conclusion still remains undisturbed, even though the shape be not one of revolution.

Whether the conditions of the limit can be sufficiently attained in experiment is a question upon which I am not prepared to express a decided opinion. From the logarithmic character of the infinity upon which the argument is founded, one would suppose that there might be practical difficulty in reducing the section sufficiently. Even if an adequate reduction were possible mechanically, the conductivity of actual materials might fail. We must remember that in the theory the conductivity is supposed to be *perfect*.

Terling Place, Witham,
      June 12, 1904.

XI. *The Rate of Decay of Thorium Emanation.* By C. LE ROSSIGNOL *and* C. T. GIMINGHAM[*].

DURING the investigation of a substance, giving an emanation which was suspected to be that of thorium, we had occasion to determine the time taken for known thorium emanation to decay to half-value, and finding that the value obtained was rather less than Prof. Rutherford's (Phil. Mag. 1900, xlix. p. 1), we carried out a series of experiments, which go to prove that the true value is 51 seconds, and not 60 as found by him.

Before going on to describe our method of measurement, it might be advantageous to give a short account of the apparatus employed (fig. 1).

The electrometer was of the Thomson-White pattern, with the bifilar suspension removed and replaced by a quartz fibre; the electrodes were insulated by means of ebonite coated

Fig. 1.
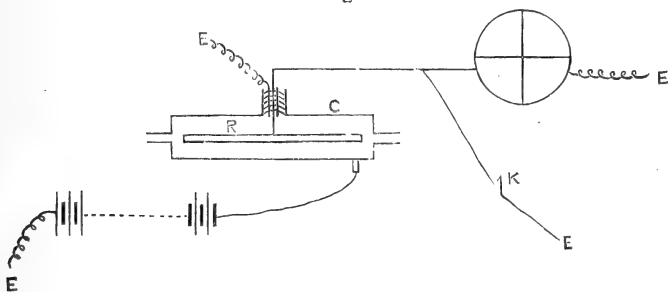
with shellac, and the instrument was practically dead-beat. A transparent celluloid scale was used, divided in millimetres;

[*] Communicated by Prof. F. T. Trouton, F.R.S.

and for a difference of potential of 1 volt between the quadrants, the spot of light travelled through 120 mms.

The apparatus was almost identical with that described by Prof. Rutherford; one pair of quadrants was permanently earthed while the other pair was connected to the central rod R of the emanation cylinder C, and so arranged that the quadrants could be earthed at will by means of the key K recommended by Rutherford. The rod was insulated from the external cylinder by a plug of shellac, into which a guard-ring connected to "earth" was inserted. After considerable trouble the apparatus was brought into such a condition that no sensible leak could be detected in five minutes, and consequently no correction was necessary for it in the present experiments.

The charge was proved to be proportional to the deflexion throughout the scale by placing some uranium oxide (giving a constant current) inside the cylinder and reading the deflexion at intervals of five seconds. On plotting these values against the time, a straight line was obtained.

A battery of test-tube accumulators was used to create a difference of potential of 300 volts between the central rod and the emanation cylinder.

Time in secs.	Scale-readings.	Time in secs.	Scale-readings.	Time in secs.	Scale-readings.	Time in secs.	Scale-readings.
	cms.		cms.		cms.		cms.
0	0	100	13·85	200	17·77	300	18·96
5	1·15	105	14·2	205	17·89	305	19·0
10	2·2	110	14·5	210	17·99	310	19·1
15	3·22	115	14·8	215	18·09	315	19·17
20	4·22	120	15·07	220	18·14	320	19·2
25	5·2	125	15·3	225	18·22	325	19·22
30	6·04	130	15·56	230	18·3	330	19·26
35	6·8	135	15·8	235	18·35	335	19·28
40	7·64	140	16·02	240	18·41	340	19·30
45	8·35	145	16·18	245	18·5	345	19·32
50	9·08	150	16·4	250	18·53	350	19·33
55	9·78	155	16·6	255	18·6	355	19·37
60	10·37	160	16·78	260	18·65	360	19·37
65	11·0	165	16·88	265	18·60	365	19·39
70	11·5	170	17·0	270	18·68	370	19·4
75	12·02	175	17·17	275	18·7	375	19·45
80	12·5	180	17·3	280	18·76	380	19·5
85	12·88	185	17·44	285	18·84	385	19·5
90	13·27	190	17·56	290	18·91	390	19·54
95	13·64	195	17·68	295	18·9	395	19·58
						400	19·6

Each experiment was carried out as follows:—A definite quantity of emanation was blown into the cylinder as quickly

as possible through a calcium-chloride drying-tube and the time noted. The cylinder was then closed, the earth connexion broken, and readings taken every 5 seconds over a period of 400 seconds; any residual emanation was immediately blown out, and the rate of charge due to the excited activity taken as quickly as possible. It was unnecessary to apply any correction for the lag of the needle, as it did not require more than 5 seconds to become steady.

An example will make the method clear.

After introducing the emanation, an operation which lasted 10 seconds, readings were begun 30 seconds later (see table, p. 108).

The emanation was then blown out, and the subsequent rate of charge, which was due solely to the induced activity developed during the experiment, was found to be ·65 cm. in 200 seconds.

Fig. 2
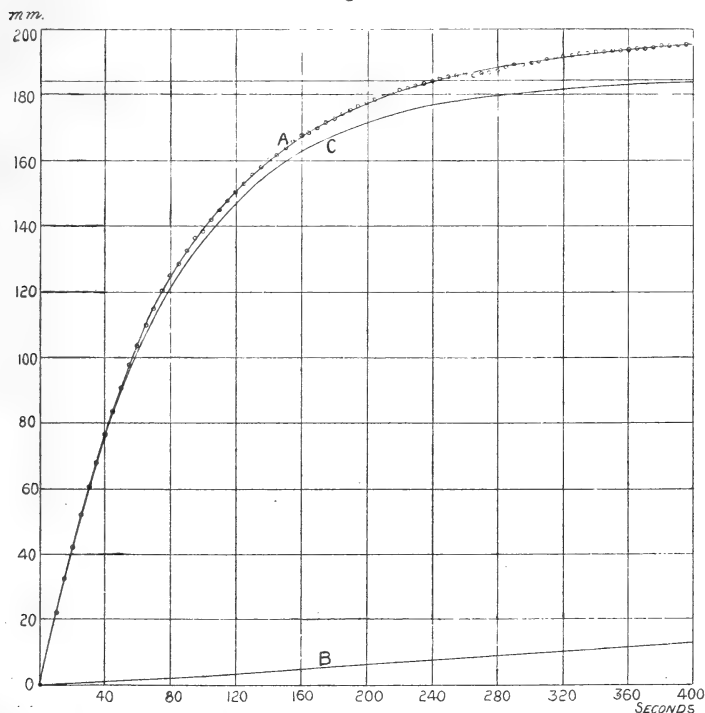
In the annexed diagram (fig. 2) the values given above are plotted against time and a smooth curve A drawn through the points, from which it will be seen that they are nearly all on the

curve. This gives the charge at any time due to the emanation together with the excited activity.

Now, knowing the time when the emanation was blown in, the excited activity at any time can be calculated, assuming an approximate rate of decay for the emanation, and from these values the curve B showing the charge due to the excited activity can be drawn.

If the ordinates of this curve be subtracted from the ordinates of the former, then the curve C drawn through the resulting points will show the charge due to the emanation alone. An asymptote was then drawn, and the logarithms of the ordinates measured from this to the curve were plotted and, from fig. 3, it will be seen that the points thus obtained lie on a straight line.

Fig. 3.
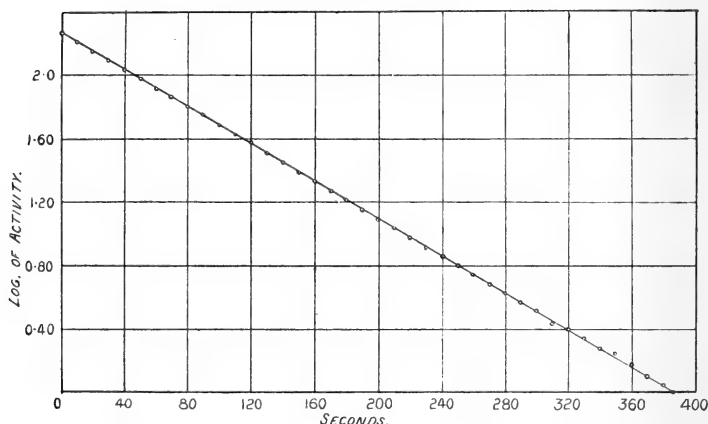
This proves the curve to be a true exponential. From this the time taken by the emanation to decay to half value was found to be 51·2 seconds. Other experiments gave very similar values, *e. g.* 50·8 and 51·5 seconds. The mean of these numbers gives 51·2 seconds as the time taken to decay to half value.

In conclusion we wish to express our thanks to Sir William Ramsay and Prof. Trouton for the kind interest they have taken in the work, and especially are we indebted to Mr. A. W. Porter for his valuable advice and assistance which were always at our disposal.

Physical Laboratory,
University College, London.

XII. *A Compact Arrangement for Reading off Deflexions (of Galvanometers, &c.), with the Possibility of Increasing the Sensitiveness without Increase of Space or Loss of Light. By* J. J. Taudin Chabot*.

THE light from the scale, 1, is received by the mirrors 2 and 3, the instrument mirror 4, the totally reflecting prism 5, and on passing through the telescope 6 reaches the eye of the observer.

The instrument mirror moves about xx as axis, while the mirrors 2 and 3 are maintained in continuous rotation about the same axis, and in such a manner that by means of double-reduction toothed gearing, x_1x_1, the mirror 2 is given n times (where n is an integer) the angular velocity of the mirror 3 about xx.

The mirrors 3 and 2 may be driven pneumatically by means of a foot bellows, the compressed air driving a small turbine wheel carried by the axle of the mirror 3 ; the speed need only be high enough to produce a continuous picture of
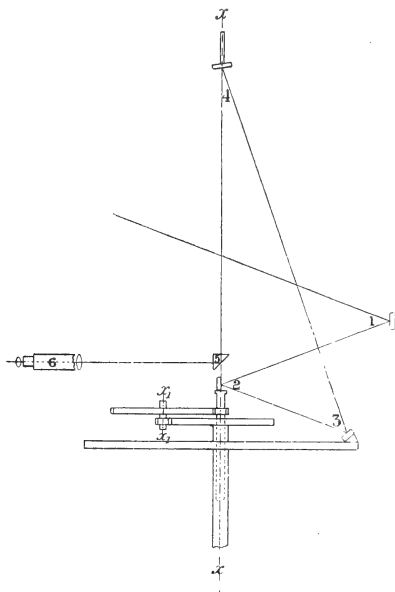
the scale by means of what are in reality intermittent flashes of light ; a small regulating stop-cock is for this purpose fitted to the air-supply pipe. The whole forms a very easily and

noiselessly running combination, which may also be driven
by means of a rubber ball pressed by hand, or by clockwork.

The action is as follows:—For each revolution of the
mirror 3 about the axis xx, the mirrors 2, 3, and 4 come into
optical conjunction only once, hence the light from the scale
can only once, during each revolution of 3, momentarily
reach the eye of the observer. Now when the instrument
mirror 4 is deflected through an angle a about the axis xx,
the position of momentary conjunction between the mirrors
4 and 3 is shifted through an angle a one way or the other,
depending on the direction of deflexion relatively to the
direction of rotation of the mirror 3 ; at the instant of this
conjunction, however, the mirror 2, which rotates n times
faster than the mirror 3, will be displaced forwards or back-
wards through an angle na, so that the light which is reflected
at 2, 3, and 4 must come from a part of the scale whose
angular distance is $2na$ one way or the other from the initial
reading.

The path of the ray from 2 to 3, 3 to 4, and 4 to 5,
evidently lies in the same plane, and this plane follows the
deflexion a of the instrument mirror 4 about the axis xx.

Thus a mere increase in the velocity ratio n of the mirrors
2 and 3 increases the sensitiveness of the arrangement, and
does so without any increase of space or loss of light.

Degerloch, Württemberg, February 29, 1904.

XIII. *Notices respecting New Books.*

Elektrische Fernphotographie und Ähnliches. Von Dr. ARTHUR
   KORN, *a. o. Professor an der Universität München.* Mit 13
   Figuren im Text. Leipzig: S. Hirzel. 1904. Pp. 66.

IN this pamphlet, the author gives an account of the extremely
   ingenious arrangements devised by him for obtaining synchronous
rotation of two cylinders at stations a considerable distance apart,
and of the transmitting and receiving instruments to be used in
connection therewith for the telegraphic transmission of photo-
graphs, manuscripts, drawings, &c. The pamphlet is prefaced by
an historical introduction, and concludes with a forecast of the
probable improvements to be effected in the future.

*Théorie de Maxwell et les Oscillations Hertziennes.—La Télégraphie
   sans Fil.* Par H. POINCARÉ. Paris: C. Naud. 1904 (*Scientia*
   Series, No. 23). Pp. vi + 110.

THE bulk of this volume has already appeared as No. 1 of the
Scientia series, but here we have important additions in the shape of
three chapters, one of which deals with coherers and magnetic
detectors, another with the principles of wireless telegraphy, and
a third with its practical applications. The volume is a most
readable and entirely non-mathematical account of the subject
with which it deals.

LOGARITHMS OF GALVANOMETER DEFLECTIONS.

TRANSPARENCY OF THE ATMOSPHERE FROM BOLOGRAPHIC OBSERVATIONS.

AIR MASSES.
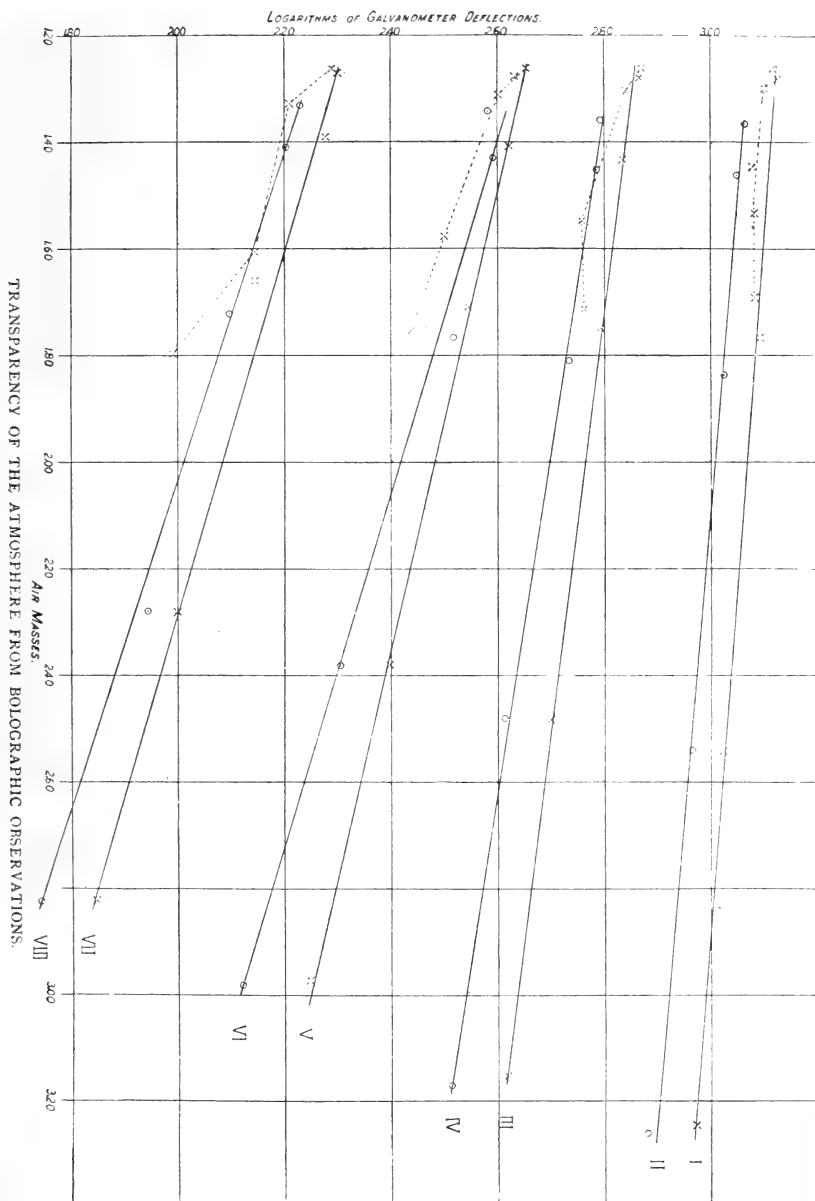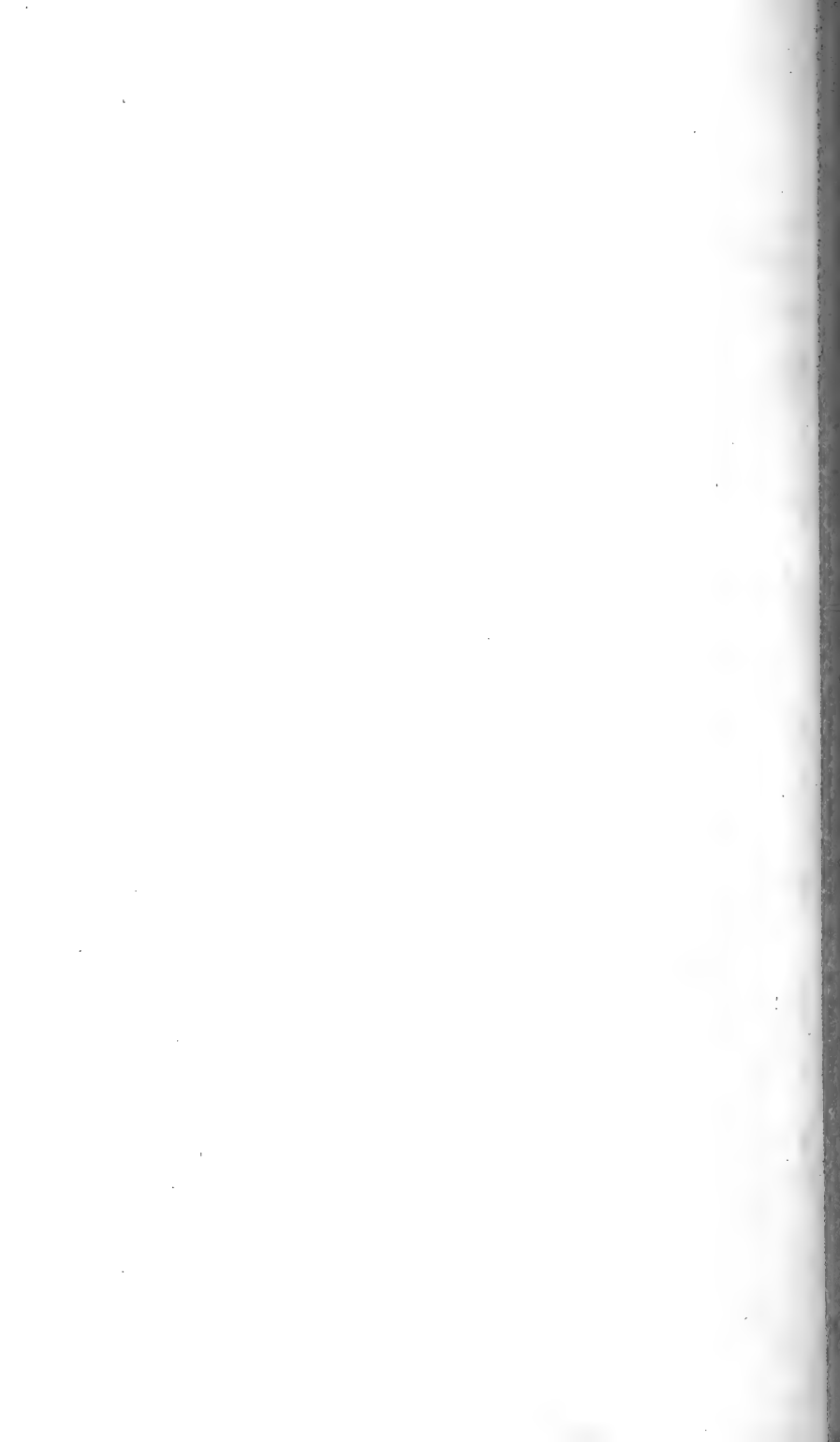
TEMPERATURE DEPARTURES.

NORTH AMERICA. 20 STATIONS.

AZORES, MADEIRAS, BRITISH ISLES, S.W. EUROPE, N. AFRICA. 18 STATIONS.

N.W. EUROPE. 15 STATIONS.

CENTRAL EUROPE. 10 STATIONS.

EUROPEAN RUSSIA. 11 STATIONS.

ASIATIC RUSSIA. 8 STATIONS

HIGH ALTITUDES S.W. EUROPE. 7 STATIONS

GENERAL MEAN NORTH TEMPERATE ZONE. 89 STATIONS.

SOLAR RADIATION OUTSIDE THE ATMOSPHERE.
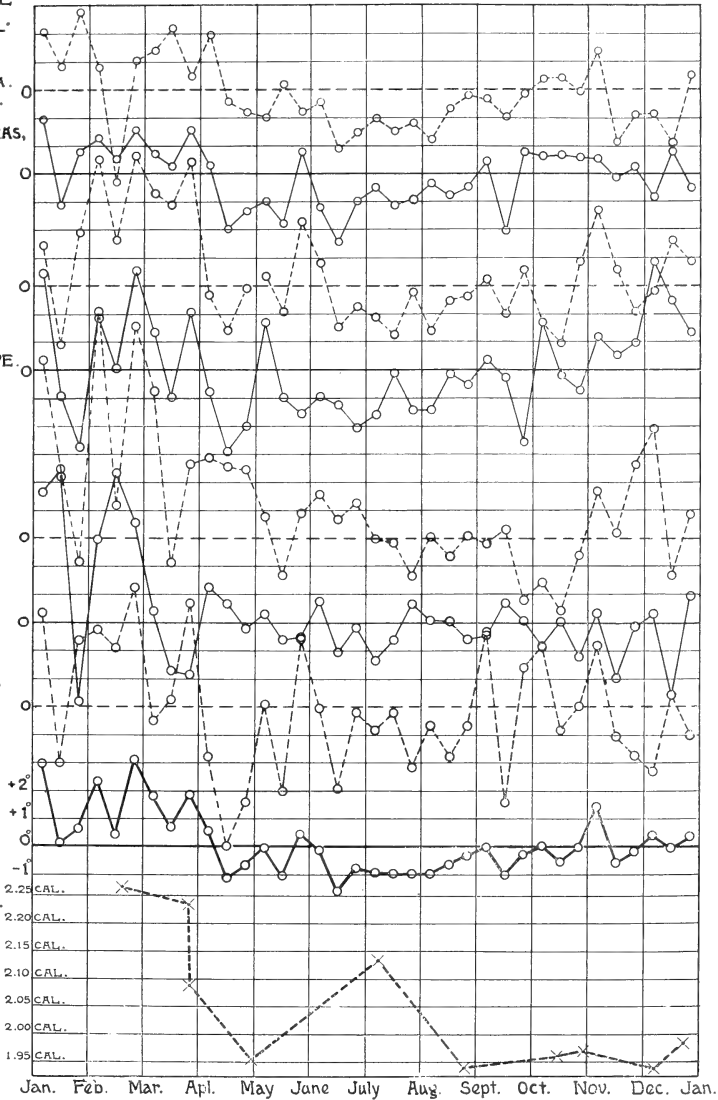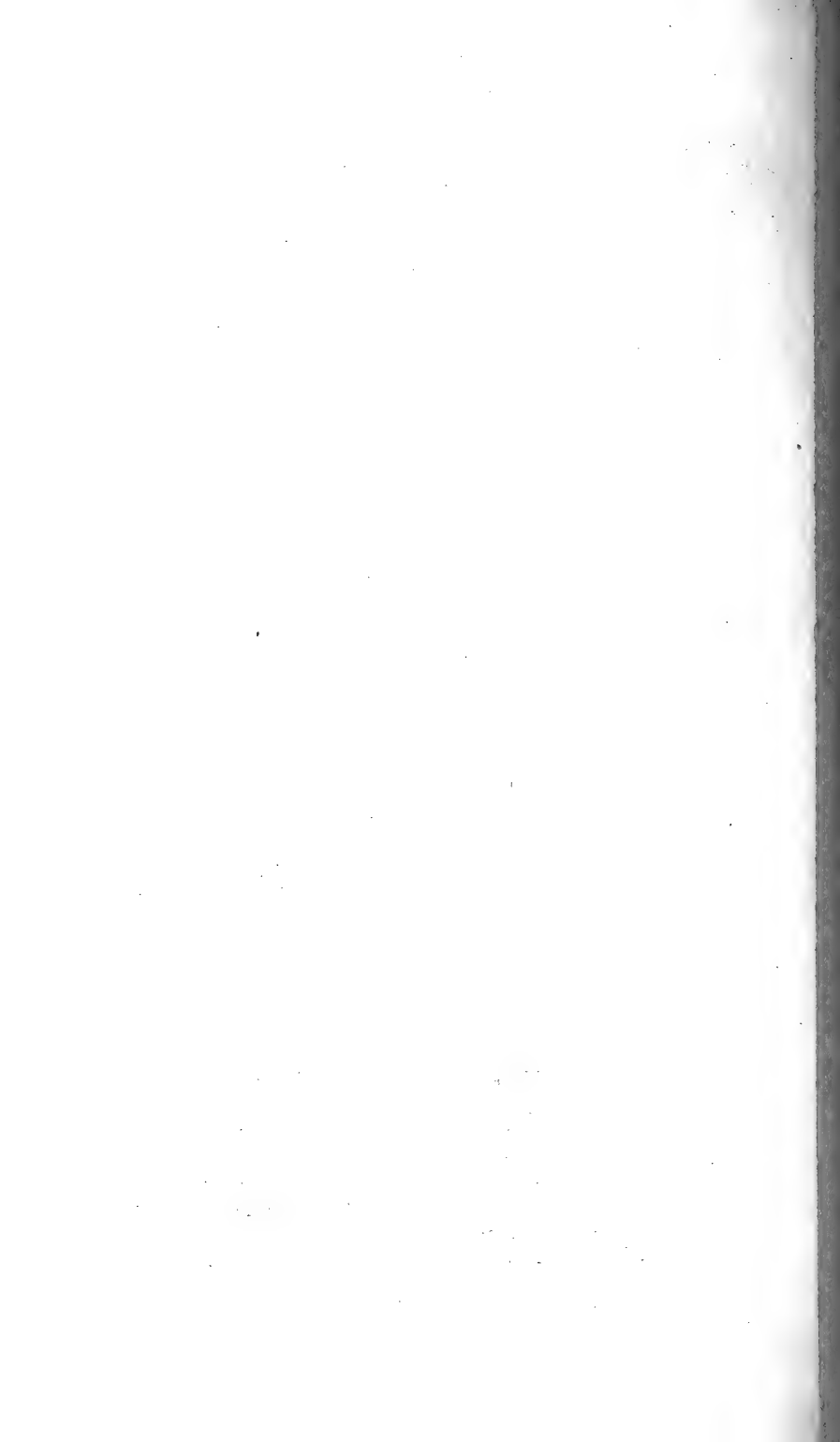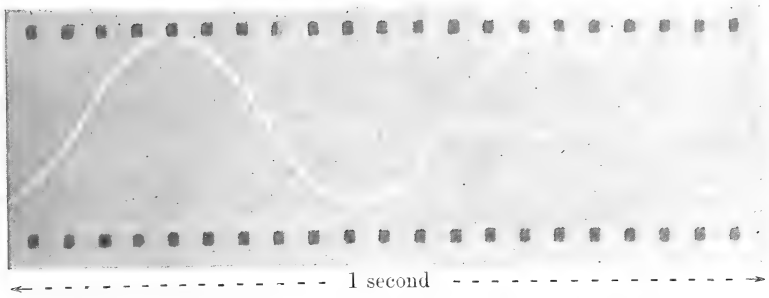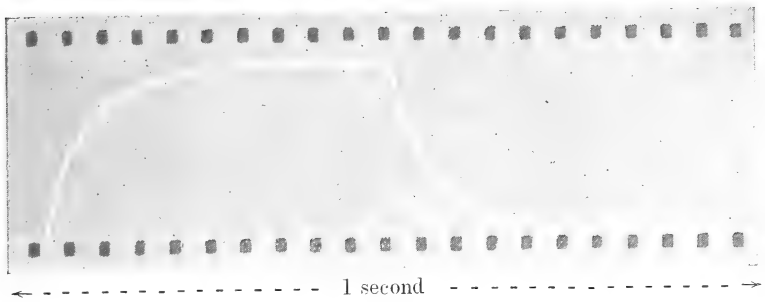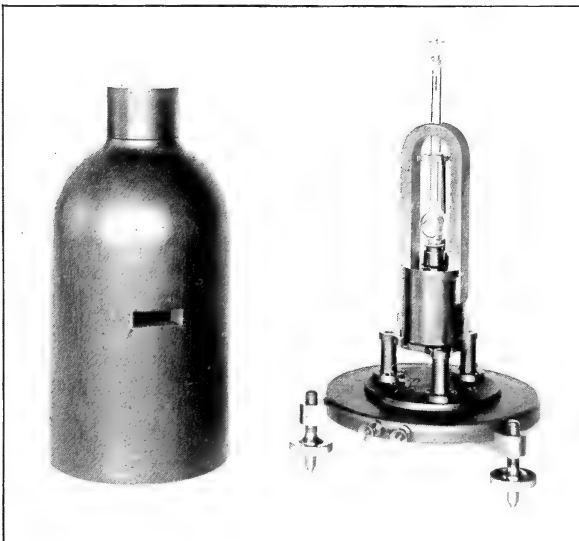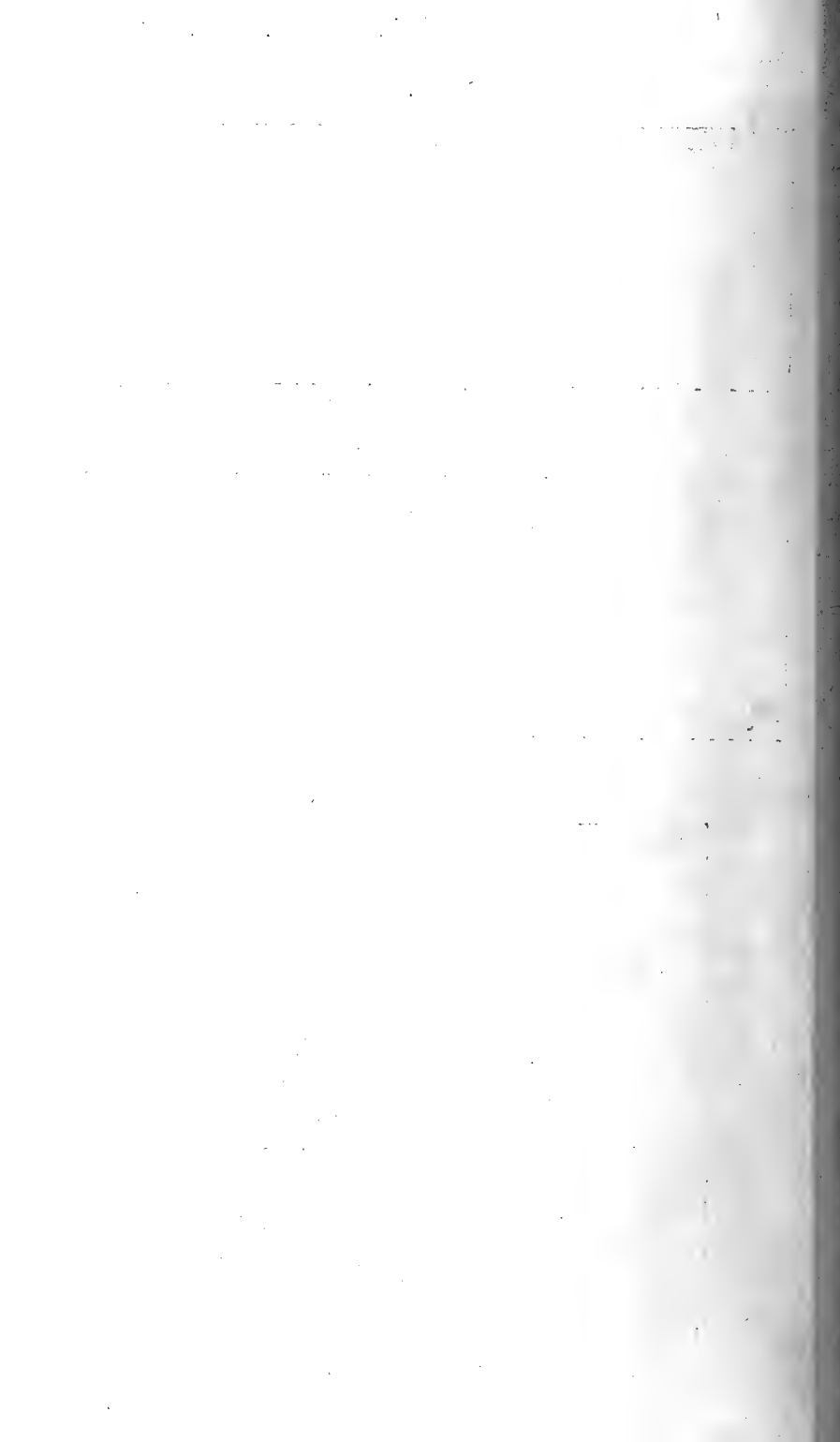
+2°
+1°
0°
-1°
2.25 CAL.
2.20 CAL.
2.15 CAL.
2.10 CAL.
2.05 CAL.
2.00 CAL.
1.95 CAL.

Jan. Feb. Mar. Apl. May June July Aug. Sept. Oct. Nov. Dec. Jan.

Fig. 2.

← - - - - - - - - - - - - - - 1 second - - - - - - - - - - - - - - - →

Fig. 3.

← - - - - - - - - - - - - - - 1 second - - - - - - - - - - - - - - →

Fig. 5.

Thermogalvanometer.

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XIV. *The Law of Action between Magnets and its bearing on the Determination of the Horizontal Component of the Earth's Magnetic Force with Unifilar Magnetometers. By* CHARLES CHREE, *Sc.D., LL.D., F.R.S. (From the National Physical Laboratory.)*[*]

CONTENTS.

§ 1. WHEN a freely suspended magnet is deflected out of the magnetic meridian by a second magnet, the two being at right angles to one another in the first Lamont's position, the following relation holds :—

$$\frac{2m}{r^3}\left(1 - \frac{2\mu}{r^3} - qt - q't^2\right)\left(1 + \frac{P}{r^2} + \frac{Q}{r^4} + \frac{R}{r^6} + \dots\right) = H \sin u. \quad (1)$$

Here m is the magnetic moment, at 0° C., of the deflecting magnet, μ its temporary induction coefficient, q and q' its

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temperature coefficients, r the true distance between the centres of the two magnets, H the horizontal component of the earth's magnetic force, u the deflexion-angle, and P, Q, R... constants depending on the dimensions of the two magnets and their distribution of magnetism.

In 1899 * I pointed out that the assumption usually made that all the higher constants Q, R .. are negligible did not seem fully justified for the ordinary Kew pattern magnetometer. Before investigating this point further, I shall explain how Horizontal Force observations may be carried out when Q is not negligible.

§ 2. Let us for shortness put

$$\tfrac{1}{2}r^3 \sin u \left(1 - \frac{2\mu}{r^3} - qt - q't^2\right)^{-1} = \mathrm{W}, \quad . \quad . \quad (2)$$

and employ suffixes 1, 2, 3 to refer to deflexions at three distances r_1, r_2, r_3; then from observations at these distances we have

$$\left.\begin{array}{l}(m/\mathrm{H})(1 + \mathrm{P}r_1^{-2} + \mathrm{Q}r_1^{-4}) = \mathrm{W}_1, \\ (m/\mathrm{H})(1 + \mathrm{P}r_2^{-2} + \mathrm{Q}r_2^{-4}) = \mathrm{W}_2, \\ (m/\mathrm{H})(1 + \mathrm{P}r_3^{-2} + \mathrm{Q}r_3^{-4}) = \mathrm{W}_3.\end{array}\right\} \quad . \quad . \quad . \quad (3)$$

The induction and temperature coefficients are supposed known, the angle u and the distances r are observed, thus the only unknowns in the above relations are m/H, P, and Q. Our primary object is to determine m/H. To do so, we eliminate P and Q in the usual way, and find

$$m/\mathrm{H} = \begin{vmatrix} \mathrm{W}_1 & r_1^{-2} & r_1^{-4} \\ \mathrm{W}_2 & r_2^{-2} & r_2^{-4} \\ \mathrm{W}_3 & r_3^{-2} & r_3^{-4} \end{vmatrix} \div \begin{vmatrix} 1 & r_1^{-2} & r_{1-4} \\ 1 & r_2^{-2} & r_2^{-4} \\ 1 & r_3^{-2} & r_{3-4} \end{vmatrix},$$

or

$$m/\mathrm{H} = \mathrm{A}_1\mathrm{W}_1 + \mathrm{A}_2\mathrm{W}_2 + \mathrm{A}_3\mathrm{W}_3, \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where $\quad \mathrm{A}_1 = r_1^4(r_3^2 - r_2^2)$

$$\div \{(r_1 - r_2)(r_2 - r_3)(r_3 - r_1)(r_1 + r_2)(r_2 + r_3)(r_3 + r_1)\}, \quad (5)$$

and A_2, A_3 may be written down from symmetry.

In reality, the distances r_1, r_2, r_3 vary slightly with the temperature; but assuming the deflexion-bar uniform, the distances all alter in the same proportion. Thus the coefficients A, being of dimensions 0 in length, are absolutely independent of temperature, and once calculated cause no further trouble.

If we were to work out a value for m/H from (4), employing values actually found for W_1, W_2, and W_3 on an individual occasion, slight errors in these values might produce undesirably large errors in the result. The above method, in short, though theoretically perfect, cannot be recommended for the separate treatment of individual day's observations. Let us suppose, however, that a large number of observations have been made at one station, with an instrument whose magnets may be regarded as of constant moment, and that the mean values of the W's so observed are denoted by $\overline{W}_1$, $\overline{W}_2$, and $\overline{W}_3$. Let us put

$$\left. \begin{aligned} A_1\overline{W}_1 + A_2\overline{W}_2 + A_3\overline{W}_3 = N, \\ \overline{A}_1 = N/3\overline{W}_1, \ \overline{A}_2 = N/3\overline{W}_2, \ \overline{A}_3 = N/3\overline{W}_3. \end{aligned} \right\} \quad . \quad . \quad (6)$$

If now W_1, W_2, W_3 refer to any individual set of observations,

$$(m/\text{H})_1 = 3\overline{A}_1 W_1,$$
$$(m/\text{H})_2 = 3\overline{A}_2 W_2,$$
$$(m/\text{H})_3 = 3\overline{A}_3 W_3$$

are three values for m/H given by the deflexions at the three distances r_1, r_2, r_3, after applying a correction for P and Q based on the whole series of observations, and

$$m/\text{H} = \overline{A}_1 W_1 + \overline{A}_2 W_2 + \overline{A}_3 W_3 \quad . \quad . \quad . \quad . \quad (7)$$

the arithmetic mean of the three values may be accepted.

If the coefficient R in (1) also required to be taken into account, a similar procedure would apply, observations being taken at four instead of three distances.

§ 3. There is always a certain advantage in observing at two distances, as this leads to the detection of errors of setting and reading which otherwise might escape notice. But observations at a third distance, whilst adding materially to the labour, provide little additional security against observational errors. It is thus desirable to consider whether it is really necessary always to observe at three distances when Q requires to be allowed for. Obviously this is not necessary if the magnets and the apparatus employed remain in an absolutely constant state, for then P and Q and $\overline{A}_1$, $\overline{A}_2$, $\overline{A}_3$ are absolute constants, and once determined remain available for all future use. It is also conceivable that whilst changes in P may be sufficiently large to be taken into account, there is no such necessity in the case of Q, in which event observations at two distances might suffice.

As to the actual formulæ for P and Q, if we suppose R negligible we easily find from (3)

$$P = \frac{W_1 r_1^4 (r_3^4 - r_2^4) + W_2 r_2^4 (r_1^4 - r_3^4) + W_3 r_3^4 (r_2^4 - r_1^4)}{W_1 r_1^4 (r_2^2 - r_3^2) + W_2 r_2^4 (r_3^2 - r_1^2) + W_3 r_3^4 (r_1^2 - r_2^2)}, \quad \cdot \quad \cdot \quad \cdot \quad (8)$$

$$Q = \frac{r_1^2 r_2^2 r_3^2 \{ W_1 r_1^2 (r_2^2 - r_3^2) + W_2 r_2^2 (r_3^2 - r_1^2) + W_3 r_3^2 (r_1^2 - r_2^2) \}}{W_1 r_1^4 (r_2^2 - r_3^2) + W_2 r_2^4 (r_3^2 - r_1^2) + W_3 r_3^4 (r_1^2 - r_2^2)}. \quad (9)$$

Here W_1, W_2, W_3 may represent results from a single observation, but it is highly desirable they should represent means from a large number of observations. Only those days' results should be used on which observations have been taken at all three distances.

It will be noticed that P being of dimensions r^2, and Q of dimensions r^4, they are not strictly speaking independent of the temperature. Thus, theoretically, there is an objection both to combining observations taken at different temperatures, and to treating the functions of the distances in (8) and (9) as constants. In practice, however, the contributions from P and Q to the value of H are so small that these objections may be disregarded.

The most satisfactory way of investigating the degree of variability of P and Q under ordinary conditions, would be to take observations at three distances at frequent intervals during several years, with the same instrument, and compare the results deduced by formulæ (8) and (9) from different groups of the observations. We shall presently consider existing evidence as to the variability of P and Q, and the question how these quantities are related to the dimensions and other peculiarities of the magnets employed. But before doing so we shall discuss the uncertainties in the determination of P and Q, and in the value of H, arising either from errors of observation or in the assumed values of the deflexion distances.

Effect of Errors in the Setting of the Deflecting Magnet or in the Values accepted for the Deflexion Distances.

§ 4. The temporary induction correction is not absolutely unaffected by errors in the deflexion distances, but assuming ordinary care any error so introduced is too small to require consideration. Thus, for the purposes of the present inquiry, we may leave induction and temperature corrections out of account. As will be seen later, the coefficient R is seldom likely to be of practical moment in the case of English magnetometers, so to avoid undue length in the formulæ and discussion I shall in general neglect it.

Combining the vibration and deflexion experiments, omitting temperature and induction corrections, we find

$$1 + Pr^{-2} + Qr^{-4} = \tfrac{1}{2}(H^2T^2/\pi^2I)r^3 \sin u,$$

where T denotes the time of a half vibration (rest to rest), and I the moment of inertia of the collimator magnet. The other letters have the same signification as before.

Assuming T and u correctly measured, we may for the present inquiry put

$$(T^2/2\pi^2I) \sin u = B, \quad . \quad . \quad . \quad . \quad . \quad (10)$$

and treat B as a constant. Also if B_1, B_2, B_3 refer to the three observation distances, we have as a first approximation (since Pr^{-2} and Qr^{-4} are always small compared with unity)

$$B_1r_1^3 = B_2r_2^3 = B_3r_3^3 = H^{-2}. \quad . \quad . \quad . \quad (11)$$

We can use (11) in *small* terms, *after* differentiation.

From three equations of the type

$$H^2B - Pr^{-5} - Qr^{-7} = r^{-3},$$

we find

$$H^2 = (r_2^2 - r_3^2)(r_3^2 - r_1^2)(r_1^2 - r_2^2) \div D, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

$$-P = \{r_1^7(r_3^4 - r_2^4)B_1 + r_2^7(r_1^4 - r_3^4)B_2 + r_3^7(r_2^4 - r_1^4)B_3\} \div D, \quad . \quad . \quad (13)$$

$$Q = r_1^2 r_2^2 r_3^2 \{r_1^5(r_3^2 - r_2^2)B_1 + r_2^5(r_1^2 - r_3^2)B_2 + r_3^5(r_2^2 - r_1^2)B_3\} \div D, \quad (14)$$

where

$$D \equiv r_1^7(r_3^2 - r_2^2)B_1 + r_2^7(r_1^2 - r_3^2)B_2 + r_3^7(r_2^2 - r_1^2)B_3. \quad . \quad (15)$$

Supposing r_1 alone to vary, we find from (12) and (15)

$$\frac{2}{H}\frac{dH}{dr_1} = \frac{2r_1}{r_1^2 - r_2^2} + \frac{2r_1}{r_1^2 - r_3^2}$$
$$- \frac{7r_1^6(r_3^2 - r_2^2)B_1 + 2r_1r_2^7B_2 - 2r_1r_3^7B_3}{r_1^7(r_3^2 - r_2^2)B_1 + r_2^7(r_1^2 - r_3^2)B_2 + r_3^7(r_2^2 - r_1^2)B_3}.$$

In this use $B_1 : B_2 : B_3 :: r_1^{-3} : r_2^{-3} : r_3^{-3}$ from (11), and after reduction we find

$$\frac{2}{H}\frac{dH}{dr_1} = -3r_1^3 \div \{(r_1^2 - r_2^2)(r_1^2 - r_3^2)\}.$$

This will sufficiently illustrate the mathematical operations necessary in obtaining the following results.

Supposing $\delta r_1, \delta r_2, \delta r_3$ small independent increments in r_1, r_2, r_3, we find the corresponding increments in H, P,

and Q given by

$$\delta H/H = (3/2\Delta)\{(\delta r_1/r_1)r_1^4(r_2^2 - r_3^2) + (\delta r_2/r_2)r_2^4(r_3^2 - r_1^2)$$
$$+ (\delta r_3/r_3)r_3^4(r_1^2 - r_2^2)\}, \quad \textbf{(16)}$$

$$\delta P = (3/\Delta)\{\delta r_1/r_1)r_1^4(r_2^4 - r_3^4) + (\delta r_2/r_2)r_2^4(r_3^4 - r_1^4)$$
$$+ (\delta r_3/r_3)r_3^4(r_1^4 - r_2^4)\}, \quad \textbf{(17)}$$

$$\delta Q = -3(r_1^2 r_2^2 r_3^2/\Delta)\{(\delta r_1/r_1)r_1^2(r_2^2 - r_3^2) + (\delta r_2/r_2)r_2^2(r_3^2 - r_1^2)$$
$$+ (\delta r_3/r_3)r_3^2(r_1^2 - r_2^2)\}, \quad \textbf{(18)}$$

where

$$\Delta = (r_1^2 - r_2^2)(r_2^2 - r_3^2)(r_3^2 - r_1^2). \quad . \quad . \quad \textbf{(19)}$$

We shall suppose r_1 the smallest, and r_3 the largest of the three distances.

If δr_1, δr_2, δr_3 are the corrections required to the values accepted for r_1, r_2, and r_3, then δH, δP, and δQ are the consequent corrections necessary to the values calculated for H, P, and Q. Considering, for example, the case where a correction is necessary to only one of the distances, say r_1, we find

$$\delta H/H = \tfrac{1}{2}\delta P/(r_2^2 + r_3^2) = -\tfrac{1}{2}\delta Q/r_2^2 r_3^2$$
$$= -\tfrac{3}{2}(\delta r_1/r_1)r_1^4 \div \{(r_1^2 - r_2^2)(r_1^2 - r_3^2)\}. \quad \textbf{(20)}$$

The corrections to H, P, and Q thus depend not merely on the size of δr_1 and on r_1, but also—and that most materially—on the size of r_2 and r_3. In particular, if either or both of the quantities $r_1 \sim r_2$ and $r_1 \sim r_3$ is small, a slight error in r_1 may be decidedly serious.

§ 5. To see the full significance of this result, let us consider the formulæ which take the place of (16) when

(i.) P and Q are both negligible and observations are taken at the single distance r_1;

(ii.) Q is negligible, and P is eliminated by observations at the two distances r_1 and r_2.

The formulæ in question are

for (i.) $\delta H/H = -(3/2)\delta r_1/r_1$, ... (21)

(ii.) $\delta H/H = \dfrac{3}{2}\left\{ \dfrac{\delta r_1}{r_1} \dfrac{r_1^2}{r_2^2 - r_1^2} - \dfrac{\delta r_2}{r_2} \dfrac{r_2^2}{r_2^2 - r_1^2} \right\}$.   **(22)**

Comparing (21) and (22) with (16), we see that the errors in H answering to the same error in r_1 stand to one another in the following ratio :—

P and Q negligible.	Q negligible.	Neither negligible.
1	$r_1^2/(r_1^2 - r_2^2)$	$\{r_1^2/(r_1^2 - r_2^2)\}\{r_1^2/(r_1^2 - r_3^2)\}$

When we observe at a single distance, as (21) shows, an error of 1 part in 10,000 in r entails an error of 1·5 parts in 10,000 in H, whatever r may be.

This relation is so simple that it is often convenient to make it the point of departure when considering the effect of errors in the distance when P, or P and Q, are retained, applying multipliers answering to the ratios considered above.

The following results for specified cases will serve for illustration.

<div align="center">TABLE I.</div>

Distances one, two, three of the following : 22·5, 30, 40 cms.

For given error in	Observations at						
	Single distance.			Two distances.			Three distances.
	22·5	30	40	22·5 & 30	22·5 & 40	30 & 40	22·5, 30 & 40 cms.
22·5...	1	...	...	−1·29	−0·46	...	+0·60
30·0...	...	1	...	+2·29	...	−1·29	−2·94
40·0...	...	...	1	...	+1·46	+2·29	+3·34

The Table is to be interpreted as follows :—

If a certain error in the value accepted for the distance 22·5 cms. produces an error of $e^* \times 1$ in the value of H when P and Q are negligible, and observations are made at 22·5 cms. only, then if observations are taken at two distances, and combined in the usual way, the error is $-1·29e$ or $-0·46e$, according as the distances are 22·5 and 30, or 22·5 and 40 cms., while if P and Q are eliminated from observations at 22·5, 30, and 40 cms. the error is $+0·60e$. As a concrete instance, suppose a correction of $+·003$ cm. necessary to the value assigned the 30 cms. distance, the local value of H being ·186 c.g.s. Then the value deduced for H by employing the erroneous value for the 30 cms. requires to be *diminished* by

$$(3/2) \times (0·003/30) \times ·186 = 2·79\gamma \text{ (where } 1\gamma \equiv 1 \times 10^{-5} \text{ c.g.s.)},$$

when observations at 30 cms. suffice, P and Q being negligible. When, however, Q only is negligible, the value requires to be

> *diminished* by $2·29 \times 2·79$, or $6·4\gamma$, when observations are made at 22·5 and 30 cms.,
> *increased* by $1·29 \times 2·79$, or $3·6\gamma$, when observations are made at 30 and 40 cms.

* When we observe at a single distance the errors in the values accepted for the distance and calculated for H are really opposite in sign.

When neither P nor Q is negligible, and they have to be eliminated by observations at 22·5, 30, and 40 cms., the value of H requires to be *increased* by 2·94 × 2·79 or 8·2γ.

§ 6. Other likely distances are dealt with in Table II., which is to be interpreted on similar lines to Table I. All distances are in centimetres.

TABLE II.

Distances one, two, or three of the following :—

For given error in	Single distance.			Two distances.			Three distances.
	26·25	35	45	26·25 & 35	26·25 & 45	35 & 45	26·25, 35, & 45
26·25 .	1	...	...	−1·29	−0·52	...	+0·66
35 ..	...	1	...	+2·29	...	−1·53	−3·50
45 ...	...	...	1	...	+1·52	+2·53	+3·84
	25	35	45	25 & 35	25 & 45	35 & 45	25, 35, 45
25 ...	1	...	...	−1·04	−0·45	...	+0·47
35 ...	...	1	...	+2·04	...	−1·53	−3·13
45 ...	...	...	1	...	+1·45	+2·53	+3·66
	30	35	40	30 & 35	30 & 40	35 & 40	30, 35, 40
30 ...	1	...	...	−2·77	−1·29	...	+ 3·56
35 ...	...	1	...	+3·77	...	−3·27	−12·31
40 ...	...	...	1	...	+2·29	+4·27	+ 9·75

The results in Tables I. and II. depend in reality only on the *ratios* of the distances, not on their absolute values.

The last combination dealt with in Table II. exemplifies what happens when the distances $r_1 \sim r_2$ and $r_2 \sim r_3$ are comparatively small. It clearly cannot be recommended when observations are to be taken at three distances. Unless Q were exceptionally large, it is clear that a trifling error in the 35 cms. distance might more than neutralize the advantages to be derived from not neglecting Q.

§ 7. When observations are made at three distances, and more than one of them is in error, the following Table will be found convenient. In it δH, δr_1, δr_2, δr_3 are all corrections, or else all errors.

TABLE III.

Values of			Value of $\delta H/H$.		
r_1 cms.	r_2 cms.	r_3 cms.			
22·5	30	40	$-0{\cdot}0397\ \delta r_1$	$+{\cdot}1469\ \delta r_2$	$-{\cdot}1254\ \delta r_3$
25	35	45	$-0{\cdot}0279\ \delta r_1$	$+{\cdot}1340\ \delta r_2$	$-{\cdot}1220\ \delta r_3$
26·25	35	45	$-0{\cdot}0379\ \delta r_1$	$+{\cdot}1500\ \delta r_2$	$-{\cdot}1279\ \delta r_3$
27	36	48	$-0{\cdot}0331\ \delta r_1$	$+{\cdot}1224\ \delta r_2$	$-{\cdot}1045\ \delta r_3$
30	35	40	$-0{\cdot}178\ \delta r_1$	$+{\cdot}528\ \ \delta r_2$	$-{\cdot}366\ \ \delta r_3$

In Table III. the unit of length is 1 cm., the unit of force is immaterial.

At a particular station it would be simplest to use an approximate value for H, and it might be most generally convenient to take ·001 cm. for unit of length, and 1γ for unit of force. This is done in Table IV., applicable to a station where H is approximately ·185 c.g.s.

TABLE IV.

Values of			Value of δH. (H$=$·185 c.g.s.) (·001 cm. unit of length, 1γ unit of force.)		
r_1 cms.	r_2 cms.	r_3 cms.			
22·5	30	40	$-0{\cdot}73\ \delta r_1$	$+2{\cdot}72\ \delta r_2$	$-2{\cdot}32\ \delta r_3$
25	35	45	$-0{\cdot}52\ \delta r_1$	$+2{\cdot}48\ \delta r_2$	$-2{\cdot}26\ \delta r_3$
26·25	35	45	$-0{\cdot}70\ \delta r_1$	$+2{\cdot}77\ \delta r_2$	$-2{\cdot}37\ \delta r_3$
27	36	48	$-0{\cdot}61\ \delta r_1$	$+2{\cdot}26\ \delta r_2$	$-1{\cdot}93\ \delta r_3$
30	35	40	$-3{\cdot}29\ \delta r_1$	$+9{\cdot}76\ \delta r_2$	$-6{\cdot}76\ \delta r_3$

At a station where H$=$·370 c.g.s. the coefficients of δr_1, δr_2, δr_3 in Table IV. would be simply doubled.

§ 8. If in (16) we suppose

$$\delta r_3/r_3 = \delta r_2/r_2 = \delta r_1/r_1, \quad . \quad . \quad . \quad . \quad . \quad (23)$$

or if in (22) we suppose

$$\delta r_2/r_2 = \delta r_1/r_1,$$

we alike find

$$\delta H/H = -\tfrac{3}{2}\,\delta r_1/r_1.$$

Thus if all the distances have errors proportional to the true lengths—as might, for instance, occur through an error as to the coefficient of thermal expansion—the simple relation (21) applies irrespective of the number of points at which deflexions are made.

§ 9. The corrections δP, δQ to the values of P and Q, when corrections δr_1, δr_2, δr_3 are applied to the distances r_1, r_2, r_3 at which deflexions are made, are given in Table V. for the same combinations of three distances as appear in Table IV.

<div align="center">

TABLE V.

(Unit of length 1 cm.)

</div>

r_1 cms.	r_2 cms.	r_3 cms.	δP.	$10^{-3}\,\delta Q$.
22·5	30	40	$- 198\,\delta r_1 + 619\,\delta r_2 - 353\,\delta r_3$	$114\,\delta r_1 - 238\,\delta r_2 + 114\,\delta r_3$
25	35	45	$- 181\,\delta r_1 + 710\,\delta r_2 - 452\,\delta r_3$	$138\,\delta r_1 - 339\,\delta r_2 + 187\,\delta r_3$
26·25	35	45	$- 246\,\delta r_1 + 814\,\delta r_2 - 490\,\delta r_3$	$188\,\delta r_1 - 419\,\delta r_2 + 216\,\delta r_3$
27	36	48	$- 238\,\delta r_1 + 743\,\delta r_2 - 423\,\delta r_3$	$197\,\delta r_1 - 411\,\delta r_2 + 197\,\delta r_3$
30	35	40	$-1006\,\delta r_1 + 2638\,\delta r_2 - 1554\,\delta r_3$	$698\,\delta r_1 - 1520\,\delta r_2 + 806\,\delta r_3$

Here again we see the large effect of even small errors in the three distances when the intervals are only 5 cms. When $r_1/r_2 = r_2/r_3 = 3/4$, the coefficients of δr_1 and δr_3 in the expression for δQ are absolutely equal.

A check on the results in Table V. and analogous Tables is supplied by the fact that if

$$\delta r_1/r_1 = \delta r_2/r_2 = \delta r_3/r_3,$$

then we ought to have

$$\delta P = 0 = \delta Q.$$

This is obvious from the general formulæ (17) and (18) applicable to three distances (and it is equally true for deflexions at two distances only, when Q is negligible, though this case is not covered by Table V.).

Additional interest attaches to this simple result from the fact pointed out in a recent paper [*], that in ordinary English magnetometers the increments in the distances r_1 &c. due to bending of the deflexion-bar are nearly proportional to the distances themselves. The neglect of the bending effect thus entails fortunately but little error in the determination of P or Q.

<div align="center">

Effect of Errors in Angles.

</div>

§ 10. When a small error—whether due to reading or to defective graduation—has been made in a deflexion-angle u, the consequent small error δH is given by the following formulæ :—

[*] Phil. Mag. Jan. 1904, p. 39.

(i.) Observation at single distance r_1, P and Q negligible.

$$\delta H/H = -\tfrac{1}{2} \cot u_1 \,\delta u_1 ; \quad \ldots \quad (24)$$

(ii.) Observations at two distances r_1, r_2, Q negligible,

$$\delta H/H = \tfrac{1}{2}\{r_1{}^2 \cot u_1 \,\delta u_1 - r_2{}^2 \cot u_2 \,\delta u_2\}/(r_2{}^2 - r_1{}^2) : \quad (25)$$

(iii.) Observations at three distances r_1, r_2, r_3, neither P nor Q negligible,

$$\delta H/H = \frac{1}{2\Delta}\{r_1{}^4(r_2{}^2 - r_3{}^2)\cot u_1 \,\delta u_1 + r_2{}^4(r_3{}^2 - r_1{}^2)\cot u_2 \,\delta u_2$$

$$+ r_3{}^4(r_1{}^2 - r_2{}^2)\cot u_3 \,\delta u_3\} ; \quad \ldots \quad (26)$$

where Δ is given by (19), and the suffixes distinguish the three distances.

These formulæ are deducible from the formulæ for the effects on H of errors in the distances (*e.g.* (16)) by writing $\cot u_1 \,\delta u_1$ for $3\delta r_1/r_1$ &c. Thus the way in which error in an angle modifies the value of H according as observations are made at one, or at two, or at three distances, is exactly the same as in the corresponding case of error in a distance, and Tables I. and II. apply equally to the case of errors in angles.

A complication arises, however, when we come to consider the *absolute* size of the error in H. This depends partly on the size of the angle, *i.e.* on the strength of the deflecting magnet. The stronger this magnet, the larger the deflexion-angle, and the smaller the effect of a given error in reading or graduation. Tables VI., VII., and VIII. give some numerical results for two different values of H, and two different strengths of collimator magnet. The error in the angle is supposed to be 1′, the consequent error in H is given in terms of 1γ as unit. The sign + or − is attached according as the corrections to be applied to the angle and to H agree or differ in sign. It should be noticed that the value of a deflexion-angle is usually based on four readings, so that it would require an error of 4′ in a single reading to produce an error of 1′ in the angle.

Table VI.

Error of 1′ in deflexion-angle. Effect on H (unit 1γ).
Observation at a single distance, P and Q negligible.

H C.G.S.	m C.G.S.	Distances 22·5 Sign ...... −	25 −	26·25 −	30 −	35 −	40 −	45 cms. −
·185	1000	....	2·8	3·6	6·2	10·3	15·7	22·5
	750	.... 2·7	4·4	5·4	8·5	14·0	21·1	30·1
·370	1000	.... 10·0	14·6	17·2	26·3	42·3	63·4	90·6
	750	.... 14·1	20·1	23·4	35·4	56·7	84·8	120·8

When the magnetic moment is 1000 c.g.s., 22·5 cms. is too short a distance unless H exceeds ·185.

Table VII.

Error of 1′ in deflexion-angle.　Effect on H (unit 1γ).
Observations at two distances, Q negligible.

H C.G.S.	m C.G.S.	Distances Sign	22·5 +	30 −	25 +	35 −	26·25 +	35 −	30 +	40 −	35 +	45 cms. −
·185	1000	....	...	...	2·9	21·1	4·6	23·6	7·9	35·9	15·8	57·0
	750	....	3·4	19·5	4·6	28·5	6·9	31·9	11·0	48·1	21·4	76·3
·370	1000	....	12·8	60	15·2	86	22·1	97	33·9	145	...	...
	750	....	18·2	81	20·9	116	30·1	130	45·6	194	...	...

Table VIII.

Error of 1′ in deflexion-angle.　Effect on H (unit 1γ).
Observations at three distances, neither P nor Q negligible.

H C.G.S.	m C.G.S.	Distances Sign	22·5 −	30 +	40 −	25 −	35 +	45 −	26·25 −	35 +	45 −	30 −	35 +	40 −
·185	1000	....	...	...	...	1·3	32	82	2·4	36	86	22	127	153
	750	.:.......	1·6	25	70	2·1	44	110	3·6	49	116	30	172	205
·370	1000	....	5·9	77	212	6·8	132	332	11·4	148	348	...	...	...
	750	....	8·4	104	283	9·3	177	442	15·5	198	464	...	...	...

As an illustration of how to read the Tables, suppose that when a deflexion is made at 30 cms. with a magnet of moment 750, at a station where H is ·185, the angle is taken 1′ too high ; then the *error* in the value calculated for H is :

—8·5γ when P and Q are negligible, and the 30 cms. observation is alone used ;

—19·5γ when Q is negligible, and observations are taken at 30 cms. and 22·5 cms. ;

+11·0γ when Q is negligible, and observations are taken at 30 cms and 40 cms. ;

+25γ when neither P nor Q is negligible, and observations are taken at 22·5, 30, and 40 cms. ;

—30γ when neither P nor Q is negligible, and observations are taken at 30, 35, and 40 cms.

So long as the errors are small, the effect on H is proportional to the size of the error, and the results in Tables VI.,

VII., and VIII. have only to be multiplied by the error (in minutes) to give the error in H in terms of 1γ as unit.

§ 11. Tables VI., VII., and VIII. give results only for two specified values of H and of m. Approximate values are, however, easily deducible for other values of H and m from the following considerations: For any given combination of distances, the relation between the error in u and the consequent error in H is of the form

$$\delta H = f(r)\delta u \cdot \frac{\cos u}{\sin u} \cdot H,$$

where $f(r)$ depends only on the distances. Also, as a first approximation, we have

$$\sin u = 2m/Hr^3 \; ;$$

and so in the above equation

$$\delta H = \tfrac{1}{2}r^3 f(r)\delta u \cdot \cos u \cdot H^2/m . \quad . \quad . \quad . \quad (27)$$

Now, in practice, the departure of $\cos u$ from unity is seldom great. For $u = 20°$ we have $\cos u = 0\cdot94$, and except at the shortest distance of deflexion, angles as large as $20°$ are unusual. Thus, treating δu as given, we may accept

$$\delta H \propto H^2/m$$

as giving a good approximation at all the larger distances of deflexion, and a fair approximation even at the least distance, unless H is exceptionally low.

According to this approximation, the corrections to H in Tables VI., VII., and VIII., for a given value of H, when $m = 1000$ and when $m = 750$ should be in the proportion $3:4$; and the corrections to H, for a given value of m, should be four times as great when $H = \cdot370$ as when $H = \cdot185$. At the larger distances in the Tables, it will be seen that these relations are very nearly satisfied indeed.

§ 12. When one or more of the higher constants Q, R is not really negligible, the value found for P in the usual way from observations at two distances applies in reality to a different quantity, which we may call P'. As to the relation between P and P', let us suppose that observations are taken at the two distances r_1, r_2, and that P, Q, R are the only constants requiring to be taken into account.

We clearly have from (1)

$$1 + P'r_1^{-2} = 1 + Pr_1^{-2} + Qr_1^{-4} + Rr_1^{-6},$$
$$1 + P'r_2^{-2} = 1 + Pr_2^{-2} + Qr_2^{-4} + Rr_2^{-6};$$

whence

$$P' = P + Q(r_1^{-2} + r_2^{-2}) + R(r_1^{-4} + r_1^{-2}r_2^{-2} + r_2^{-4}). \quad (28)$$

Table IX. gives the form taken by (28) for a number of combinations of distances.

<div align="center">TABLE IX.</div>

r_1 cms.	r_2 cms.	Expression for P'.
18	24	$P + 4822Q \times 10^{-6} + 1790R \times 10^{-8}$
22·5	30	$P + 3086Q \times 10^{-6} + 733R \times 10^{-8}$
22·5	40	$P + 2600Q \times 10^{-6} + 553R \times 10^{-8}$
25	35	$P + 2416Q \times 10^{-6} + 453R \times 10^{-8}$
25	45	$P + 2094Q \times 10^{-6} + 359R \times 10^{-8}$
26·25	35	$P + 2268Q \times 10^{-6} + 396R \times 10^{-8}$
26·25	45	$P + 1945Q \times 10^{-6} + 307R \times 10^{-8}$
30	40	$P + 1736Q \times 10^{-6} + 232R \times 10^{-8}$
35	45	$P + 1310Q \times 10^{-6} + 131R \times 10^{-8}$

§ 13. When Q and R have wrongly been assumed negligible, the consequent effect on H may be found as follows :—

Assuming coefficients of higher terms negligible, we have

$$H^2 = A(1 + Pr^{-2} + Qr^{-4} + Rr^{-6}), \quad . \quad . \quad (29)$$

where r is a distance of deflexion, and A is independent of P, Q, or R. Supposing δH an increment to H, answering to increments δP, δQ, and δR, then

$$2\delta H/H = (r^{-2}\delta P + r^{-4}\delta Q + r^{-6}\delta R)/(1 + Pr^{-2} + Qr^{-4} + Rr^{-6}).$$

Unless r is unduly small, or the constants exceptionally large, we may replace the denominator on the right-hand side of the equation by unity. If, then, H has been originally determined from observations at r_1 and r_2, we have

$$2\delta H/H = r_1^{-2}\delta P + r_1^{-4}\delta Q + r_1^{-6}\delta R = r_2^{-2}\delta P + r_2^{-4}\delta Q + r_2^{-6}\delta R,$$

whence, eliminating δP,

$$2\delta H/H = -\delta Q r_1^{-2} r_2^{-2} - \delta R r_1^{-2} r_2^{-2}(r_1^{-2} + r_2^{-2}). \quad (30)$$

If we now suppose H to be the value found when Q and R were neglected, $H + \delta H$ to be the correct value when Q and R are assigned their true values, then we have to substitute Q for δQ and R for δR in (30), and so find

$$\delta H/H = -\frac{1}{2}\frac{Q}{r_1^2 r_2^2} - \frac{1}{2}\frac{R}{r_1^2 r_2^2}\left(\frac{1}{r_1^2} + \frac{1}{r_2^2}\right). \quad . \quad (31)$$

Here δH is the *correction* which is necessary to the value of H obtained in the usual way by neglecting Q and R. It is assumed that the correction is but a small fraction of H.

The result (31) may also be obtained by consideration of the quantities P and P'.

Nature of Coefficients P, Q, R.

§ 14. The law of action between two magnets in Lamont's first position—the position met with in deflexion experiments—was investigated by Lamont himself, and more recently with greater completeness by Dr. Börgen[*]. Let us first suppose the cross sections of the magnets negligible, and that their axes lie in the same horizontal plane, and are strictly perpendicular to one another. Let $d\mu$, $d\mu'$ be quantities of "magnetic matter" in the deflecting and deflected magnets, at distances x, x' from their centres, and let r be the distance between the centres. Then the couple L arising from the action of the one magnet on the other is given by

$$L = \iint (r-x)x' \{(r-x)^2 + x'^2\}^{-\frac{3}{2}} d\mu \, d\mu',$$

where the integration extends throughout both magnets.

Using the Lamont-Börgen notation

$$\int x d\mu = M, \qquad \int x' d\mu' = M',$$
$$\int x^i d\mu = M_i, \qquad \int x'^i d\mu' = M_i',$$

Börgen finds

$$L = 2MM'r^{-3}\left[1 + \frac{1}{r^2}\left(2\frac{M_3}{M} - 3\frac{M_3'}{M'}\right) + \frac{1}{r^4}\left(3\frac{M_5}{M} - 15\frac{M_3}{M}\frac{M_3'}{M'} + \frac{45}{8}\frac{M_5'}{M'}\right)\right.$$
$$\left. + \frac{1}{r^6}\left(4\frac{M_7}{M} - 42\frac{M_5}{M}\frac{M_3'}{M'} + \frac{105}{2}\frac{M_3}{M}\frac{M_5'}{M'} - \frac{35}{4}\frac{M_7'}{M'}\right) + \ldots\right] (32)$$

M and M' represent the magnetic moments of the magnets, and so M is equivalent to the more ordinary notation m used earlier in the paper. The coefficients of r^{-2}, r^{-4}, and r^{-6} inside the square bracket represent the P, Q, R of (1). If the magnets are replaceable by poles, at distances 2λ, $2\lambda'$ apart respectively, then according to Börgen

$$M_i/M = \lambda^{i-1}, \quad M_i'/M' = \lambda'^{i-1}.$$

§ 15. When the areas of the cross sections are not neglected small additional terms are introduced. These must partly depend on the distribution of magnetism throughout the thickness of the magnets, which is unknown. In thin-walled circular cylinders the uncertainty cannot, however, be large. Taking the result which Börgen approves for this case [†], we have in full

$$P = 2\lambda^2 - 3\lambda'^2 + (3/4)\{3(a'^2 + b'^2) - 2(a^2 + b^2)\}, \quad . \quad (33)$$

[*] Terrestrial Magnetism, vol. i. 1896, p. 176; *Archiv. der Deutschen Seewarte*, 1891, &c.

[†] Terrestrial Magnetism, *l. c.* p. 189.

where a, b are the radii of the outer and inner surfaces of the deflecting magnet, while a', b' are the corresponding radii for the deflected magnet. The value of P when the a, b, a', b' terms are neglected I shall denote by $P(\lambda, \lambda')$. Similarly, $Q(\lambda, \lambda')$, $R(\lambda, \lambda')$ will represent those parts of the complete expressions for Q and R which remain when the areas of the cross sections are neglected. Complete expressions for Q and R would contain terms involving a, b, a', b', but the influence of Q and R on the value of the deflecting couple is ordinarily so small that it seems desirable to avoid the complication which the retention of these small terms would entail. Omitting them, we have in terms of λ and λ'

$$\left. \begin{aligned} Q(\lambda, \lambda') &= (3/8)(8\lambda^4 - 40\lambda^2\lambda'^2 + 15\lambda'^4), \\ R(\lambda, \lambda') &= (1/4)(16\lambda^6 - 168\lambda^4\lambda'^2 + 210\lambda^2\lambda'^4 - 35\lambda'^6) \end{aligned} \right\}. \quad (34)$$

Table X. gives the numerical value of the contribution to P from the terms in a, b, a', b', in some actual cases. The diameters $2a$, &c. are in cms.

TABLE X.

	$2a$.	$2b$.	$2a'$.	$2b'$.	$(3/4)\{3(a'^2+b'^2)-2(a^2+b^2)\}$
Jones's magnets :—					
Magnetometer i.	1·00	·735	·76	·50	−·11
„ ii.	·975	·74	·77	·50	−·09
„ iii.	·955	·665	·73	·435	−·10
Cooke magnets	1·02	·76	·70	·52	−·18
Dover „	1·035	·82	·825	·515	−·12
Elliott „	0·98	·77	·75	·40	−·18

The Jones's magnets—a very old form—are more variable in diameter than recent patterns, but absolute uniformity can hardly be claimed for any pattern, and in most cases the tube-walls are not quite uniform in thickness. Most collimator magnets, in fact, have shallow screws cut on the inner surface at the ends, a circumstance introducing some slight additional uncertainty, both as to the values of b and as to the degree of accuracy of Börgen's formula.

According to the figures in Table X. the contribution to P from the terms in a, b, &c. is negative in each case. It would be reduced numerically by a reduction in the wall thickness of the mirror magnets—keeping their external diameters unchanged or slightly increasing them—which would have the further recommendation of making these magnets more nearly than at present exact copies of the collimator magnets on a uniformly reduced scale.

There are, as will appear in § 24, other possible contributions to the value of the couple L, but as these arise from instrumental or observational defects it is desirable to postpone their consideration.

§ 16. There has been considerable difference of opinion as to the ratio between the pole-distance 2λ and the length l of an ordinary magnet. Kohlrausch found to a close degree of approximation

$$2\lambda/l = 5/6 = 0\cdot8\dot{3}.$$

Börgen concludes that the average value of the ratio is ·805, deducing this as a mean from numerous experiments based on a theoretical investigation, which appears more complete than Kohlrausch's, though not absolutely free from assumptions. Börgen finds that as the magnetic moment of a magnet is reduced, the pole distance tends normally to increase, until in a very weak magnet $2\lambda/l$ may rise to ·815. Whether he regards this as an absolutely limiting value is not quite clear, but he strongly combats the idea that it can ever approach unity, a result which he attributes to Mr. Blakesley [*] in a paper read before this Society in 1890.

§ 17. As the two magnets of an English magnetometer are fairly similar in type, there are grounds even apart from Börgen's experiments for regarding the assumption

$$2\lambda/l = 2\lambda'/l' = p \quad . \quad . \quad . \quad . \quad (35)$$

as likely to be at least approximately true. Adopting this hypothesis, provisionally, for the average magnetometer of any particular make, and for shortness writing z for λ'/λ, we have

$$P(\lambda, \lambda') = p^2 l^2 (1/4)(2 - 3z^2) = p^2 l^2 f_2(z), \text{ say,} \quad . \quad . \quad . \quad (36)$$

$$Q(\lambda, \lambda') = p^4 l^4 (3/128)(8 - 40z^2 + 15z^4) = p^4 l^4 f_4(z), \quad . \quad . \quad (37)$$

$$R(\lambda, \lambda') = p^6 l^6 (1/256)(16 - 168z^2 + 210z^4 - 35z^6) = p^6 l^6 f_6(z). \quad (38)$$

Table XI. gives the values of the above functions of z for values of z increasing by ·1 from 0 to 1, including the range encountered in practice.

TABLE XI.

$z=$	0	·1	·2	·3	·4	·5	·6	·7	·8	·9	1·0
$10^4 f_2(z)=$	+5000	+4925	+4700	+4325	+3800	+3125	+2300	+1325	+ 200	−1075	−2500
$10^4 f_4(z)=$	+1875	+1782	+1506	+1060	+ 465	− 249	−1044	−1875	−2685	−3412	−3984
$10^5 f_6(z)=$	+6250	+5602	+3755	+ 998	−2206	−5243	−7382	−7819	−5734	− 351	+8984

[*] Phil. Mag. March 1891, p. 281.

$f_2(z)$, and so P(λ, λ'), vanishes when $z = \cdot 8165$. It is positive for smaller, and negative for all larger values of z ; it diminishes algebraically continually as z increases from 0.

$f_4(z)$, and so Q(λ, λ'), vanishes when $z = \cdot 4667$ or $1 \cdot 565$. It diminishes algebraically as z increases from 0 to $1 \cdot 155$, where there is an (algebraical) minimum. It is negative when z lies between $\cdot 4667$ and $1 \cdot 565$, otherwise it is positive.

$f_6(z)$, and so R(λ, λ') vanishes when $z = \cdot 3318$, or $\cdot 9048$, or $2 \cdot 252$. It is positive when z lies between 0 and $\cdot 3318$, or between $\cdot 9048$ and $2 \cdot 252$. Outside these limits it is negative. There is an algebraical minimum (numerical maximum) when $z = \cdot 6714$, an algebraical (and numerical) maximum when $z = 1 \cdot 884$.

On the hypothesis $\qquad \lambda'/\lambda = l'/l,$

inferences can be drawn from Table XI. as to the best ratio between the lengths of the two magnets, assuming the terms depending on the cross section negligible to a first approximation.

Where the horizontal force is very high, as in India, it might be best, from a purely theoretical standpoint, to secure that R is negligible by making l'/l equal $\cdot 3318$ or a slightly larger value. This has the advantage of making Q small as well as R.

Where the force is comparatively low, and deflexion-angles are sufficiently large at distances so considerable that R may be left out of account, a good deal is to be said in favour of the ratio

$$l'/l = \cdot 4667,$$

as reducing Q to zero, and so rendering observations at more than two distances theoretically unnecessary.

Where the force is so small that very large deflexion distances are necessary, the value

$$l'/l = \cdot 8165,$$

for which P vanishes, has a good deal to commend it.

In polar regions, where the horizontal force is low, magnetic disturbances seem to abound, and climatic conditions are unfavourable ; there are thus obvious advantages in an arrangement which admits theoretically of a high degree of accuracy from deflexions at a single distance.

§ 18. Table XII. gives numerical results for representative English magnetometers, assuming $2\lambda'/l' = 2\lambda/l = p$, and assigning a series of values to p. The values of P$'(\lambda, \lambda')$ assume the two deflexion distances to be 30 and 40 cms.

Table XII.

p	Jones, and Cambridge Instrument Company. $l=9\cdot35$ cms. $l'=7\cdot60$ "				Cooke. $l=9\cdot27$ cms. $l'=6\cdot35$ "				Dover. $l=9\cdot13$ cms. $l'=6\cdot37$ "				Elliott. $l=9\cdot26$ cms. $l'=6\cdot37$ "			
	$P(\lambda,\lambda')$ +	$Q(\lambda,\lambda')$ −	$R(\lambda,\lambda')$, $10^2\times$ −	$P'(\lambda,\lambda')$	$P(\lambda,\lambda')$ +	$Q(\lambda,\lambda')$ −	$R(\lambda,\lambda')$, $10^2\times$ −	$P'(\lambda,\lambda')$ +	$P(\lambda,\lambda')$ +	$Q(\lambda,\lambda')$ −	$R(\lambda,\lambda')$, $10^2\times$ −	$P'(\lambda,\lambda')$ +	$P(\lambda,\lambda')$ +	$Q(\lambda,\lambda')$ −	$R(\lambda,\lambda')$, $10^2\times$ −	$P'(\lambda,\lambda')$ +
·76	0·23	710	68	1·02	7·35	431	96	6·58	6·50	430	87	5·73	7·19	435	96	6·41
·78	·24	788	79	1·15	7·74	478	113	6·89	6·84	477	102		7·76	483	112	
·79	·24	829	85	1·21	7·94	503	122	7·04	7·02	502	110	6·12	7·96	508	121	6·85
·80	·25	872	92	1·28	8·14	529	131	7·20	7·20	528	119	6·25	8·16	534	130	7·01
·81	·26	916	99	1·36	8·35	556	141	7·35	7·38	555	128	6·38	8·37	562	140	7·16
·82	·26	962	106	1·43	8·56	584	152	7·51	7·56	583	138	6·52	8·37	590	151	7·31
·84	·28	1060	123	1·59	8·98	643	176	7·82	7·94	642	159	6·78	8·78	649	175	7·61
·86	·29	1164	142	1·76	9·41	707	203	8·14	8·32	705	184	7·05	9·20	714	201	7·92
·88	·30	1276	163	1·95	9·85	775	233	8·45	8·71	773	211	7·32	9·63	782	231	8·22
·90	·32	1396	186	2·15	10·31	848	266	8·77	9·11	846	241	7·58	10·08	856	264	8·53

K 2

§ 19. If we suppose the pole-distance in each magnet to be ·8 of its length, and neglect terms involving the radii of the cross sections, except in the value of P, we obtain the following expressions for the couple between the two magnets (moments m and m') :

Jones, or Cambridge Instrument Company. $\quad 2mm'r^{-3}(1+0.15r^{-2}-872r^{-4}-9180r^{-6})$,

Cooke .... $\quad 2mm'r^{-3}(1+7.96r^{-2}-529r^{-4}-13130r^{-6})$,

Dover .... $\quad 2mm'r^{-3}(1+7.08r^{-2}-528r^{-4}-11890r^{-6})$,

Elliott.................... $\quad 2mm'r^{-3}(1+7.78r^{-2}-534r^{-4}-13030r^{-6})$.

The relative importance of the different terms in the above formulæ is shown in Table XIII. for the typical Jones and Cooke magnetometers.

TABLE XIII.

Deflexion distance cms.	Jones Magnetometer.				Cooke Magnetometer.			
	Principal term. +	P term. +	Q term. –	R term. –	Principal term. +	P term. +	Q term. –	R term. –
22·5	100000	30	340	7	100000	1572	206	10
26·25	100000	22	184	3	100000	1155	111	4
30	100000	17	108	1	100000	884	65	2
35	100000	12	58	0	100000	650	35	1
40	100000	9	34	0	100000	497	21	0

The results for the typical Elliott instrument are practically the same as for the Cooke; and those for the Dover instrument are practically the same in the case of the Q term, though some 10 per cent. less for the P and R terms.

In the above typical case, $p = 0.8$, the corrections (all +)

TABLE XIV.

Distances cms.	Jones or Camb. Inst. Co.			Cooke or Elliott.			Dover.		
	$10^5\frac{\delta H}{H}$	δH (unit 1γ). H=·185	H=·37	$10^5\frac{\delta H}{H}$	δH (unit 1γ). H=·185	H=·37	$10^5\frac{\delta H}{H}$	δH (unit 1γ). H=·185	H=·37
22·5 & 30	99	18·3	36·6	63	11·6	23·1	62	11·5	22·9
26·25 & 35	53	9·8	19·6	33	6·1	12·2	33	6·1	12·2
30 & 40	31	5·7	11·4	19	3·5	7·1	19	3·5	7·1

necessary to the value of H when Q and R are neglected,—observations being taken only at two distances,—are given in Table XIV.

The effect of an increase in p would be to raise the corrections. For instance, if we assumed $p = \cdot 86$, then for H $= \cdot 185$, and observations at 30 and 40 cms., we should raise the value of δH to $7 \cdot 6 \gamma$ for the Jones, $4 \cdot 8 \gamma$ for the Cooke, and $4 \cdot 7 \gamma$ for the Dover magnetometer.

§ 20. In any individual case the values of $2\lambda/l$ and $2\lambda'/l'$ may one or both differ from the mean, and if they do differ, they may differ in opposite directions. Table XV. gives the changes in P, Q, and R due to change of p from $\cdot 80$ to $\cdot 81$. A change from $\cdot 80$ to $\cdot 79$ has a nearly equal effect, but in the opposite direction.

TABLE XV.
Effect of change in p from $\cdot 80$ to $\cdot 81$.

Magnet in which p increased.	P		Q		R		P′(30 & 40 cms.).	
	Collr.	Mirror	Collr.	Mirror	Collr.	Mirror	Collr.	Mirror
	+	−	−	−	−	+	+	−
Jones (or Camb. Inst. Co.).	·704	·697	18·9	24·3	1444	750	·668	·737
Cooke (and practically Elliott).	·692	·487	4·6	21·5	1050	60	·682	·524
Dover.............	·671	·490	5·5	20·6	994	98	·659	·526

Increments in p in the collimator and mirror magnets always influence P in opposite directions, and a given increment in p has a very much greater effect when occurring in one than when occurring in both of the magnets. Small uncertainties in the pole-distance have comparatively little influence on Q, but materially affect P and P′.

§ 21. It has been usual when testing at Kew to observe at least twice with each magnetometer, so as to check the values of the constants and the general behaviour. Deflexions are made at 30 and 40 cms., and the value of P′ is thence determined. In the case of Dover magnetometers the following are the results :—

mean value of P′ from all instruments examined $+ 6 \cdot 20$

”   ”   ”   last 13   ”   ” $+ 6 \cdot 43$.

Applying the correction $- \cdot 12$ from Table X. to the values of P′ (λ, λ') in Table XII., we have for the average Dover magnetometer :

$$p = \qquad \cdot 80 \qquad \cdot 81 \qquad \cdot 82$$

Calculated value of P′ $= \quad + 6 \cdot 13 \quad + 6 \cdot 26 \quad + 6 \cdot 40$

Thus the mean from all the Dover magnetometers would

accord practically identically with the value ·805 assigned to p by Börgen; but the mean from the last 13 instruments would answer to $p = ·82$.

From the last 18 pairs of Cooke magnets, I find
mean value of P' (at 30 and 40 cms.) $= +8·00$.
Allowing a contribution of $-·18$ from Table X., we should infer from Table XII. that p slightly exceeds ·86.

From the last dozen pairs of Elliott magnets, I find
mean value of P' (at 30 and 40 cms.) $+6·77$.
Allowing a contribution of $-·18$ from Table X., we deduce $p = ·80$.

For the Cambridge Instrument Co's. magnets the average P' (at 30 and 40 cms.) has been $-1·50$; which allowing for the correction in Table X. answers to $p = ·81$.

The above mean values accord pretty closely with Börgen's value ·805 except in the case of the Cooke magnets. In their case the collimator has a central transverse groove which affords a grip for a screw determining the position of the magnet in its stirrup. This groove presumably exercises some influence on the pole-distance, and a very small influence would suffice to explain the observed high value of P'. The combination, for instance, of the value ·815 in the p of the collimator magnet with ·805 in that of the "mirror" magnet would raise P' to 8·0.

§ 22. To test the influence of the magnetic moment, m, I divided each maker's instruments into two equal groups, containing respectively the strongest and the weakest collimator magnets. The mean results appear in Table XVI.

TABLE XVI.

	Cooke.		Dover.		Elliott.	
	m.	P'.	*m.*	P'.	*m.*	P'.
Group with largest m	977	+7·76	884	+5·88	1031	+6·04
,, ,, smallest m ...	907	+8·24	788	+6·89	888	+7·49

Nothing is known of the strength of the mirror magnets, but we may reasonably suppose that a strong mirror magnet is at least as likely to accompany a strong as a weak collimator magnet. Assuming the average mirror magnet the same for the two groups, a larger P' means a larger p—*i. e.*, an increased pole-distance—in the collimator magnet. Thus the results seem all in favour of Börgen's law that the pole-

distance is greater the weaker the magnet. There are grounds, however, for some reserve of judgment. To account for the differences between the mean values of P′ in Table XVI. would require changes in p varying from ·007 to fully ·02. Now ·01 seems the limiting departure from the mean allowed by Börgen for the weakest magnets, whereas the difference between the stronger and weaker magnets in Table XVI. is comparatively small.

§ 23. Table XVI. gives but an inadequate idea of the apparent variability of P′ in different magnetometers. How far this apparent variability is real is somewhat doubtful, in view of observational and other uncertainties. We are, however, on surer ground when we consider changes in the mean annual values found for P′ at a fixed observatory, where numerous deflexions have been taken with the same two magnets, at the same two distances, in a single magnetometer.

At Kew, the same instrument and magnets have been in use for a generation. During the last twenty years, the extreme values found for P′ (at 30 and 40 cms.) from a year's observations have been −1·899 and −0·603, the mean value for the whole period being −1·238. The difference between the extremes might be accounted for by changes of ·01 in opposite directions in the values of p for the two magnets. There is, however, no reason to think that either magnet has increased in strength, and whilst the moment of the collimator magnet has gradually diminished, it altered by less than 2 per cent. between 1889 and 1892 when the extreme values of P′ presented themselves. If the cause lay with the mirror magnet, whose moment has not been recorded, its p must have altered nearly ·02. As a matter of fact, the variation of P′ has been so irregular as to suggest that the principal cause must have been something other than change of magnetic moment.

At Batavia Observatory, values were calculated for the P′ of a magnetometer from each two months' observations, from 1885 to 1893. A table recording the mean for the year, and the greatest and least of the 2-monthly values, appears in the Batavia " Mag. & Met. Observations " for 1893. The extreme 2-monthly values are −5·073 and +1·638, while the mean annual values vary from −0·920 to −3·786. The magnets are, I think, of English make, and the figures calculated here for the old Jones magnetometers will probably apply fairly. The view expressed at Batavia is that " the values of the constant P...show rather large differences which are to be ascribed to variation in the momentum (magnetic moment) of the deflected magnet ; since 1891 this

momentum has been kept as constant as possible." If this explanation be correct, we should infer, even from the annual means, a variation in the p of the mirror magnet amounting to nearly ·04.

According to the records of the Royal Alfred Observatory, Mauritius, the mean annual values of P′ for two English magnets 24_A and 24_C changed from $-5·059$ in 1890 to $-4·236$ in 1893, and then to $-5·006$ in 1897. This might be accounted for by a change of little over ·01 in the p of one of the magnets.

Fig. 1.
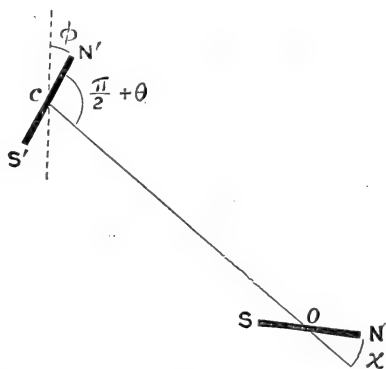
§ 24. In general, the ideal conditions of the deflexion experiment cannot be strictly fulfilled. The magnets are not only of finite section, but their axes may be neither strictly horizontal nor perpendicular to one another, while their centres may differ in level. Having already, § 15, considered the finiteness of the cross section, we shall in treating the other defects suppose the magnets linear. Also, as a small inclination in the deflected magnet reduces practically in the same proportion the couples acting on it in the horizontal plane, whether due to the deflecting magnet or the earth's field, we shall, to avoid undue complexity, suppose this magnet strictly horizontal. In fig. 1—which is practically Börgen's * figure—N′S′ represents the axis of this magnet, C its centre. The deflecting magnet if properly adjusted has its axis horizontal when suspended, but unless this axis happens to coincide with the mechanical axis it will ordinarily in the deflexion experiment be slightly inclined to the horizontal. In fig. 1, NS (middle point O) is the projection of the magnetic axis of the deflecting magnet on the hori-

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* *Archiv der Deutschen Seewarte*, 1891, no. 2, p. 2.

zontal plane containing N'S'. The notation employed—which differs from Börgen's—is as follows :—

$$\phi = \text{inclination of N'S' to magnetic meridian,}$$

$$\frac{\pi}{2} + \theta = \quad ,, \quad ,, \text{ N'S' to CO,}$$

$$\chi = \quad ,, \quad ,, \text{ NS to CO,}$$
$$\psi = \quad ,, \quad ,, \text{ deflecting magnet to horizon,}$$
$$z = \text{height of centre of deflecting magnet above O,}$$
$$r = \text{CO.}$$

We shall suppose θ, χ, ψ, and z/r all small quantities, as they ought to vanish with perfect adjustment.

The general expressions given by Börgen are complicated, and considering several sources of uncertainty I shall not go beyond the terms in λ^2 and λ'^2, and shall retain only the principal corrective terms. The results were mostly obtained independently, but have been checked by reference to Börgen's formulæ. The deflexion equation is

$$\text{H} \sin \phi = 2mr^{-3} \cos \psi \cos \chi \cos \theta (1 - \tfrac{1}{2} \tan \theta \tan \chi) \times$$

$$\Big[1 - 3(z^2/r^2)(1 + \tfrac{1}{4} \tan \theta \tan \chi)$$

$$+ 2(\lambda^2/r^2)(1 - \tan \theta \tan \chi - \tfrac{5}{2} \sin^2\psi - \tfrac{5}{2} \sin^2\chi - \tfrac{15}{2} z^2/r^2)$$

$$- 3(\lambda'^2/r^2)(1 + \tfrac{1}{4} \tan \theta \tan \chi - \tfrac{15}{2} \sin^2\theta + \tfrac{3}{2} \sin \theta \sin (\theta - \chi) - \tfrac{15}{4} z^2/r^2)$$

$$+ \tfrac{3}{2}(z/r) \tan \psi \Big\{ 1 + \tfrac{1}{2} \tan \theta \tan \chi + \tfrac{1}{2} \tan^2\chi - \tfrac{5}{2} z^2/r^2 + 10 \frac{\lambda^2}{r^2} - \tfrac{5}{2} \frac{\lambda'^2}{r^2} \Big\} \Big] \quad (39)$$

A departure of θ from zero may be due to the magnetic axis of the deflected magnet not being normal to the attached mirror, or to a want of perpendicularity between the axis of the mirror magnet's telescope and the deflexion-bar. An examination of some old mirror magnets (see § 26) showed that the inclination of the magnetic axis to the normal to the mirror may be as much as 1° or 2°, if not more. The necessity of providing for accuracy in this adjustment seems to have been somewhat overlooked.

A departure of ψ from zero may arise from want of level of the deflexion-bar, or want of parallelism between this bar and the magnetic axis of the deflecting magnet, due for instance to defect in the magnet carriage or to difference between the magnet's magnetic and mechanical axes.

A departure of χ from zero may be due to want of coincidence between the horizontal projections of the magnetic and mechanical axes of the deflecting magnet, to want of

straightness in the deflexion-bar or to its not lying in the same vertical plane as the centre of the deflected magnet.

For the same instrument θ, ψ, and χ should be practically constants, at least for a considerable period of time; z, on the other hand, will vary from day to day. If the axis of the sighting-tube when in the carriage takes the same position as that ordinarily occupied by the magnetic axis of the deflecting magnet, positive and negative values of z are equally probable, so that the last line in (39) should not appreciably influence the mean value of H from a large number of observations. Observational bias and other causes —*e. g.* stretching of the suspending fibre, or slipping of its support—may of course lead to a preponderance of values of z of one sign.

§ 25. To get some idea of the probable importance of the different terms, let us suppose, as a concrete case, that deflexions are being taken at 30 cms., while

$$\lambda = 4, \ \lambda' = 3, \ z = 0 \cdot 3 \text{ cm.},$$
$$\theta = \psi = \chi = 1°.$$

Then we may take $\tan 1° = \cdot 0175$,

$$\tan \theta \tan \chi = \sin^2\chi = \sin^2\psi = \sin^2\theta = \tan^2\chi = \cdot 0003.$$
$$z^2/r^2 = 1 \times 10^{-4}, \ \lambda^2/r^2 = \cdot 0178, \ \lambda'^2/r^2 = \cdot 0100.$$

Writing in the terms in the precise order they occupy in (39) we have

$$\text{H} \sin \phi = 2m \ r^{-3} \times 0 \cdot 99939 \times$$
$$[1 - \cdot 0003(1 + \cdot 0001)$$
$$+ \cdot 0356(1 - \cdot 0003 - \cdot 0008 - \cdot 0008 - \cdot 0007)$$
$$- \cdot 0300(1 + \cdot 0001 - \cdot 0021 - \cdot 0004)$$
$$+ \cdot 00026\{1 + \cdot 0002 + \cdot 0002 - \cdot 0002 + \cdot 178 - \cdot 025\}].$$

The most important correction is the introduction of $\cdot 99939$ in place of unity in the coefficient of $2m \ r^{-3}$. This represents a change of 6 per 10,000 in the value of H/m, or of 3 per 10,000 in the value of H. If the factor were, as is usual, assumed to be unity, H would be overestimated to this extent.

The next most important correction arises from the term $- 3z^2/r^2$ in the first line of the expression in square brackets. This term, it should be noticed, contributes to the value of P, thus showing how variations in the height of the deflected magnet may partly account for differences found for P on different days. In the concrete case the departure of the

coefficient of $-3z^2/r^2$ from unity is quite negligible. The subsidiary terms in the coefficients of $2\lambda^2/r^2$ and $3\lambda'^2/r^2$ are also very small, but might come in. At all events they cannot safely be assumed negligible. The terms depending on the angles and on z inside the bracket in the last line are wholly negligible, but clearly the last line is not safely negligible as a whole, unless somewhat greater care is taken about the level of the suspended magnet than is supposed in the concrete case. It is, however, mainly the fault of the observer if the last line is not negligible.

Experiments on Pole-Distance.

§ 26. Let us suppose that two magnets 1, 2, closely identical in size and pattern, can deflect each the other in the same magnetometer. Let the suffix 1 distinguish the properties of the first magnet, and let P_1, Q_1 &c. answer to the case when it is the deflector; similarly, let the suffix 2 relate to the second magnet. Then we have

$$\left.\begin{array}{l} 2\lambda_1^2 - 3\lambda_2^2 = P_1(\lambda_1,\ \lambda_2) = P_1' - f(a_1,\ a_2) - AQ_1 - BR_1, \\ 2\lambda_2^2 - 3\lambda_1^2 = P_2(\lambda_2,\ \lambda_1) = P_2' - f(a_2,\ a_1) - AQ_2 - BR_2, \end{array}\right\}\quad (40)$$

where
$$f(a_1,\ a_2) = (3/4)\{3(a_2^2 + b_2^2) - 2(a_1^2 + b_1^2)\},$$
$$A = r_1^{-2} + r_2^{-2},\quad B = r_1^{-4} + r_1^{-2}\,r_2^{-2} + r_2^{-4},$$

r_1 and r_2 being the two deflexion-distances.

Differences between the values of p for the pole-distances of the two magnets are, as we have seen, generally much less effective in altering Q and R than in altering P, and the contributions from Q and R in the above equations are relatively small. Thus a close approximation may be expected from the formula

$$5(\lambda_1^2 - \lambda_2^2) = P_1' - f(a_1,\ a_2) - \{P_2' - f(a_2,\ a_1)\},\quad .\quad (41)$$

which neglects the difference between Q_1 and Q_2 and between R_1 and R_2.

I have found in this way approximations to the differences between pole-distances in two sets of mirror magnets. The first set consisted of three very old magnets AN, Na, and Ne by Jones, practically identical in length, and differing only slightly in diameter. Their great similarity led one to anticipate a very close approach to equality in their pole-distances. This expectation was fulfilled for two of the magnets, but not for the third, Ne. At first I was disposed to look for an explanation in a difference between the directions of the

magnetic axis and the normal to the attached mirror (see § 24). This does not seem, however, to be the true explanation, as may be seen from the following results. The notation is the same as in § 24; λ refers in each case to the deflecting, λ' to the deflected magnet.

TABLE XVII.

Deflecting magnet.	Deflected magnet.	θ.	χ.	True formula for P(λ, λ').
AN	Na	$-\ 60$	$+25$	$2\lambda^2 - 2 \cdot 996\,\lambda'^2$
Na	AN	-123	$+85$	$1 \cdot 999\lambda^2 - 2 \cdot 987\,\lambda'^2$
AN	Ne	$+\ 49$	$+25$	$2\lambda^2 - 2 \cdot 996\,\lambda'^2$
Ne	AN	-123	$+61$	$2\lambda^2 - 2 \cdot 985\,\lambda'^2$
Na	Ne	$+\ 49$	$+85$	$1 \cdot 996\lambda^2 - 2 \cdot 995\,\lambda'^2$
Ne	Na	$-\ 60$	$+61$	$1 \cdot 999\lambda^2 - 2 \cdot 998\,\lambda'^2$

The differences from the normal form $2\lambda^2 - 3\lambda'^2$ are too trifling to be of real moment, and have been neglected in the subsequent calculations.

The second set of magnets were also mirror magnets, No. 60 by Elliott Bros., Nos. 138 and 140 by Dover.

Observations were made at two pairs of distances 18 and 24 cms., and 22·5 and 30 cms. The dimensions of the magnets were as follows, all in centimetres.

TABLE XVIII.

Magnet	AN.	Na.	Ne.	60.	138.	140.
Length l..................	7·60	7·60	7·60	6·385	6·42	6·385
External diameter $2a$...	·725	·76	·77	·74	·82	·81
Internal „ $2b$...	·43	·50	·50	·405	·52	·53

The diameters appeared in all the magnets to be very nearly uniform throughout the entire length.

§ 27. Some fifty deflexion experiments were made by the Chief Assistant of the Observatory Department, Mr. T. W. Baker, of whose observational skill I was fortunately able to avail myself. The results appear in Table XIX. λ_1 refers to AN or 60, λ_2 to Na or 138, λ_3 to Ne or 140.

TABLE XIX.

Deflecting magnet.	Deflected magnet.	$f(a, d)$.	$P' - f(a, d)$ for 18 & 24 cms.	$P' - f(a, d)$ for 22·5 & 30 cms.	The $\lambda^2 - \lambda'^2$ involved.	Value of $\lambda^2 - \lambda'^2$ from observations at 18 & 24 cms.	22·5 & 30 cms.	Mean.
		+	—	—		+	+	+
AN	Na	·20	13·90	13·63	$\lambda_2^2 - \lambda_1^2$	·006	·130	·068
Na	AN	·09	13·87	12·98				
AN	Ne	·21	18·44	16·80	$\lambda_3^2 - \lambda_1^2$	1·248	1·136	1·192
Ne	AN	·09	12·20	11·12				
Na	Ne	·16	18·02	16·81	$\lambda_3^2 - \lambda_2^2$	1·112	1·032	1·072
Ne	Na	·16	12·46	11·65				
60	138	·26	9·75	9·12	$\lambda_2^2 - \lambda_1^2$	·194	·074	·134
138	60	·05	8·78	8·75				
60	140	·26	9·20	8·46	$\lambda_1^2 - \lambda_3^2$	·068	·156	·112
140	60	·05	9·54	9·24				
138	140	·17	8·36	8·56	$\lambda_2^2 - \lambda_3^2$	·418	·274	·346
140	138	·18	10·45	9·93				

After the observations were completed, it was noticed that through the employment of a sighting-tube intended for a larger deflecting magnet, the level of the suspended magnet had been in each case slightly too high. A small correction $\cdot12$, answering to the mean error $\cdot2$ cm. in the level, has been applied in the Table to the observed values of P′.

§ 28. To determine the value of, say, $\lambda_2 - \lambda_1$, we put

$$\lambda_2{}^2 - \lambda_1{}^2 = (\lambda_2 - \lambda_1)(\lambda_2 + \lambda_1),$$

and as $\lambda_2 - \lambda_1$ is small, we may for a first approximation take

$$\lambda_2 + \lambda_1 = \cdot8l = 6\cdot1 \text{ cm. for AN, Na, and Ne,}$$
$$= 5\cdot1 \quad \text{,, \quad ,, \quad 60, 138, and 140.}$$

In this way we find

TABLE XX.

For AN, Na, and Ne.		For 60, 138, and 140.	
cms. $\lambda_2 - \lambda_1 = \cdot011$	$2(\lambda_2 - \lambda_1)/l = \cdot003$	cms. $\lambda_2 - \lambda_1 = \cdot026$	$2(\lambda_2 - \lambda_1)/l = \cdot008$
$\lambda_3 - \lambda_1 = \cdot195$	$2(\lambda_3 - \lambda_1)/l = \cdot051$	$\lambda_1 - \lambda_3 = \cdot022$	$2(\lambda_1 - \lambda_3)/l = \cdot007$
$\lambda_3 - \lambda_2 = \cdot176$	$2(\lambda_3 - \lambda_2)/l = \cdot046$	$\lambda_2 - \lambda_3 = \cdot068$	$2(\lambda_2 - \lambda_3)/l = \cdot021$

If theory and experiment were perfect, the results for $\lambda^2 - \lambda'^2$ from the two pairs of distances in Table XIX. should be identical, and in Table XX. the value of, say, $\lambda_3 - \lambda_2$ found directly should agree with that deduced from the values of $\lambda_1 - \lambda_2$ and $\lambda_3 - \lambda_1$. This cannot be claimed, but on the whole there is a very satisfactory agreement, considering that a complete elimination of the effects of variation of horizontal force and temperature could only be hoped from a largely increased number of observations.

The two magnets AN and Na behave practically alike, but differ notably from Ne, though to the eye that magnet seems an almost exact duplicate of Na. As these three magnets are practically equal in length, Table XX. implies that the value of p must be about $\cdot05$ larger in Ne than in the other two.

In the case of 60, 138, and 140, the agreement in pole-distance is closest between 60 and 140, though by different makers. The excess, however, of pole-distance apparent in 138 is partly accounted for by its excess of length, about $\cdot035$ cm., over the other two. If we allow for this, we find that the value of p in 138 exceeds that in 60 by only about $\cdot003$. The value obtained directly for $\lambda_2 - \lambda_3$ is probably slightly in excess, and the excess of p in 138 over 140 is probably about $\cdot01$.

§ 29. Having now proved a close similarity in pole-distance

between AN and Na on the one hand, and between 60, 138, and 140 on the other, we occupy a favourable position for determining an approximate value for p by the method of § 21. The necessary comparison between calculated and observed quantities is made in Table XXI. In the case of 60, 138, and 140, I have assumed a mean value 6·40 for l and combined all the observations. To the present degree of approximation, that should give a satisfactory mean result.

TABLE XXI.

Magnets.	p.	Calculated values of						Observed mean value of $P'(\lambda,\lambda')$.	
		$P(\lambda,\lambda')$.	$(r_1^{-2}+..)Q$ $+(r_1^{-4}+..)R.$		$P'(\lambda,\lambda')$.				
			18 & 24 cms.	22·5 & 30 cms.	18 & 24 cms.	22·5 & 30 cms.	18 & 24 cms.	22·5 & 30 cms.
AN and Na	·84	10·19	3·08	2·00	13·27	12·19		
	·86	10·68	3·38	2·19	14·06	12·87	13·88	13·30
	·88	11·18	3·70	2·40	14·88	13·58		
60, 138, 140	·84	7·23	1·57	1·01	8·80	8·24		
	·86	7·57	1·72	1·11	9·29	8·68	9·35	9·01
	·88	7·93	1·88	1·22	9·81	9·15		

By interpolation we deduce for the value of p :

From observations	at 18 & 24 cms.	at 22·5 & 30 cms.	Mean.
Mean for AN and Na	·855	·872	·86
Mean for 60, 138, and 140...	·862	·874	·87

The results can be readily checked by reference to the fact that when λ and λ' are equal

$$P(\lambda, \lambda') = -\lambda^2 = -p^2 l^2/4.$$

§ 30. The excess of these values for p over Börgen's limiting value ·815 may arise possibly from some instrumental peculiarity, or some defect in the theory; but it is difficult to imagine that any such cause can account for the apparent difference between Ne and its companion magnets. To obtain, however, a further check, I got Mr. Baker to carry out a series of observations in which AN, Na, and Ne were deflected in turn by the same collimator-magnet KC1 in the Kew magnetometer at the three distances 22·5, 30, and 40 cms.

Values were calculated for P′ from the observations at 22·5 and 30 cms., and from those at 30 and 40 cms. The mean results appear in Table XXII.

<div align="center">Table XXII.</div>

Magnet deflected.	$f(a, b)$.	Observed P′—$f(a, b)$.		P(λ, λ') allowing for Q and R.		
		22·5 & 30 cms.	30 & 40 cms.	22·5 & 30 cms.	30 & 40 cms.	Mean.
AN	·10	3·23	1·92	−0·33	−0·31	−0·32
Na	·03	2·67	2·51	+0·23	−0·90	−0·33
Ne	·03	6·84	4·78	−3·94	−3·17	−3·55

In allowing for Q and R, p was taken as ·81, and the observations were not numerous, so that high accuracy cannot be claimed ; but the difference between Ne and the two other magnets is conspicuous. Again, taking the mean observed values obtained above for P(λ, λ'), and employing 2λ to denote the pole-distance of the magnet KC1, we have

$$\left. \begin{array}{l} 2\lambda^2 - 3\lambda_1^2 = -\ \cdot32 \\ 2\lambda^2 - 3\lambda_2^2 = -\ \cdot33 \\ 2\lambda^2 - 3\lambda_3^2 = -3\cdot55 \end{array} \right\} \text{ whence } \begin{array}{l} \lambda_3^2 - \lambda_1^2 = 1\cdot08 \\ \lambda_3^2 - \lambda_2^2 = 1\cdot07. \end{array}$$

The agreement with the values 1·19 and 1·07 obtained for $\lambda_3^2 - \lambda_1^2$ and $\lambda_3^2 - \lambda_2^2$ in Table XIX. is very satisfactory.

§ 31. During the deflexion experiments the magnetic moments of the magnets, in C.G.S. units, were approximately as follows :

AN.	Na.	Ne.	60	138	140
250	310	217	333	285	222

Of the Jones magnets Ne thus possessed the lowest moment as well as the greatest pole-distance ; but amongst the other three magnets 140 had both the lowest moment and the smallest pole-distance. Also the difference between the moments of Ne and AN is less than the difference between AN and Na, which seemed practically identical so far as pole-distance is concerned. The facts thus throw no certain light on the question whether the strength influences the pole-distance.

The Jones magnets, as already stated, are very old. They

were magnetized first, so far as is known, some fifty years ago, and are not known to have been remagnetized since.

The magnets 140, 138, and 60 vary in age from five to twenty years. They too are supposed never to have been remagnetized. It would seem worth inquiry whether mere age, as apart from change of strength, may not tend to increase the pole-distance.

Experiments with a set of collimator magnets, following the same lines as those with the six mirror magnets, would have been of fully greater interest ; and the combination of the results obtained for the collimator magnets alone, and for the mirror magnets alone, with results obtained by employing the collimators to deflect the mirror magnets, would have formed a promising though laborious line of research. Ordinary magnetometers, however,—except those by Messrs. Cooke of the India Office pattern—do not possess a deflexion-chamber sufficiently long to take a collimator magnet ; and no opportunity of carrying out deflexion experiments on collimators has presented itself.

XV. *Cathode Disintegration in the Discharge through Gases at Low Pressures.* By L. HOLBORN *and* L. W. AUSTIN [*].

THE use of the disintegration of the cathode in the vacuum discharge for the production of thin metal films was first proposed by Plücker [†]. This proposal was later carried out by Wright [‡] and Kundt [§]. The first quantitative observations were made by Crookes [||], who compared the disintegration of a large number of metals by means of a rotating switch which connected alternately four cathodes in the same exhausted tube with an induction-coil. In each of the experiments, which lasted several hours, one of the cathodes was gold, which was chosen as the standard with which the disintegration of the other metals was compared. Metals with a high melting-point were used in the form of wire. The more easily melted metals, such as tin, cadmium, and lead, were in the form of thick rods surrounded by porcelain cups. In the latter cases the gold was in similar form.

Crookes's results are given in the following table, the disintegration of gold being represented arbitrarily by 100.

Palladium....	108	Brass........	52	Iridium......	10·5
Gold	100	Platinum	44	Iron	5·5
Silver........	83	Copper	40	Aluminium ..	0
Lead	75	Cadmium	32	Magnesium ..	0
Tin..........	57	Nickel	11		

Recently Granquist* has investigated the disintegration of gold, platinum, silver, and copper. The metals were used in the form of foil in a tube 20 cm. long and 3·5 cm. in diameter, in which five cathodes could be inserted by means of ground-glass connexions. The current from a battery of small storage-cells was sent for a given length of time, one hour or less, through one cathode at a time, the potential as well as the gas-pressure being kept as constant as possible during the time of the experiment.

From these experiments Granquist drew the following conclusions:—That the loss in weight increases rapidly with decreasing pressure; below 0·6 mm. pressure gold appears to disintegrate more rapidly than the other metals, then follows platinum, and finally copper and silver. The last two metals, at any rate within the range of pressure investigated with the copper cathode, disintegrate in about the same degree. At pressures above 0·6 mm. platinum shows the greatest disintegration. If the pressure of the gas be kept constant so that the difference of potential increases with increasing current, the disintegration is nearly proportional to the square of the current strength. Platinum cathodes heated to a red heat show the same disintegration as at ordinary temperatures.

With the idea that the investigation of the disintegration of the cathode is of great importance for an understanding of the passage of electricity through gases, we have again taken up the investigation of the subject.

1. *Arrangement of Apparatus.*

The cathodes consisted of circular disks of metal, 1 cm. in diameter, hung on wires of the same metal †, and were introduced by means of ground-glass connexions into the side tubes of the 4·5 cm. wide glass tube A (fig. 1). In order to make the connexions air-tight, mercury was at first used, but was soon abandoned and pump-grease was substituted, since it was difficult to prevent small drops of mercury entering the tube in the frequent opening of it; and it was soon found that if the

* G. Granquist, *Öfvers. af K. Vetenskaps-Ak. Förhandl.* p. 709 (1898).
† Iron wire was used to suspend the cathodes of Sb, Bi, Zn, Sn, and Pb.

discharge struck even the smallest particle of mercury the cathodes became amalgamated *, a phenomenon which was most marked in the case of gold.

In order to confine the disintegration to the front of the cathode, the back was covered by a disk of mica and the supporting wire was surrounded by a narrow glass tube.

Fig. 1.
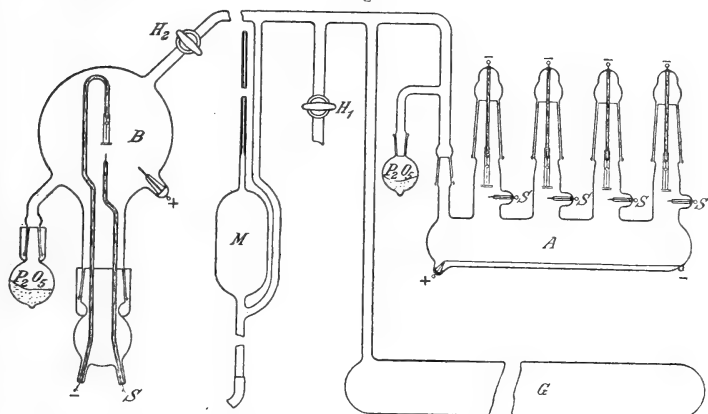
A few millimetres below the cathode a platinum wire S was sealed into the tube, and the difference of potential between this and the cathode was measured with Braun electrometers, one reading to 1500 V. and the other to 10,000 V. During each series of observations, the difference of potential was kept as constant as possible by regulating the gas-pressure. As this was found to be a rather difficult matter, a glass tube G of 1 litre capacity was connected with the apparatus in order to diminish the lowering of the gas-pressure, and the accompanying increase of potential-difference which took place when the current flowed. Notwithstanding this, it was frequently necessary to admit air from the Kahlbaum pump through the cock H_1 into the apparatus. With a little practice it became possible, by turning the cock more or less rapidly, to introduce the desired quantities of gas, if the pressure in the pump was kept a little higher than in the discharge apparatus. The pressure was measured in the ordinary way, by means of a MacLeod manometer M.

A 30-plate Toepler machine furnished the current, which in most of the experiments was kept constant at 0.6×10^{-3} amp. It was found possible to prevent the current from varying

* The amalgamation of Al cathodes was observed by E. Warburg (Wied. *Ann.* xxxi. p. 577, 1887).

more than 1 per cent. for hours at a time by regulating the electric motor, which was driven by the current from a storage-battery. The cathodes were connected to earth through a very sensitive milliamperemeter.

In the course of the investigation, a second spherical tube B, 12 cm. in diameter, was connected to the first. A cathode and its attendant exploring wire were placed approximately in the centre of the tube. As this tube was also frequently used for regulating the gas-pressure, it was separated from the rest of the apparatus by a cock H_2.

2. *Observations in Tube* A.

Four cathodes were situated in tube A, and the discharge was sent through one of them at a time for from 15 to 30 minutes. Before using, the cathodes were left over-night *in vacuo*. The weighings before and after the passage of the current gave the loss of weight with a certainty of 0·01 mg. In the tables the losses in weight are calculated for a uniform time of 30 minutes on the assumption that the loss is proportional to the time of passage of the current.

The cathode-fall was varied from its normal value (about 350 V.) where the negative discharge begins to cover the whole cathode, to about 2500 V. Experiments in which the potential was higher were inclined to give contradictory results, probably on account of irregularities in the discharge. These irregularities appeared to begin as soon as the negative column began to contract so that it ceased to cover the whole of the surface of the cathode. If the potential was forced still higher, so that green phosphorescence appeared on the glass, the loss of weight of the cathode decreased in a marked degree, whereas before it had steadily increased with increasing potential-difference. The metal deposits on the walls of the tubes were not removed between the series of observations. The mica disks on the backs of the cathodes, on which a deposit was also formed, were removed after each experiment.

Table I. contains the observations on different metals. Under the cathode-fall **V** are given the means of the readings taken every five minutes during the course of the discharge. The loss of weight y (for a time of discharge of 30 minutes and a current-strength of $0·6 \times 10^{-3}$ amp.) are given in mg. $\times 10^{-2}$, the gas-pressure p in mm. Hg.

For all the metals, with the exception of gold, the loss of weight y from a certain point on increases directly as the cathode-fall **V** (fig. 2, p. 150). All these curves prolonged backwards cut the axis of abscissas at the point **V** = 495. It is to be observed that the disintegration does not disappear at this point, but appears to decrease asymptotically as **V** approaches the normal value.

TABLE I.—Disintegration in Tube A.

Cathode.	p (mm. Hg)	V (Volts)	y (mg×10⁻²). Obs.	Obs.−Cal.
		Silver *a*.		
B 3	0·38	900	73	+2
A 1	0·28	1120	109	0
B 1	0·27	1310	145	+2
B 2	0·25	1490	172	−3
A 2	0·20	2140	288	0
		Silver *b*.		
A 4	0·6	590	21	+2
A 7	0·40	910	82	−2
A 12	0·24	1330	167	−3
A 6	0·23	1460	198	+1
A 5	0·24	1540	215	+2
		Platinum.		
A	0·34	890	63	+3
B	0·28	1040	83	0
A	0·28	1190	104	−2
B	0·25	1300	129	+6
A	0·25	1440	138	−6
		Iridium.		
	0·52	640	10	− 1
	0·31	1080	53	+ 7
	0·29	1300	62	1
	0·26	1520	84	+ 3
	0·21	1880	106	− 3
	0·18	2450	138	−16
		Copper.		
	...	450	2	...
	0·45	810	15	−1
	0·33	1010	27	0
	0·25	1560	58	+2
	0·19	2380	99	0
		Nickel.		
	0·33	1200	22	−1
	0·26	1500	36	+3
	0·25	1920	48	+1
	0·23	2290	53	−6

	p (mm. Hg)	V (Volts)	y(mg.×10⁻²). Obs.	Obs.−Cal.
Bismuth.				
	0·27	1110	117	−2
	0·22	1270	152	+2
Palladium.				
	0·45	870	29	−8
	0·39	1100	55	−4
	0·28	1460	97	+2
	0·27	1520	104	+2
	0·27	1570	104	−1
	0·17	2610	214	+7
Antimony.				
	0·31	800	25	+2
	0·26	1090	46	+2
	0·22	1450	69	−2
	0·21	1730	88	−4
Rhodium.				
	0·58	590	7	+1
	0·40	1020	29	−5
	0·28	1480	69	+5
	0·27	1540	71	+3
	0 22	1980	99	+3
	0·19	2500	127	−3
Gold.				
	1·25	360	8	...
	0·48	700	62	− 4
	...	950	138	− 9
	0·25	1230	193	− 45
	0·26	1240	202	− 40
	0·26	1300	205	− 57
	0 23	1520	296	− 37
	0·18	1860	365	− 78
	0·15	2350	433	−169

The values of y are calculated from the equations:—

Silver *a*, $y=0.175(V-495) = 0.00162_5 A(V-495)$;
Silver *b*, $y=0.204(V-495) = 0.00189 A(V-495)$;
Platinum, $y=0.153(V-495) = \frac{1}{2}A \times 0.00157(V-495)$;
Iridium, $y=0.0786(V-495) = \frac{1}{4}A \times 0.00163(V-495)$;
Copper, $y=0.0525(V-495) = \frac{1}{2}A \times 0.00165(V-495)$;
Nickel, $y=0.0328(V-495) = \frac{1}{3}A \times 0.00168(V-495)$;
Bismuth, $y=0.193(V-495) = \frac{1}{2}A \times 0.00185(V-495)$;
Palladium, $y=0.0982(V-495) = \frac{1}{2}A \times 0.00184(V-495)$;
Antimony, $y=0.0745(V-495) = \frac{1}{3}A \times 0.00186(V-495)$;
Rhodium, $y=0.0646(V-495) = \frac{1}{3}A \times 0.00188(V-495)$;
Gold, $y=0.325(V-495) = 0.00165 A(V-495)$.

If we now determine the inclination of the different curves, the equations for which are written after the tables, it is seen that the ratios of the trigonometric tangents of the angles stand in simple relations to the ratios of the atomic weights A. It is necessary, indeed, to distinguish two groups of the metals, to the first of which belong silver, platinum, iridium, copper, and nickel; to the second, silver, bismuth, palladium, antimony, and rhodium. The members of each group disintegrate in equivalent proportions at the same potential, when for the individual metals valencies are assumed which are recognized in chemistry.

Fig. 2.
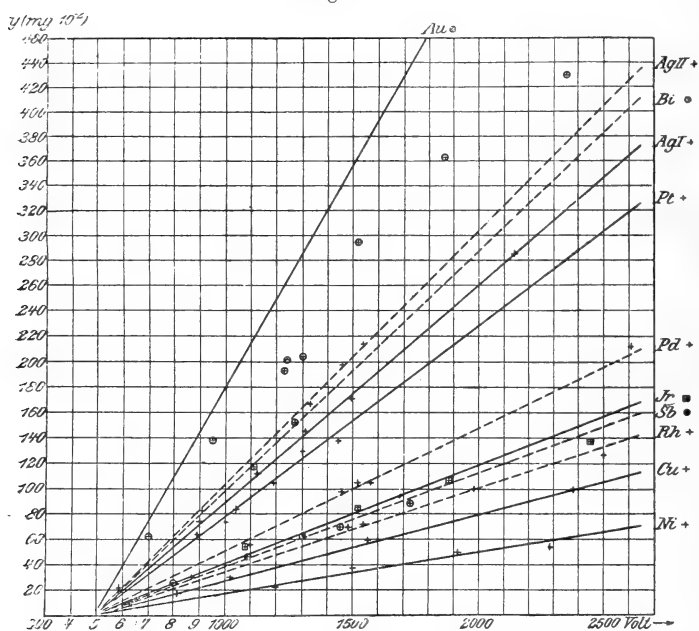
The disintegration of silver, which belongs to both groups, follows two different curves, whose ordinates stand in the ratio $189 : 162 \cdot 5 = 1 \cdot 16$. The cause of this difference has not been made clear. It can only be said that the silver cathode A (Table I.) gave first the values given under silver *a*, and represented by the curve Ag I, and after being in use for some time suddenly began giving the values given under silver *b*, and shown in curve Ag II. A fresh silver cathode B, put in the same place in the tube, gave values for *y* corresponding to the lower curve. The order in which the observations

were taken is indicated by the numbers in the first column in the two tables. A difference in gas-pressure for the same cathode-fall could not be discovered in the two cases.

In the two groups of metals, the loss of weight y of the cathode, due to the passage of a current of 0.6×10^{-3} amp. for half an hour is represented very closely by the equations

$$y = 0.00163 \ A/n(V - 495), \quad \text{or}$$
$$y = 0.00186 \ A/n(V - 495) \ \text{mg.} \times 10^{-2}.$$

In a voltameter during the same time, the same current would deposit $0.0112 \ A/n$ mg. according to the Faraday law, where A/n denotes the chemical equivalent. This quantity is disintegrated in the discharge at low pressures (tube A) at a cathode-fall of 1180 V. or 1090 V. respectively. A second effect must be assumed in addition to the Faraday law to explain the change of disintegration with the change of potential.

A similar curve has been calculated for gold as a univalent metal. The calculated values are all larger than the observed, and the difference increases with increasing cathode-fall.

Several easily oxidized metals of low melting-point were also observed. At low potentials, zinc gave fairly good results, on the assumption that it is a di-valent metal. We do not lay great stress on this, however, as the cathode was strongly oxidized as soon as the cathode-fall was increased above 1600 volts. After this experiment, the loss of weight was found to be abnormally large, and the cathode and walls of the tube were entirely covered with white oxide. The surface also showed a crystalline structure which denoted a high temperature. Cadmium, lead, and tin were also tried, but as they are still more easily oxidized the results are not taken into consideration, as we desired to investigate the disintegration under conditions as far as possible free from sublimation and ordinary oxidation. Many of the metals given in the tables were coloured on account of slight oxidation, especially after the discharge at low potentials. Even platinum showed the Priestley rings at the point opposite the exploring wire. Sometimes however, this point remained unchanged, and the rim of the cathode showed the coloured rings.

We must also mention the experiments with aluminium, iron, and steel, all of which show very little disintegration. In the case of steel we examined both a hard and a soft specimen, to see whether the hardness of the material was a cause of difference. The loss of weight in both cases was so small, however, that the difference fell within the limits of

the observation errors.   The loss of weight of the aluminium cathode was only 0·01 to 0·02 mg. (V = 800 to 1800), while iron and steel at 1000 V. did not seem to disintegrate at all, and at 2000 V. lost 0·15 mg.

In the above-mentioned observations on gold, it appears that the amount of the disintegration is too small in comparison with the chemical equivalent, considering gold as a uni-valent metal.   As gold in some other cases, as we shall see later, behaves regularly, we can suppose that possibly the irregularities are caused by the method of observation *, or we may suppose that a portion of the atoms under certain conditions assume a larger valency.

Observations on platinum and bismuth make the last assumption probable, since a few cases occurred in which both metals, which we have assumed to be di-valent, must be treated as tri-valent if we are to make the formula agree with the observed values.   For example, for V = 800 and 900, bismuth gave the disintegration $y = 44$ and 51 (the tri-valent metal would give 39 and 52 as the calculated values).   In the case of platinum, for V = 1900 the value $y = 140$ was once found (the tri-valent metal would indicate 143).   If it is possible for atoms to be thrown off having different valencies, the curves calculated for the smaller valencies can only be considered an upper limit for the possible loss in weight of the cathodes.

3. *Observations of Granquist.*

We will now undertake to determine how far our hypothesis can be applied to the observations made by Granquist. These were made with comparatively strong currents; the losses of weight, however, are given only to tenths of milligrams.   The difference of potential between the cathode and the tube-wall was not often more than 1000 V.   Several observations were made at very low potential, which on account of the above mentioned grounds we do not consider   With these exceptions, we make use of practically all of his observations, and, indeed, not only those from his Tables IV. to VI. where the current was constant, but also the values in Tables I. to III. and in Table VII. where it was varied.   The cathodes were in most cases thin metal sheets, 12 mm. long and 4·8 mm. broad, which hung parallel to the axis of the tube, so that both sides were subject to disintegration.   It was

* A recent observation indicates that gold which has stood under vacuum for a long time in connexion with a mercury-pump becomes amalgamated.

only in the case of platinum that cathodes of larger and smaller dimensions were used *.

We have calculated from Granquist's values the disintegration for a time of discharge of 30 minutes and a current-strength of 20 scale-divisions $= 2 \cdot 49 \times 10^{-3}$ amp., on the assumption that the disintegration was proportional to the current-strength, which assumption seems to be at least approximately true according to our own observations. We leave out of account the difference in current-density, as Granquist's Table I. shows that with increasing current-density the disintegration and potential increase in the same ratio.

TABLE II.—Observations of Granquist.

$i.$	p (mm. Hg)	V (Volts)	$y_1 (\text{mg.} \times 10^{-1})$.		$i.$	p (mm. Hg)	V (Volts)	$y_1 (\text{mg.} \times 10^{-1})$.	
			Obs.	Obs.—Cal.				Obs.	Obs.—Cal.
		Gold.					Platinum.		
20	0·23	1050	80	− 3	*20	0·38	1300	55	0
19	0·20	950	74	+ 3	*20	0·38	1075	43	+1
20	0·27	865	48·5	−12	20	0·38	825	30	+2
30	0·39	825	51	− 5	*20	0·38	600	15	0
30	0·39	725	45	+ 1					
20	0·39	625	34	+ 1	30	0·54	945	37	+2
20	0·39	625	32·5	0	25	0·54	920	32	−1
20	0·39	620	32·5	0	20	0·54	835	20	−8
20	0·55	605	19·5	− 10	15	0·54	645	17	0
		Silver.			35	0·95	630	20	+4
20	0·13	1050	44	−1	35	0·95	625	18·5	+2
20	0·21	755	20·5	−5	25	0·95	580	13	0
20	0·23	700	19	−4	23	0·95	515	12	+2
20·5	0·31	625	14	−4					
					20·5	0·17	1330	63	+6
		Copper.			19	0·20	1265	48	−5
15	0·20	1410	19	−1	21	0·23	985	46	+9
28	0·31	1130	16	+1	21	0·38	835	30	+2
28	0·47	1150	3	−2	20	0·54	835	20	−8
10	0·47	590	7	+2	23	0·95	580	13	0
28	1·01	525	4	0	23	0·95	515	12	+2

The values of y_1 are calculated from the equations :—

Gold, $\quad y_1 = 0 \cdot 118 (V - 350) = 0 \cdot 00060 A (V - 350)$;

Silver, $\quad y_1 = 0 \cdot 0648 (V - 350) = 0 \cdot 00060 A (V - 350)$;

Copper, $\quad y_1 = 0 \cdot 0191 (V - 350) = \frac{1}{2} A \times 0 \cdot 00060 (V - 350)$;

Platinum, $y_1 = 0 \cdot 0584 (V - 350) = \frac{1}{2} A \times 0 \cdot 00060 (V - 350)$.

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* The observations when the cathodes had other dimensions are marked with an asterisk.

In this way we obtain Table II., in which i represents the current-strength in scale-divisions, V the difference of potential between the cathode and the neighbouring tube-wall, and y the loss in weight of the cathode for a current of 20 scale-divisions $2 \cdot 46 \times 10^{-3}$ amp. flowing for 30 minutes. The observed values of y are compared with values calculated on the assumption that the loss of weight of different metals is proportional to their chemical equivalents. With the exception of a few large deviations where the observed values are too small, the differences (observed — calculated) fall within the limits of the errors of observation.

The values for y lie in a straight line which cuts the axis of abscissas at the point $V = 350$. The value $0 \cdot 00060$, which when multiplied by the chemical equivalent gives the trigonometric tangent of the angle between the axis of abscissas and the given line, would correspond to $0 \cdot 00146$ for a current-strength of $0 \cdot 6 \times 10^{-3}$ amp. and $0 \cdot 01$ mg. as the unit of weight. From these observations it is impossible to discover any direct influence of gas-pressure on the disintegration.

4. *Observations in Tube* B.

The fall of potential at the cathode depends not only on the gas-pressure and the current-density, but also on the situation of the cathode. A cathode surrounded by a narrow tube shows in general a higher cathode-fall for the same pressure and current-density than one in a wider tube. We have attempted to discover how these conditions affect the disintegration by observations on platinum, gold, and silver in tube B (fig. 1).

Table III. contains the results for platinum, silver, and gold. In the case of platinum the cathodes B and C were of the form already described, while D was of similar form but of twice the diameter. This difference in size made no appreciable difference in the disintegration. As in the former experiments, the current-strength was $0 \cdot 6 \times 10^{-3}$ amp., with the exception of the two cases marked with an asterisk, where the observed loss of weight was doubled since the current-strength was only $0 \cdot 3 \times 10^{-3}$ amp. The table shows that for platinum the loss of weight is the same as in tube A.

Gold and silver behave differently. The loss of weight, indeed, would be represented graphically by straight lines, which when extrapolated cut the axis of abscissas at the point $V = 610$ and rise much more steeply than in the former cases. For the same potential, the equivalent losses of weight stand in the relation $300 : 267 = 1 \cdot 12$, which is the same ratio as exists between the two groups of silver observations.

As the tables show, the silver cathode had already begun to give the higher values in tube A before it was used in tube B.

TABLE III.—Disintegration in Tube B.

Cathode.	p (mm.Hg)	V (Volts)	y(mg.×10⁻²). Obs.	Obs. − Cal.	Cathode.	p (mm.Hg)	V (Volts)	y(mg.×10⁻²). Obs.	Obs. − Cal.
		Platinum.					**Silver.**		
B	1·25	360	1		A 11	0·25	970	115	−2
B	1·10	400	3		A 10	0·23	1120	168	+3
B	0·48	600	31		A 8	0·15	1390	255	+2
B	0·29	960	70	− 1	A 9	0·14	1530	296	−2
D	...	1200	104	− 4					
C	...	1200	122	+14			**Gold.**		
B	0·11	1540	156	− 4					
D	0·05	1850	211	+ 4					
C	...	2210	252*	−10	...	0·20	1000	210	+4
D	0·03	2220	267	+ 3	...	0·19	1040	223	−3
C	...	2360	280*	− 5	...	0·12	1270	345	−3

The values of y are calculated from the equations:—

Platinum, $y = 0·153(V − 495) = \frac{1}{2}A × 0·00157(V − 495)$;
Silver, $y = 0·324(V − 610) = 0·00300A(V − 610)$;
Gold, $y = 0·527(V − 610) = 0·00267A(V − 610)$.

5. *Disintegration in Hydrogen.*

Observations were also made in hydrogen. Two cathodes of each metal were used, one which had been before used in air and one fresh one. No difference in the behaviour of the two was observed. In order to be able to disintegrate more cathodes at one filling of the apparatus with hydrogen, a second tube of the form A was added. The hydrogen was produced electrolytically and freed from oxygen by passing it through an alkaline pyrogallol solution.

Before each series of observations the whole apparatus was several times pumped out and partially filled with hydrogen. Notwithstanding this, it is hardly to be assumed that the tube contained only pure hydrogen, especially as it was not possible to heat the apparatus during the pumping. Small quantities of oxygen undoubtedly adhered to the walls and to the cathodes, and were set free when the discharge passed. At high potentials, when the cathode becomes heated, oxygen must be certainly given off, especially in the cases of the more easily oxidized metals. Nevertheless, the deposits of

TABLE IV.—Disintegration in Hydrogen.

p (mg. Hg).	V (Volts).	y (mg. $\times 10^{-2}$).	p (mm. Hg).	V (Volts).	y (mg. $\times 10^{-2}$).
Gold.			Copper.		
0·55	1020	112			
0·53	1060	144	0·55	1250	31
0·41	1330	126	0·54	1420	57
0·44	1520	142	0·39	1790	52
...	1780	178	0·41	2070	68
0·36	1800	191			
Silver.			Nickel.		
0·56	1130	121			
0·53	1300	119	0·57	1220	20
0·50	1410	95	0·44	1670	23
0·42	1470	124	0·45	1810	22
0 44	1660	143	0·40	1930	8
0·42	1680	142			
Platinum.			Iridium.		
0 53	1280	54			
0·45	1430	82	0·53	1100	18
0·39	1890	83	0·39	1660	19
0·37	1930	92	0·36	1700	15
...	2090	74	0·36	2000	18
Palladium.			Rhodium.		
0·52	1110	38			
0·46	1350	48	0·46	1140	16
0 39	1720	51	0·39	1590	10
0·36	1840	47	0·36	1770	13
0·37	1930	86	0·36	1960	9
0·30	2200	82			

gold, silver, copper, and nickel on the tube-walls now appeared in the colours of the pure metals without oxide. In the case of the platinum metals the deposit was in part black, especially in the cases of iridium and rhodium. But it is quite possible that these metals in a finely divided condition would give this colour.

On the assumption that the disintegration in air is caused by oxidation *, it seems quite possible to suppose that the somewhat irregular disintegration in hydrogen, which is

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* The discovery of E. Goldstein (*Ber. d. Deutsch. Chem. Ges.* xxxvi. p. 3042, 1903) that the oxygen is practically all transformed into ozone during the discharge, is of interest in this connexion.

considerably smaller than in air, is produced by the traces of oxygen remaining.

The observations in Table IV. show no regular increase of loss of weight with increasing cathode-fall. In several cases a limit seemed to be reached at a comparatively low potential, beyond which there is no increase in disintegration. The loss in weight of silver and nickel, which is approximately constant, agrees fairly with Faraday's law, while iridium and rhodium disintegrate much less.

Physikalisch-Technische Reichsanstalt,
        February 1904.

XVI. *Conduction of Electricity through High Vacua, under the influence of Radioactive Substances.* By Hon. R. J. STRUTT, *Fellow of Trinity College, Cambridge* *.

IT is a matter of no great difficulty to detect the negative charge carried by the β rays of radium ; but all attempts to observe directly the positive charge carried by the α rays have hitherto been unsuccessful. I have recently made some experiments on the subject, which have satisfied me that even in high vacua there is a loss of electricity from a charged body, in presence of the α rays, independent of traces of residual gas. This effect necessarily defeats any attempt to detect the charge of the α rays.

A rod of bismuth made active by a deposit of "radio-tellurium" was used. This emitted α rays only, and thus any complication due to the negative charge of the β rays was avoided. A further advantage was the absence of any gaseous emanation. The rod was attached to an electroscope, and the whole arrangement hung up by an insulating support, in a vessel which could be exhausted very much after the manner described in a former paper (Phil. Mag. Nov. 1903)†. By means of an iron wire, movable by an external magnet, the insulated system could be charged up when desired. The position of the leaf was read by a microscope, as usual. The apparatus was connected to a Töpler pump, a manometer, and a Röntgen focus-tube, which latter served to show when a really good vacuum had been attained.

The system was charged, and the rate of loss of the charge measured at various pressures. At pressures of from 300 mm.

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* Communicated by the Author.
† Some experimenters have experienced difficulty in projecting that apparatus on the screen owing to radiometer effects. I have not found this, using a limelight, and a thick alum cell to absorb heat.

down to 2 mm. the rate of leak was found to be nearly pro-
portional to the pressure, in accordance with the results of
previous observers.   But, when the pressure was lowered
beyond that, a different state of things began to be apparent.
The rate of leak diminished more and more slowly, apparently
reaching a limit.   Exhaustion was continued until the vacuum
was so high that a discharge could not be forced through
the Röntgen tube attached.   Even then a considerable
leakage was observed from the charged system.   This leakage
was of about the magnitude which would correspond to
$\frac{1}{4}$ mm. pressure if the law of proportionality, which held
good at higher pressures, were applicable to low ones.   This
leak was the same whether the charge was positive or negative.
It is certain, therefore, that the current carried by the α rays
must be small in comparison with it.   The leak which occurs
in high vacua must be quite distinct from the ordinary leak
due to ionization of the gas ; for the latter, at a pressure as
low as that used in these experiments, would be at least a
hundred times smaller.   The nature of this conduction at
high vacua is a difficult problem.   It is very repugnant to
modern ideas to believe that the current is carried independ-
ently of moving ions ; as these cannot be derived from the
gas, the only alternative seems to be that they are derived
from the material of the radioactive substance, being torn
away from it by the issuing α particle.

XVII. *A Theory of Solutions.   By* Professor J. Traube[*].

WHILE it is true that the theory of electrolytic disso-
ciation has at all times had many admirers, it cannot
be denied that the same theory has also always had a great
many adversaries[†] who, in spite of all its success, have felt
themselves unable to accept it.

  There are many objections to this theory.   Firstly, we
are to believe that the ions attract each other by *electric* but
not by *chemical* forces ; there we create a distinction which
does not exist, for if we follow Faraday, Helmholtz, and
others[‡], the chemical forces are entirely, or in a great part, of
electric nature.

  Further, Arrhenius tried to support his theory by remark-
ing on the additive qualities of the dissolved electrolytes.   But

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  [*] Communicated by the Author.
  [†] Compare, among others, J. Traube, *Chemiker Zeitung*, xxvi. No. 90
(1902), and Wied. *Ann. Phys.* lxii. p. 504 (1897) ; Kahlenberg, *Journ.
Phys. Chem.* v. p. 339 (1901).
  [‡] Ebert, Wied. *Ann. Phys.* l. p. 255 (1893).

even if we leave out of account the known facts that volume, refraction, &c., are also additive properties in the homogeneous state, it is certain that the earlier hypothesis of Clausius of dissociation in solutions explains that additive quality of electrolytes as well as the hypothesis of electrolytic dissociation without contradicting in so many points the views of the chemists. Even the idea that in analytic solution the ions are especially reactive is not a general one, for metal potassium decomposes water, and ion potassium does not react in spite of the great electric charge.

There are only gradual differences in electric conductivity between dissolved electrolytes and nonelectrolytes. Even an aqueous solution of alcohol, ether, or sugar shows a molecular conductivity increasing with increasing dilution. In consequence of this fact, we are obliged to imagine that there are present in the solution free ions such as C_2H_5, O, &c.* In the same way we cannot believe that melted salts contain a great many free ions, and the fact that the osmotic pressure diminishes in many concentrated solutions with growing percentage† awakens some doubts against the theory.

One of the most important objections against the hypothesis of Arrhenius is the circumstance that Guldberg and Waage's Law does not agree with that hypothesis. The known formula.

$$\frac{\gamma^2}{(1-\gamma)v} = K$$

does not hold good for solutions of strong electrolytes.

In a very high degree the theory has been shaken since the investigations of osmotic and conductive phenomena have been extended to solvents different from water such as SO_2, NH_3, CH_3OH, &c. There is in most cases no agreement between van't Hoff's values of i calculated by the osmotic and by the electric method ‡.

In many cases the osmotic method shows an association of

* Walden, *Zeitschr. Physik. Chem.* xliii. p. 385 (1903). This author concludes from investigations of electric conductivity of solutions containing Br_2, I_2, PBr_3, SBr_2, &c., that even such substances are dissociated into ions.

† Kahlenberg, *l. c.*

‡ Walden, *Zeitschr. Physik. Chem.* xl. p. 513 (1902); *Ber. d. d. chem. Ges.* xxxiv. p. 4185 (1901), and xxxv. pp. 1764 & 2018 (1902); *Zeitschr. f. anorg. Chem.* xxv. p. 209; xxix. p. 371; xxx. p. 145; Carrara, *Gazz. chim. Ital.* xxvi. p. 119 (1896), and xxvii. p. 207 (1897). Kahlenberg & Lincoln, *Zeitschr. f. Elektrochem.* v. p. 544 (1899). Franklin & Krauss, *Amer. Chem. Journ.* xx., xxi., xxiii., and xxiv. (1898–1900). See especially the computations by Jones, *Zeitschr. Phys. Chem.* xxxi. p. 137 (1899), and Reychler, *Les Théories Physiko-Chimiques Brüssel,* p. 278 (1903), &c.

molecules, though we might infer from the electric conductivity that the dissolved body was strongly dissociated. **Again**, the concordance between conductivity and affinity (? *Affinitätskonstant*) of acids and bases is often missing, and on the other hand it is known that the bases follow one another in the same series of affinity as in water in other solvents, where an electrolytic dissociation cannot occur *.

These and other reasons show that the doubts against the fundamental idea of Arrhenius are justified ; notwithstanding, the success of that hypothesis is so enormous that we may ask : How can it happen that a theory based upon false assumptions has had such an influence upon the development of physics and chemistry ?

I think the answer is not difficult.

The reason for the considerable progress of the theory by Arrhenius in comparison with the former theories, especially that of Clausius, is to be found in the circumstance that Arrhenius has proved the existence of two sorts of particles in the solution of an electrolyte which have different values in reference to the most different properties, and above all that he has calculated the relative quantity of both particles.

The calculation of the coefficient of dissociation is the undisputable merit of the Swedish author, but the interpretation of that coefficient we should certainly reject.

By comparing Arrhenius's theory with the former theory of Clausius, we find the following difference in the respective assumptions :—

Both Clausius and Arrhenius assume dissociations in a solution of electrolytes, but according to Clausius every dissociation of a molecule is accompanied by an association of another even in very diluted solutions, while Arrhenius assumes nothing but dissociations in diluted solutions. The idea of Clausius was in agreement with all the views of the chemists, not so the idea of Arrhenius, but the last one seemed to explain in a very simple manner the osmotic and electrolytic behaviour of the solutions.

Let us try if we cannot obtain the advantages of Arrhenius's theory by returning to and improving Clausius's assumptions.

While at first, after the advance of the osmotic theory, the effect of the solvent often was quite neglected, there is to-day nobody who does not admit the great influence of the solvent on the properties of solutions. That influence is quite specific for every solvent. The different dissociating power of the solvents, the different quantity of heat produced or disappearing especially at the solution of a salt which may

* Goldschmidt & Salcher, *Zeitschr. Phys. Chem.* xxix. p. 89 (1899).

or may not contain water of crystallization, the coefficient of temperature of the electric conductivity*, the phenomena of contraction of volume and many others †, show that the process of solution is not only a purely physical one, but that there exist specific attractions among the particles of the solvent and of the dissolved body which will surely cause a combination of both. There would be no special reason why a salt such as NaCl should be dissociated, or why the heat of dissociation of so many electrolytes should be positive, unless we assumed such a combination of the ions with the particles of the solvent.

We do not suppose that there exist such stable hydrates as in the solid state, but we think that the combination is of a very mobile kind so that a molecule or an ion of the dissolved body wanders from one molecule of the solvent to another. There is nobody who to-day would not subscribe to these assertions.

According to the kinetic theory a liquid is in equilibrium with its vapour, if in every moment the number of the molecules which pass from the vapour to the liquid is the same as the number of molecules of the liquid which evaporate. The vapour-pressure of a liquid, for example of a solvent, is in the first place proportional to the number of evaporating particles. There may be dissolved in the solvent a non-volatile body, for instance n molecules of the same to N molecules of the solvent, and we may suppose that each of those n molecules joins with a particles of the solvent. In this case an particles of the solvent are prevented from evaporating. Since the vapour-pressures of the solvent f_0 and of the solution f are proportional to the numbers of the particles which are able to evaporate, we get the equation

$$\frac{f}{f_0} = \frac{N - an}{N} \quad \text{or} \quad \frac{f_0 - f}{f_0} = \frac{an}{N}.$$

Choosing for dissolved nonelectrolytes $a = 1$, for electrolytes with 2 ions $a = 2$, &c., *we get the well-known law of Raoult, and from that all the other laws of the osmotic theory.*

We have to suppose here that one molecule of sugar, urea, &c., always wanders from one molecule of water to another, and is combined at every moment with one molecule of the solvent ; 1 molecule of NaCl &c. ought to be combined with 2 molecules of water either dissociated or not.

We are indebted to Prof. Poynting‡ for those ideas by

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* F. Kohlrausch, Proc. Roy. Soc. lxxi. p. 338 (1903).
† Hantzsch, *Chem. Centrbl.* ii. p. 922 (1902); Bogdan, *ibid.* i. p. 71 (1904); and Traube, Wied. *Ann. d. Phys.* lxii. p. 490 (1897).
‡ Poynting, Phil. Mag. [5] xlii. p. 289 (1896).

which he has proved that the osmotic laws are to be understood by simple kinetic considerations. But the objection was made that the existence of nothing but monohydrates, bihydrates, &c., seemed highly improbable ; and surely this objection would be justified did not this idea seem much more probable by the following considerations :—

Dutoit and Aston*, Dutoit and Friderich †, and Jones ‡ have shown that *the more a solvent is associated the greater is its power to dissociate the associated molecules of another associated liquid dissolved in it and to dissociate the molecules of an electrolyte into its ions.*

This fact is only to be understood by supposing *that the process of dissociation of an associated molecule or the process of ionization of a single one is connected with dissociations of double molecules of the solvent in such a way that every single molecule arising from the dissociated double molecule of the dissolved body joins with one single molecule of the solvent, and that every ion which results from the dissociation of the electrolyte joins with one single molecule of the solvent.*

Dissolved molecules of electrolytes and of associated bodies are able to dissociate the associated molecules of a solvent and to join with them. Jones's investigations especially, *l. c.*, show that experiment has affirmed these assertions.

A solvent, as water, may be considered as a solution of probably some single molecules in a great many complex molecules. If we dissolve an associated liquid like alcohol, the double molecules of alcohol are dissociated (compare Jones) ; and it is probable that at first the existing single molecules of water join with them in such a way that one molecule of alcohol wanders from one molecule of water to another. But then the equilibrium of the single and double molecules of the solvent is disturbed, and the further dissolved molecules of alcohol bring about the formation of new single molecules of water by dissociation of complex molecules of the solvent.

We find quite the same considerations if we reflect over the process of dissolving an electrolyte, only with the difference that here the ions, for instance Na and Cl, wander from one single molecule of water to another, and are able in the moment of their formation to dissociate the double molecules of the water.

Thus we get to the same results as by Poynting's equation. Considering an associated solvent as a solution, and taking it

  * Dutoit & Aston, *Compt. Rend.* cxxv. p. 240 (1897).
  † Dutoit & Friderich, *Bull. Soc. Chim.* [3] xix. p. 321.
  ‡ Jones, *Boltzmann-Festschrift* (Leipzig, Barth), p. 105 (1904).

for granted that only single molecules of the solvent are able to join with the ions or the single molecules of a non-electrolyte, and finally reflecting that the process of dissociation of the dissolved body is connected with the dissociation of the complex molecules of the solvent, we understand why *one* ion or *one* molecule respectively is united with *one* molecule of the solvent.

Thus the osmotic laws are the result of these considerations founded on empirical investigations.

We suppose, like Arrhenius, that the molecules of a dissolved electrolyte dissociate, but there is a great difference between our views. We believe that even in the most diluted solutions in one moment every dissociation for instance of a molecule NaCl is followed by an association of Na and Cl. There will be a chain of dissociations and associations, and not only a series of dissociations.

Arrhenius's coefficient of dissociation is given by the relation of the number of particles of the electrolyte which are joined with 2, 3, &c. *single molecules of the solvent and the number of its particles which are joined with one molecule of the solvent or none.* We think it is not improbable that even at the moment when the ions are joined every ion takes hold of its particle of the solvent, excepting in stronger solutions where it will occur that 2, 3 ions or one molecule of them have only one single molecule of the solvent at their disposal.

In my former publications I have shown that the phenomena of contraction which arise by dissolving different bodies in water support these ideas. The contraction of one gramme-molecule of the most different nonelectrolytes (non-associated) in water seems to be nearly the same, the contraction produced by a gramme-molecule of a binary electrolyte reaches nearly double the amount, &c. But further investigations are desirable in this direction.

We think that we can explain by our theory many phenomena better than by that of Arrhenius. Take the case of the catalysis of a solution of sugar by acids. Only the single molecules of water are " active," or able to invert the molecules of sugar, and the number of these active molecules increases with rise of temperature, and especially in the presence of an acid which according to its intensity increases the number of the active molecules of the solvent.

But in the first place we shall try whether we cannot succeed in deriving the known formula of Rudolphi-van't Hoff [*] $\dfrac{\gamma^3}{(1-\gamma^2)v} = K$ with the help of our theory, for every

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[*] Rudolphi, *Zeitschr. Physik. Chem.* xvii. p. 385 (1895), and van't Hoff, *ibid.* xviii. p. 300 (1895).

one knows that Ostwald's formula $\dfrac{\gamma^2}{(1-\gamma)v} = K$ by no means agrees with the behaviour of strong electrolytes.

Let us suppose that there is a binary salt, as NaCl, dissolved in water ; then we get from Guldberg-Waage's law the known equation

$$\frac{\gamma^2}{(1-\gamma)v} = K.$$

Further, we may assume that water consists of double and single molecules, then there will be an equilibrium which is characterized by the validity of a corresponding formula $\dfrac{\gamma_w^2}{(1-\gamma_w)v_w} = K_w$, where γ_w is the coefficient of dissociation of double molecules of water into single ones, v_w means the volume of water, and K_w is the constant of equilibrium.

If we dissolve a salt and assume that the ions of the same (whether dissociated or not) are able to join with the single molecules of the solvent present and to dissociate the double molecules, the equilibrium of the double and single molecules of water evidently will be changed, and truly the hypothesis [*] is justified that the change of the constant K_w is *proportional to the concentration of the salt-ions* $\dfrac{\gamma}{v}$, *and therefore*

$$K_w = K'_w \frac{\gamma}{v}.$$

If we compare on the other hand different solvents, it is certain that solvent will have the largest dissociating power in which the number of the single molecules and especially the tendency for their formation is greatest. Therefore the change of the constant K for the dissociation of the salt is approximately proportional to the concentration of the single molecules of the solvent, that is $\dfrac{\gamma_w}{v_w}$ and consequently $K = K' \dfrac{\gamma_w}{v_w}$.

Water is the most associated liquid. By that we are justified in supposing that the number of the existing single molecules is very small [†] (Jones *l. c.*, and others) ; therefore the difference in the result is not large, if we put $1-\gamma_w=1$. Then we get

$$\frac{\gamma_w^2}{v_w} = K_w \text{ or } \gamma_w = \sqrt{K_w v_w} ;$$

and since γ_w is $= \dfrac{K v_w}{K'}$ the formula results

$$\frac{K v_w}{K'} = \sqrt{K_w v_w} ;$$

* Euler, *Zeitschr. Physik. Chem.* xxviii. p. 619 (1899), and xxix. p. 603 (1899).
† It is possible that the tendency to their formation is very great.

and therefore
$$K = \frac{K'\sqrt{K_w}}{\sqrt{v_w}}.$$

We found $K_w = K'_w \dfrac{\gamma}{v}$, therefore we get

$$K = \frac{K'\sqrt{K'_w}\sqrt{\gamma}}{\sqrt{v_w}\sqrt{v}};$$

and according to the equation $K = \dfrac{\gamma^2}{(1-\gamma)v}$ there follows

$$\frac{\gamma^2}{(1-\gamma)v} = \frac{K'\sqrt{K'_w}\sqrt{\gamma}}{\sqrt{v_w}\sqrt{v}};$$

from which we derive

$$\frac{K'\sqrt{K'_w}}{\sqrt{v_w}} = \frac{\gamma\sqrt{\gamma}}{(1-\gamma)\sqrt{v}}.$$

Since v_w the concentration of water is in diluted solutions nearly constant, we may contract the value $\dfrac{K'\sqrt{K'_w}}{\sqrt{v_w}}$ into one constant, and by squaring results

$$\frac{\gamma^3}{(1-\gamma^2)v} = C,$$

the equation of Rudolphi-van't Hoff.

Some of our assertions were only approximate, and therefore that equation cannot have an exact validity.

Finally, let us suppose that the nonelectrolytes and the weak electrolytes are not able to dissociate the double molecules of the solvent, and that therefore the equation

$$\frac{\gamma_w^2}{(1-\gamma_w)v_w} = K$$

is not changed, then we can understand that in this case the equation $\dfrac{\gamma^2}{(1-\gamma)v} = K$ is the right expression for the facts.

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XVIII. *On Coherence and Recoherence.*
By P. E. Shaw, *B.A., D.Sc., and* C. A. B. Garrett [*].

WHEN electromagnetic waves fall on a loose contact of two metallic or other conducting surfaces, coherence occurs between the surfaces whether there be a completed metallic circuit or not. The resistance of the contact falls, and the surfaces cling together and can only be

[*] Communicated by the Physical Society: read May 27, 1904.

sundered by forces which are small, but measurable. The best proof that coherence has taken place is, not that the P.D. between the surfaces has fallen (for this always occurs when the pressure between the surfaces is great), but that definite force is necessary to sunder them, always provided pseudo-coherence is avoided*.

In this paper coherence is tested and measured solely by the sundering forces. In a former paper† one of us showed how, by taking the simplest case of two surfaces only, it is possible to measure the forces necessary to sunder them when cohered. It was also shown that the surfaces recohere, *i. e.* show a power of clinging when brought together after coherence has been broken, and that this clinging power is comparable with that occurring in the original coherence.

In the present paper the experimental investigation is carried further than before, and an attempt is made at explanation.

Fig. 1.
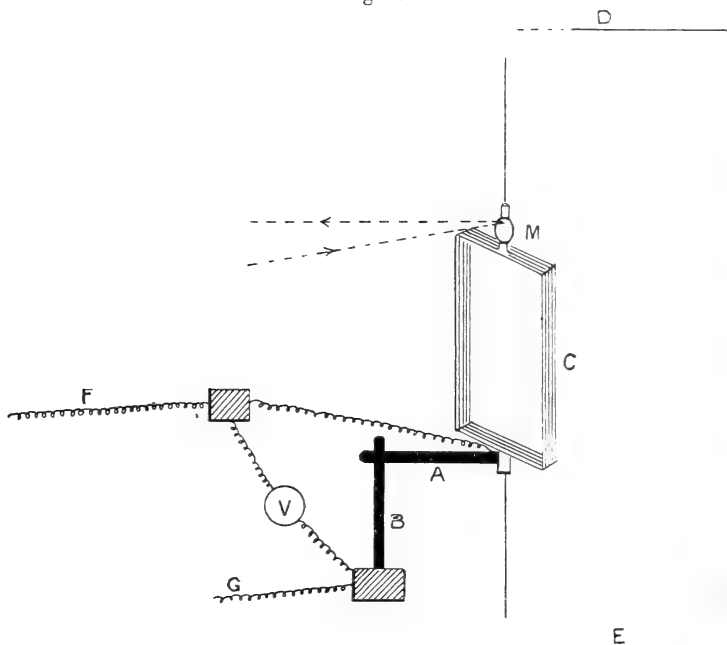
Apparatus.

Two forms are used.

I. *The Boom Apparatus* (fig. 1).—Similar to an apparatus

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* See Shaw, Phil. Mag. March 1901, p. 271.
† Shaw, Phil. Mag. March 1901.

described in the former paper. The suspended coil C of a D'Arsonval galvanometer is suitably wound, and to its frame is soldered a boom A of copper wire. The boom projects a short distance and is perpendicular to the coil. A similar wire B is vertical and is long enough to make contact with A as shown. Coherence is produced at the single contact where A and B meet. M is a mirror by which a spot of light is made to indicate the position of the boom. The suspensions of the coil are of thin strip phosphor-bronze, and the connexion between F and A is a long loose piece of thin phosphor-bronze. D and E are leads for the coil. The circuit is completed through a battery and resistances (a dial pattern rheostat is used to give slow gradational changes in resistance). F and G are the leads for the contact-circuit. In this circuit we have a battery, resistances, galvanometer, and as shown a voltmeter V as shunt to the contact. The voltmeter, reading to $\frac{1}{100}$ volt, is a convenient indicator. It shows whether there is (1) full coherence, (2) partial coherence, (3) pressure between A and B without current passing, or (4) pressure between A and B with current passing.

This apparatus is very sensitive, but has the disadvantage that it is difficult to clean or remove the contact attached to the coil without breaking the delicate strips of phosphor-bronze of the suspension. The following device, though less sensitive, has no delicate suspension and is more handy.

Fig. 2.
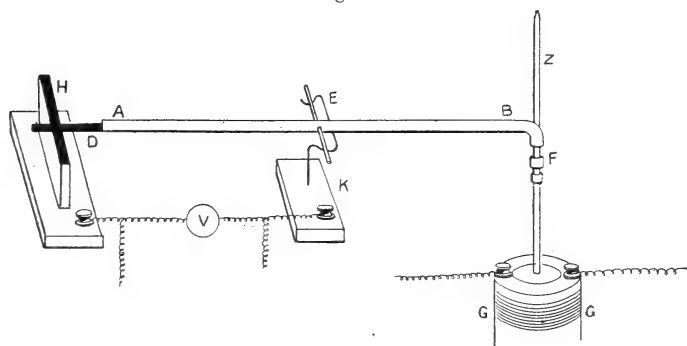
The Balance Apparatus (fig. 2).—Here DH is the variable contact. AB is a tube made of thin sheet aluminium. At B it is continued in a vertical helix F through which an ordinary sewing-needle Z is thrust. Into the end A of the tube a copper wire D is thrust. Through the point E another

fine needle is pushed, the two ends of the needle resting on two curved supports of the wire K.

By pushing D in or out of the tube, the beam can be balanced about E, and by pushing the needle up or down in F the beam can be rendered *just* stable. The beam is obviously very light, so that in AB we have all the conditions required in a sensitive balance, and the wire D can be made to rest with any small pressure on the wire H which crosses it at right angles. A coil GG is placed below the magnetized needle, so that by passing a suitable current in GG the needle is sucked into GG and the contact DH is broken.

It is easy to see how by varying the current in GG the pressure between D and H can be changed at will.

The voltmeter V is shown shunting the contacts as before. Whether we use Apparatus I. or II., it must be placed on a table supported by long rubber springs, to avoid extraneous vibration. This is indispensable, since even a small shock breaks the coherence.

Each apparatus has to be roughly calibrated for force, so that knowing what current is put on in the actuating coil GG (fig. 2) we may convert it into a known thrust or tension felt at the contact, according as the current is direct or reverse. In the former paper the method of calibrating the boom apparatus was described. For the balance apparatus calibration is easy: we put a rider of known weight on the balance itself at such a place that its effect is equal to that of the current used in GG.

Coherence.

Possible explanations of the force with which two surfaces cling when cohered are :—

1. *The fusion theory* (Lodge).—The alternating discharge from one surface to the other develops heat enough to fuse the solid surfaces, so that after the discharge a strong solid bridge remains.

2. Branly's *theory*.—The medium between the contacts undergoes a modification under the electromagnetic waves, which causes it to conduct more or less well.

3. *The adhesion theory* (Auerbach).—Mechanical shock accompanying the discharge causes the particles to come into and remain in more intimate contact.

4. *The ionic theory*.—Either a film condensed from the atmospheric gases or else an oxide film is ionized by the passage of the discharge ; so that positive and negative charges will exist in or on the opposed films causing them to attract

each other electrostatically, and also causing the resistance of the contact to fall.

It is quite possible that there may be complications, *i. e.* two or more of the above effects may occur at the same time; thus both adhesion and ionization may occur and conspire to produce coherence.

One point must be borne in mind in these contact phenomena, viz., the part played by the films on the contact surfaces. The contact between the two metal surfaces cannot pass until these films have been penetrated. The researches of Blondel*, Van Gulik†, Sundorph‡, Guthe & Trowbridge§, Shaw ‖, all show this point clearly.

It is not easy to say, in a general case, how much of these effects is due to each of the following: (*a*) condensed air, (*b*) condensed water-vapour, (*c*) oxide on the metal. For this reason, an uncleaned surface often behaves in a most contradictory way. If the movable surface meets the other impulsively, the films are penetrated and a current passes; whereas a gentle approach results in the films remaining intact and no current passes; and even if the surfaces be pushed together by reversing the current in the coil (fig. 2), it requires a great steady force to bring the metals into contact. In one case, using the balance apparatus, the force was measured, and found to be about 20 dynes. This is a very large force, since $\frac{1}{2}$ dyne is the force with which the surfaces are usually pushed together. It is hard to believe that any film other than a solid one could offer such resistance. Again, the voltmeter reading is not so directly related to the strength of coherence as would be expected if metal and no film existed at the contact.

By scraping and drying the surfaces we can get rid of the films of condensed water-vapour and of oxide, and it is then found that, for several minutes at least, the effects are quite simple.

In order to assist us in deciding between the rival theories, we will give two crucial experiments.

(*a*) Suppose the two contact surfaces, in either of the above forms of apparatus, are brought together gently so that the films are not penetrated by an impulsive blow : *no current will pass.* When a spark is produced near them, coherence occurs : this shows that the surfaces are not more than about

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* *Éclairage Électrique,* 1892. † Wied. *Ann.* 1898.
‡ Wied. *Ann.* 1899. § Phys. Rev. July 1900, p. 22.
‖ Phil. Mag. March 1901.

12×10^{-6} cms. apart[*]. It might be supposed that the P.D. for the steady current between the surfaces, being small, will cause a spark to pass if the film separating them is so thin. But the P.D. is sometimes only 1 volt, and never more than 6 volts. Until recently, the discharge distance for less than 40 volts had not been measured; but one of us has been able to show that for 6 volts the distance is about 6×10^{-6} cms., and for 1 volt is 1×10^{-6} cm. These results show us what is the superior limit to the thickness of film existing when current passes through the contact. Suppose the P.D. between the contacts is 6 volts, then, as shown, the film between the contacts must be more than 6×10^{-6} cm. thick, and when discharge occurs the electric waves burst over this interval.

(*b*) Now sunder the surfaces; clean and dry them. Bring them together gently as before: *the current now passes readily.* On producing a spark, it is found that coherence will either be slight or non-existent. Thus a high resistance of the contact is a necessary antecedent condition of coherence. We may conclude either (1) that the film acts as a flux assisting the fusion of the two surfaces, or (2) that the ionization of the film into positive and negative charges causes attraction of the surfaces, or (3) that there must be some gap between the surfaces; this gap provides a large resistance so that enough heat is developed by the passage of the waves to fuse the surfaces. The last theory can be proved experimentally to be correct, for it is possible *after cleaning the surfaces* to bring them together with such slight pressure that the current passing is small. On passing a spark near the coherer, coherence readily occurs, as shown below.

TABLE I.

	Force in dynes to sunder surfaces, F.	Voltmeter Reading, V.
Before spark.........	·5 dyne	0·25
After ,, ...	$1 \cdot 12 \times \cdot 5$,,	0·10
Before spark.........	·5 ,,	0·30
After ,, ...	$1 \cdot 12 \times \cdot 5$,,	0·10
Before spark.........	·5 ,,	0·30
After ,, ...	$1 \cdot 10 \times \cdot 5$,,	0·12

Thus coherence occurs even when no film existed previously, provided there is a gap between the surfaces. In the former

* *Loc. cit.* p. 291.

paper * it was shown, by using the electric micrometer, that
when coherence is broken the surfaces part with a "snap"
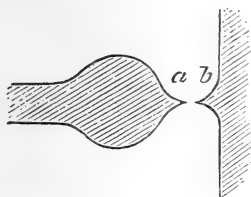
which sounds in the telephone like
the sundering of a solid body, and
that when the surfaces are pressed
together again, the ends of the
bridge *ab* (formed when the surfaces
were severed) are found and can be
easily pressed back flat.

These facts show that coherence
is a solid effect, and dispose of the
ionization theory and also of Branly's theory. It was shown
in the former paper † that the strength and resistance of the
bridge proves it to have about the tenacity of the solid metal
(for copper $T = 2 \times 10^8$ c.g.s.). This again supports the
fusion theory; and though it does not confute the adhesion
principle, it requires that the forces of adhesion should be at
least as great as if the surfaces were fused and solidly
attached to each other. But a simple calculation of the heat
developed at the contact shows that there is ample to fuse
the metal. This disposes of the simple adhesion theory; and
leaves only the fusion theory, which seems in entire accord
with all facts known.

Recoherence.

After the cohered surfaces have been sundered, if they are
brought together again they often cohere with considerable,
though much less, force than in coherence. This can be
repeated sometimes once or twice (without further sparking),
but the power to recohere soon leaves the surfaces, and they
return to the normal state of inability to cling together.
This is shown in three distinct cases below. F is the force
in dynes required to sunder the surfaces before coherence;
F_1 is the force directly after coherence; F_2, F_3 . . . , are
successive values, showing recoherence.

TABLE II.

	Case 1.	2.	3.
F...............	·5 dyne	·5 dyne	·5 dyne
F_1	$5 \times ·5$,,	$4·5 \times ·5$,,	$13 \times ·5$,,
F_2	$3·7 \times ·5$,,	$4·5 \times ·5$,,	$8 \times ·5$,,
F_3	$4·6 \times ·5$,,	$1·1$ ·5 ,,	·5 ,,
F_4	·5 ,,	·5 ,,	·5 ,,
F_5	·5 ,,	·5 ,,	·5 ,,

* *Ibid.* p. 292.   † *Ibid.* p. 282.

Thus after a few times the surfaces become normal and show no signs of clinging. This is quite a different effect from coherence, for there is now no rapidly alternating current passing through the contact causing coherence by fusion. There seems evidence here that the particles have been *orientated* by the powerful currents at the contacts when they were cohered. The orientation must be due to the coherence and not to the sundering, for it occurs after coherence, but not after the sundering of two surfaces which have been carrying a current in the ordinary way. The current-density at the bridge between the contacts is enormous, and it may be assumed that the particles composing the bridge would be orientated in some way so as to give maximum conduction. This orientation is seen for a short time, but is unstable (just as are magnetic and other orientations) and disappears after a few makes and breaks.

The Nature of Recoherence.

Various tests applied :—

(1) Recoherence is, like coherence, a solid effect, for if the surfaces be brought together gently at once after sundering, they recohere ; but if an interval, say of half a minute, elapses, they refuse to recohere ; no doubt due to the growth of a film on the surfaces.

(2) Suppose recoherence no longer occurs for a direct current, if the current passing through the contacts be reversed recoherence often occurs and is renewed once or twice more by reversals. This is shown below in five distinct cases.

TABLE III.

	1.	2.	3.	4.	5.
	dyne	dyne	dyne	dyne	dyne
F	·5	·5	·5	·5	·5
F_1.........	8 ×·5	8·2×·5	7·4×·5	6·7×·5	
F_2.........	6·1×·5	·5	·5	·5	·5
F_3.........	·5	R 3 ×·5	·5	R 3·8×·5	·5
F_4.........	R ·5	R ·5	·5	R 6·7×·5	R 12×·5
F_5.........	R 4 ×·5	·5	R 8 ×·5	R ·5	·5
F_6.........	·5	·5	·5	1·8×·5	·5

Here R stands for reversal of current ; otherwise the current is direct. In the fourth case, after 3 trials with direct current, we have 3 with reverse and then one with direct ; each change develops recoherence (see fig. 3).

(3) If the cohered wires are sundered and left touching with no current passing for many hours, they are sometimes found to recohere readily. There is no question of film since the surfaces have been in contact throughout the long interval. In the fifth case in the above column, coherence

Fig. 3.
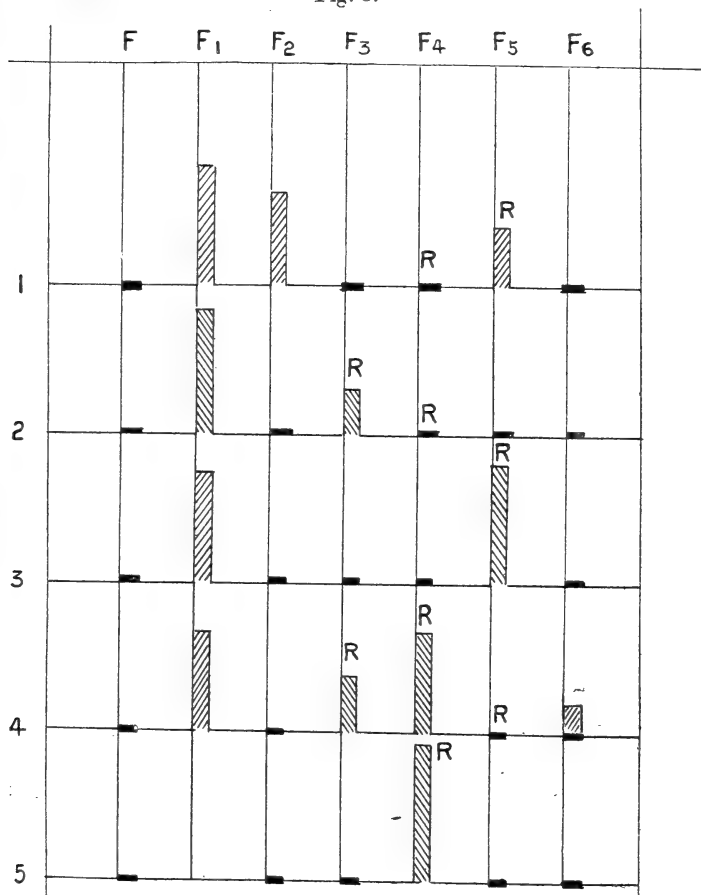
was produced and broken overnight and the wires left in contact. In the morning the values F_1, F_2 ... were found.

(4) Reversal of current. By analogy with magnetism, it might be possible to "wipe out" orientation by rapid reversal of current of gradually decreasing strength. This was tried with no result.

(5) Mechanical shock and heating. Again, by analogy with

magnetism we might expect that these would remove orientation. But it is very difficult to draw any sure conclusion from such experiments, for the first is liable to disturb the delicate balance and perhaps change the surfaces of contact, whereas the second if carried far is sure to produce abundant oxide-film troubles. No conclusion could be drawn from the experiments made by these agencies. It is, however, very likely that recoherence dies away on account of the shock when the surfaces are brought repeatedly together.

(6) The action of radium was tried without definite result.

To summarize, Coherence can be best explained by the fusion theory. Recoherence is due to residual coherence, the clinging of the surfaces being apparently an adhesion of the particles (somewhat after the theory of Auerbach). It is hard to see how recoherence can be explained, except by some change in or orientation of surface particles occurring in the violent commotion of coherence.

XIX. *On the Diffraction Images formed by a Plane Diffraction-Grating. By* ALFRED W. PORTER, *B.Sc., Fellow of, and Assistant Professor of Physics in, University College, London* [*].

THE following problems all relate to the simplest kind of diffraction-grating consisting of alternate transparent and opaque strips, and the incident light is supposed to be normal. The intensity of the image formed at the focus of the observing telescope (adjusted for infinity) may be written, as usual,

$$y^2 = F_1^2 F_2^2 = \left(\frac{\sin u}{u} \cdot \frac{\sin nv}{\sin v} \right),$$

where n is the number of transparent strips,

$$u = \frac{\pi}{\lambda} r \sin \theta, \quad v = \frac{\pi}{\lambda} (r + s) \sin \theta,$$

$\theta =$ direction of observation, and r and s are widths of transparent and opaque strips respectively.

PROBLEM A.—*The criteria for positions of maxima and minima.*

These positions are usually found by equating to zero the first differential coefficient of F_2^2 with regard to v. This

coefficient is

$$\frac{d\mathrm{F}_2^2}{dv} = 2\frac{\sin nv}{\sin v}\left[\frac{n\cos nv\sin v - \sin nv\cos v}{\sin^2 v}\right].$$

The principal maxima are given by $\sin v = 0$, which is said to make the coefficient discontinuous (*e. g.* Jamin et Bouty) ; the secondary maxima are said to be given by the equation $\tan nv = n\tan v$; and the secondary minima by $\sin nv = 0$ (unless $\sin v = 0$). The object of this note is to point out that $\sin v = 0$ makes the coefficient *zero* and not discontinuous, and that the condition given universally for the secondary maxima is incomplete.

(i.) Take the condition for the secondary maxima first. In reality, when n is *odd* the condition given is incomplete, for it does not include the maximum midway between two principal maxima. For example, when $n = 3$, although there is a secondary maximum whenever $v =$ odd multiple of $\frac{\pi}{2}$, there is *no* value of v satisfying

$$3\tan v = \tan 3v,$$

except even multiples of $\pi/2$, which give the principal maxima.

[In the neighbourhood of $v = \frac{\pi}{2}$ it should be noticed that $3\tan v = 9\tan 3v$.]

The fact is the differential coefficient also vanishes when $\cos nv$ and $\cos v$ are simultaneously zero, and this condition corresponds to the neglected maxima.

The conditions can, however, be more neatly stated if the differential coefficient be written in one of the two following forms :—

$$\frac{d\mathrm{F}_2^2}{dv} = 2\frac{\sin nv}{\sin^3 v}\left[(n-1)\sin(n+1)v - (n+1)\sin(n-1)v\right]\quad(\alpha)$$

$$= 2\frac{\sin^2 nv}{\sin^2 v}\left[n\cot nv - \cot v\right].\quad\ldots\ldots\quad(\beta)$$

The coefficient can be proved to vanish whenever

$$(n-1)\sin(n+1)v - (n+1)\sin(n-1)v = 0,$$

and this gives *all* the maxima (principal and secondary). It can also be proved to vanish whenever

$$n\cot nv - \cot v = 0,$$

and this again gives all the maxima.

The former is more convenient for graphical representations, because no infinite values occur. The latter condition is perhaps the more convenient generally.

(ii.) When $\sin v = 0$ the differential coefficient does not equal infinity but zero. The approximate value when v approaches an integral multiple of π is

$$\frac{d\mathrm{F}_2^2}{dv} = 2\,\frac{n^2(1-n^2)\alpha}{3},$$

where $\alpha = v - p\pi$ ($p =$ integer) ; and this obviously vanishes when α is zero. Since $n^2 > 1$ it is positive when α is negative, and negative when α is positive ; the vanishing of α therefore corresponds to a maximum of F_2^2.

PROBLEM B.— *The relative intensity of the principal and secondary spectra of a diffraction-grating.*

Though this question is discussed with sufficient approximation in Schwerd, *Beugungserscheinungen*, some misconception seems to exist in respect to the degree in which an increase in the number of openings in a grating diminishes the intensity of the secondary maxima compared with the principal maxima. For example, Mascart in his *Traité* (t. i. p. 355) makes the following statement :— "Tant que $\sin^2 v$ diffère notablement de zéro, les maxima secondaires sont très faibles par rapport aux maxima principaux ; ces maxima ont leurs moindres valeurs au milieu de l'intervalle des maxima principaux et augmentent à mesure qu'ils s'en rapprochent. Dans tous les cas, le rapport . . . des maxima secondaires aux maxima principaux est d'autant plus petit que le nombre N des ouvertures est plus grand, et ces maxima finissent alors par devenir insensibles."

Again, Drude in his ' Theory of Optics ' (p. 223) states :— " Between the fringes [*i. e.* the secondary minima] the intensity J reaches maxima which, however, are at most equal to the intensities produced at the same points by a single slit."

How far these statements are true may be ascertained as follows :—

The secondary maxima are given by those values of v for which

$$\cot nv = \frac{\cot v}{n},$$

$$\text{and} \quad \therefore \quad \sin^2 nv = \frac{n^2}{n^2 + \cot^2 v}.$$

Their intensity is therefore

$$\mathrm{F}_2^2 = \frac{\sin^2 nv}{\sin^2 v} = \frac{n^2}{n^2\sin^2 v + \cos^2 v}$$

$$= \frac{n^2}{1 + (n^2 - 1)\sin^2 v}.$$

Those in which we are specially interested are the two nearest to a principal maximum. The ratios of their intensities to the intensities of the principal maxima are given below for a few cases :—

n.	nv.	Secondary/Principal.
3	$pn\pi \pm 270°$	$\dfrac{1}{9}$
4	$pn\pi \pm 263°\cdot37'$	$\dfrac{1}{13\cdot5}$
5	$pn\pi \pm 261°\cdot12'$	$\dfrac{1}{16}$
15	$pn\pi \pm 257°\cdot50'$	$\dfrac{1}{20\cdot6}$
∞	$pn\pi \pm 257°\cdot27'$	$\dfrac{1}{21\cdot2}$

Thus, the ratio is nearly the same *whatever the number of openings* provided it is greater than a good dozen ; and this ratio is never very small.

The question arises as to the reason of the evanescence of the secondary maxima for ordinary gratings. For gratings of half-a-dozen openings they are quite conspicuous, and there is no striking difference in the ratio for such a grating and for one of 10,000 lines for which they are invisible. It cannot be attributed to the lines being closer together ; for provided the gratings compared with one another have the same total aperture, the secondary maxima are very nearly indeed the same angular distance apart for all values of n if n is fairly large ; for the number between two principal maxima is $n-2$, and the distance between two principal maxima is proportional to n.

The true cause of evanescence would seem to be simply that in spectra as usually obtained even the principal maxima are only feeble. In support of this it is sufficient to mention that the secondary maxima are quite conspicuous in the neighbourhood of the direct image formed by a grating of 14,000 lines to the inch ($n=20,000$) provided that a sufficiently strong source of illumination, such as the electric arc, is employed.

PROBLEM C.—*The precise position of the principal maxima.*

The maxima usually considered are the maxima of F_2^2 ; in reality, however, it is the maxima of y^2 that are required ; thus a slight error is here committed, for u and v are dependent upon one another, and the two factors of y^2 cannot therefore be separately discussed. Although the modification produced by the correct consideration of the problem turns out

to be exceedingly small, yet, in view of the fact that measurements of spectra are now made with extreme accuracy, it is necessary to become assured that the usual treatment has the accuracy required.

We shall write, when necessary, $u = \rho v$ where $\rho = r/(r+s)$, and consequently $\dfrac{du}{dv} = \rho$.

Then

$$\frac{dy^2}{dv} = 2y^2 \left\{ n \cot nv - \cot v + \rho\left(\cot u - \frac{1}{u}\right) \right\}.$$

Now the principal maxima occur for values of v equal or very nearly equal to $p\pi$ where p is an integer; writing $v = p\pi + \alpha$ we obtain

$$\frac{dy^2}{dv} = 2y^2 \left\{ -\frac{n^2 - 1}{3}\alpha + \rho\left(\cot u - \frac{1}{u}\right) \right\}$$

approximately, α being small.

Now, in practice, we are only concerned with those cases in which $\sin u$ is considerable; for in other cases the spectra are necessarily weak. If we confine attention to those cases in which the intensity is not less than one-fourth the maximum possible, u must not differ by more than 60° on either side of an odd multiple of $\pi/2$. Hence $\cot u$ lies between $+\sqrt{3}$ and $-\sqrt{3}$, and the maxima will be given by values of α such that $\dfrac{n^2 - 1}{3}\alpha$ is not numerically greater than $\rho\sqrt{3} + \dfrac{1}{p\pi}$: whence α is not greater than $\dfrac{6}{n^2}$.

Importance attaches not so much to the actual value of α as to the relation it bears to the least detectable value of $d\lambda/\lambda$, viz. :— $\dfrac{1}{np}$, where np is the "resolving power" of the grating.

Now

$$\frac{d\lambda}{\lambda} = \frac{dv}{v} = \frac{\alpha}{p\pi}$$

which is less than $\dfrac{6}{n^2 p\pi}$, and this bears to the least detectable value of $\dfrac{d\lambda}{\lambda}$ the ratio

$$\frac{6}{n^2 p\pi} \Big/ \frac{1}{np}, \quad \text{or practically } \frac{6}{n\pi}.$$

Therefore the error committed in the usual method of calculating the positions of the principal maxima is far below the least detectable error provided n is considerable and the maximum considered is fairly bright.

XX. *On the Variation with Temperature of the Magnetic Permeability of Nickel and Iron.* By E. PHILIP HARRISON, *Ph.D.* (*Zürich*), 1851 *Exhibition Research Scholar of University College, London; King's College, Cambridge* *.

Introduction.

IT is a remarkable fact that many of the physical properties of iron, nickel, and cobalt undergo sudden changes at a temperature which is roughly particular to the metal in question, but which varies slightly with individual specimens.

For instance, Tait † found that the thermoelectric power of iron altered at a red heat and Kohlrausch and Hopkinson ‡ proved the same thing for its electric resistance, while as early as 1869 Gore had noticed a change in its coefficient of expansion.

Barrett's § more recent discovery of "recalescence," an evolution of heat which occurs in cooling steel wire at approximately the same temperature as Gore's phenomenon, is only another instance of some molecular rearrangement, which doubtless takes place in iron, at a temperature of about 800° C. But of all the phenomena connected with this structural alteration in iron at high temperatures, that which has been longest known and studied is the loss of magnetic quality at what Hopkinson called "the critical temperature" for the metal. It is natural to suppose that this magnetic critical temperature is identical with that at which changes in other physical properties take place ; a supposition to some extent confirmed by experiment.

In order, however, definitely to decide this point, it is important to use the same specimens in each experiment, since small and otherwise undetectable differences in chemical or physical state of a metal may lead to very considerable alterations in thermoelectric power, resistance, or magnetic induction.

The present experiments form the third part of a research into the temperature variations of various physical properties of iron and nickel. The first two parts ‖ dealt with thermoelectromotive force and electric resistance, and showed that

* Inaugural Dissertation for the degree of Ph.D. in the University of Zürich. Communicated by Prof. H. L. Callendar, F.R.S.
† Tait, Trans. Roy. Soc. Edin. 1873.
‡ Kohlrausch, Wied. *Ann.* vol. xxxiii. (1888).
§ Barrett, Phil. Mag. Jan. 1874.
‖ Harrison, Phil. Mag. Jan. 1902.

the changes in both variables occur at practically the same temperature in nickel, while for iron the agreement is a little less definite. A paper on the Temperature Variation of the Coefficient of Expansion of Nickel, forming a fourth part of the research, appeared in the Philosophical Magazine of June last.

Throughout this research the same specimens of metal are employed.

Previous Observations.

The fact that a magnet loses its magnetism at a high temperature was known to Gilbert in the year 1600 ; but the phenomenon appears first to have been investigated by Barlow * in 1822, and again by Faraday † twenty years after. Only qualitative results were obtained, owing to the difficulties of temperature measurement. Subsequent experimenters have been many, and include in the chronological order of their work : Rowland ‡, Baur §, Berson ‖, Hopkinson ¶, Curie **, Wills ††, Wilde ‡‡, D. K. Morris§§, and Pitcher ‖‖.

The ballistic method, with ring-specimens, has been used most frequently for measuring the induction. Berson, however, used a magnetometric method, and Wilde deduced the induction of pure cobalt at various temperatures from the weight required to detach a heated cube of the metal from an electromagnet.

In the earliest experiments the metal was heated to whiteness, and was then allowed to cool in a magnetic field, but since Hopkinson's work ring-specimens have usually been heated in a gas furnace, and their temperature calculated from the resistance of the secondary winding. Morris heated his ring-specimens by means of a coil of platinum

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* Barlow, Phil. Trans. 1822.

† Faraday, Phil. Mag. vol. viii. p. 177 (1836) and vol. xxvii. p. 1 (1845).

‡ Rowland, Phil. Mag. 1874.

§ Baur, *Annalen der Physik,* vol. ii. (1880).

‖ Berson, *Annales de Chimie et de Physique,* tom. viii. (1886).

¶ Hopkinson, Proc. Roy. Soc. vol. xliv. (1888) and Phil. Trans. 1889.

** P. Curie, *Comptes Rendus,* tom. cxviii. pp. 796, 859, & 1134 (1894).

†† Wills, Phil. Mag. July 1900.

‡‡ Wilde, Proc. Roy. Soc. 1891.

§§ D. K. Morris, Phil. Mag. Sept. 1897.

‖‖ F. H. Pitcher, Phil. Mag. May 1889. Pitcher's actual investigations are concerned with the effect of circular magnetization on a longitudinally magnetized iron wire. After some preliminary experiments he appears to have abandoned a platinum tube method of heating for direct heating by current in the wire. (*Vide infra,* p. 181.)

wire, wound non-inductively round them. He was the first to avoid oxidation by heating the specimens in a vacuum.

Values of the critical temperatures obtained by these observers are:—for Nickel, about 320° ;

for Cobalt, about 1100° ;

for Iron, between 770° and 870°.

General Method Employed.

In the present investigation, the iron and nickel specimens are in the form of straight wires, 20 cms. long. A magnetometric method is used for determining the induction, and the specimens are inclosed in an electrically heated platinum tube, the temperature of which is deduced from its resistance [*]. This method was suggested by Professor Callendar and described by Pitcher in the beginning of his paper (Phil. Mag. May 1899).

Description of Apparatus.

The magnetizing solenoid is 50 cms. long, and consists of two layers of double silk-covered copper wire (diam. 0·0356 cm.) wound on a brass tube (M, fig. 2) 2·68 cms. diam. The layers are carefully insulated from each other and from the brass tube by means of paraffined paper, and the windings are correct to 1 per cent. The lower layer contains 17·85 turns per cm., and the upper layer 17·54 turns per cm. By means of a second brass tube, concentric with the first, an annular space is formed between the two tubes, through which cold water can circulate. This water-jacket protects the coil from the effects of the hot platinum within, and also prevents undue heating by the magnetizing current.

The magnetometer consists of a ring-magnet hanging inside a cavity in a mass of copper. Vertically beneath the ring is a bar-magnet, the position of which can be adjusted so as to allow of variation in the controlling field. The instrument is also provided with a one-ohm coil mounted axially with the ring-magnet. The specimen with its surrounding solenoid is arranged horizontally East and West, with the magnetometer a few centimetres from the end of the solenoid tube (fig. 1).

The coils C_1C_2 are in series with the heating and magnetizing currents respectively, and consist of a few turns of thick

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[*] The nickel wires were 20 cms. long and 0·0765 cm. diameter (300 diameters in length).

The iron wires were 20 cms. long and 0·0575 cm. diameter (400 diameters in length).

copper wire, wound in bobbins whose distance from the magnetometer is adjustable. The coils serve to compensate for direct action of the magnetizing and heating currents on the magnetometer.

Magnetometer deflexions are read by scale and telescope arrangement. The scale was tested by comparison with a standard, but a calibration was found to be unnecessary.

The magnetizing current (from accumulators) was generally read directly by means of a Hartmann & Braun ammeter calibrated against a standard Weston milliammeter. In some experiments, however, it was found more convenient to shunt part of the magnetizing current through the coil M (fig. 1) and to note the resulting magnetometer deflexion ; the value of 1 cm. scale-deflexion in terms of current was determined by previous experiment.

By means of a liquid rheostat the magnetizing current could be continuously varied so as to produce inside the solenoid any field from zero up to 50 C.G.S.

The value of the field controlling the magnetometer was obtained by sending a known current round a large copper ring, R, mounted as shown in fig. 1. From the deflexions produced the field could be calculated.

The value of the field was tested frequently during each set of experiments.

The Heating Apparatus.—The heating tube (*pp*, fig. 2) is of pure platinum, 30 cms. long, 2 mms. external diameter, and of 0·1 mm. wall-thickness. The wire to be examined is placed symmetrically inside it. Satisfactorily to insulate the specimen from the platinum tube was found to be a matter of considerable difficulty. Mica disks were first tried, but they proved to be very unmanageable. Finally, small tubes of silica, each about 2 cms. long, were threaded on the specimen, which with this insulating sheath slid easily into the platinum tube.

The tube is permanently soldered to hollow copper end-pieces EE′, and these screw into two copper rods CC′ (fig. 2) which form the heating-current leads.

A pushing spring, P, of brass is arranged to take up the slack of the platinum tube when the latter expands, and the whole is inclosed in a glass tube which just fits the inner brass tube of the solenoid.

The rods CC′ pass freely out of the glass tube, being supported by small brass tubes mounted centrally in rubber corks, one at each end of the glass. A clamping-screw, K, fixes C and enables the spring to be slightly compressed before the heating of the platinum is begun.

← EAST

C. Magnetometer-coil.   R. Copper field-ring.   M. Magnetometer. C_1, Compensating-coil for heating circuit.
C_2, Compensating-coil for solenoid.

SOLENOID

TO PUMP →

M

C   R C_2 C_1

WEST →

Fig. 1.
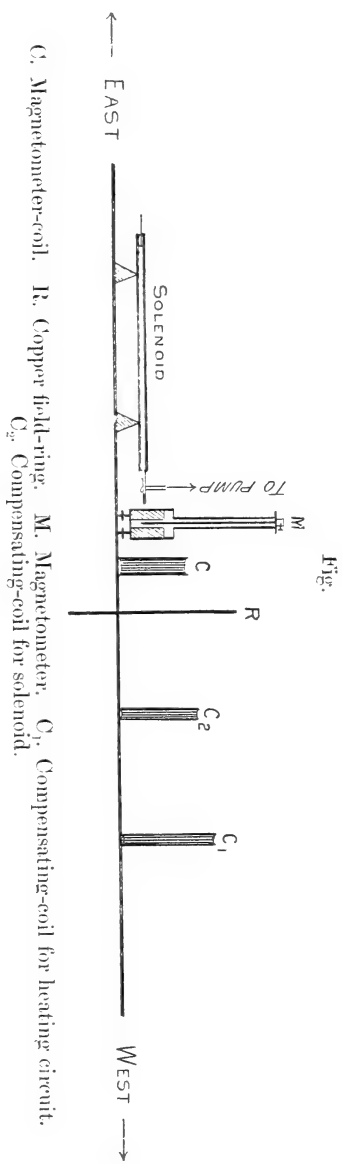
Fig. 2.

Heating Apparatus.

To Pump.
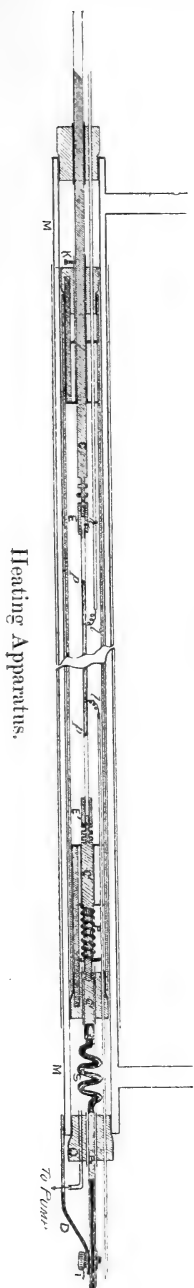
By connecting C′ and M with a brass strip D, the current returns by the solenoid tube, and thus the direct action of the former on the magnetometer is to a great extent avoided.

The heating-current, which was obtained from accumulators, was varied by means of a series of german-silver rheostats with plugs. The maximum current used was 20 amperes, and this raised the platinum tube to a temperature of about 1000° C.

In order to avoid oxidation at temperatures above 500° the specimen must be kept in a high vacuum, and this necessitated a slight modification of the apparatus. Whenever the platinum changes length, there is a corresponding movement in the rod C′, so that it was found impossible to seal up the apparatus before exhausting.

To avoid this difficulty, a second and very "slack" spring, S (made from a bundle of fine hard-drawn copper wires), is inserted between one end of the glass and the corresponding end of the brass solenoid tube ; thus any movement of C′ is taken up by S, and the portion B of the lead passes out of a cork Q (which closes the solenoid tube) and is fixed by the binding-screw T. The joints are then sealed up with pitch and wax.

Leads, $l\,l'$, of very fine platinum wire leave the platinum tube at 5 cms. from each end, and pass out along capillary glass tubes G G, the ends of which are sealed round the wire.

A mercury gauge, set up in the same reservoir as a barometer, indicates the pressure, and exhaustion is carried out by means of a mercury pump.

A small hole (h) in one of the copper end-pieces E puts the interior of the platinum tube into communication with the space to be evacuated. The platinum-tube resistance is obtained by comparison with that of a standard manganin wire, 2 mms. in diameter, which is provided with two fine copper leads, one near each end, and is connected in series with the platinum. The manganin standard is immersed in oil and surrounded by a coil of lead tubing through which cold water can circulate ; with this arrangement, the temperature of the manganin did not vary more than half a degree during an experiment.

The resistance of the platinum at various temperatures is compared with that of the "standard" by tapping off part of the current, by means of the fine leads $l\,l'$ and $g\,g'$, and putting each in turn in circuit with the magnetometer-coil C (fig. 1). The deflexions produced are proportional to the resistances of the portions AB, CD (fig. 3), provided that

the two secondary circuits AB ll', CD gg' have been previously adjusted as nearly as possible to equality *.

Fig. 3.
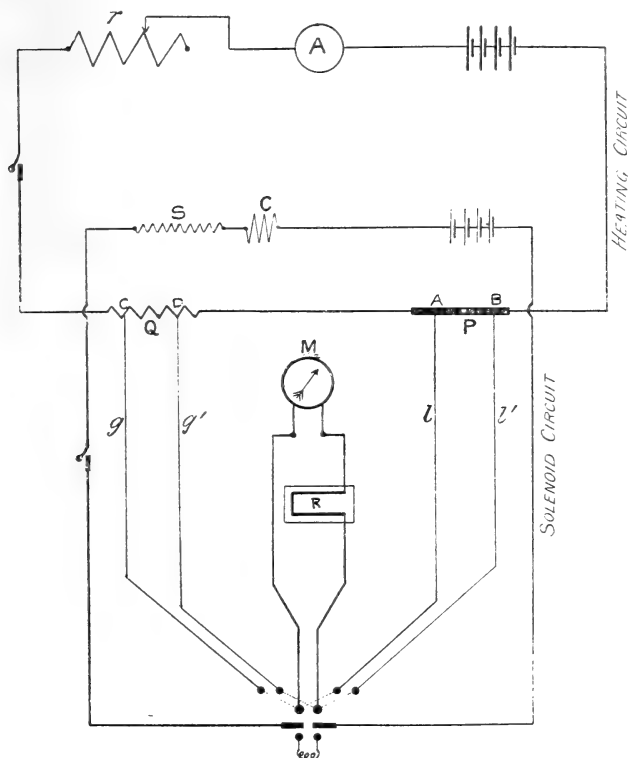
S. Solenoid. C. Compensating-coil. P. Pt. tube. Q. Manganin resistance. M. Magnetometer-coil. A. Ammeter. r. Rheostat.

The resistance of the portion CD of the manganin "standard" was accurately determined beforehand.

The platinum temperature, "pt," is derived from the platinum resistance by means of the equation

$$pt = \frac{R_t - R_0}{R_0 \alpha}, \quad \cdots \quad (1)$$

where R and R_0 are platinum resistances at $t°$ and $0°$

---

* The two circuits never differed by more than $\frac{1}{5}$ ohm, and since the resistance of the total circuit was 100 ohms, observations of platinum resistance lay within the limits of accuracy of the experiment.

respectively, and α is the mean temperature coefficient of resistance between $0°$ and $100°$.

The value of "α" for each platinum tube was obtained from observations of its resistance in ice and steam. For two of the tubes used, the coefficients were $0\cdot00387$ and $0\cdot00394$: values which were sufficiently high to justify the use of $1\cdot5$ as the difference coefficient Δ in Callendar's formula

$$t - pt = \Delta \left(\frac{t}{100} - 1 \right) \frac{t}{100},$$

where "t" represents the temperature on the air-thermometer scale.

For conversion to air-scale, curves were constructed from this formula by plotting "$t - pt$" against "pt," and all corrections to air-scale are made graphically by means of these curves.

Before beginning a series of magnetic observations, it was necessary to determine the resistance at $0°$ of that portion of the platinum tube which lay between the two side leads (R_0 in formula 1). When the heating apparatus was in position inside the solenoid, the resistance of the platinum at the temperature of the laboratory was determined, and from this was deduced the value of its resistance at $0°$.

Method of taking the Observations.

The method adopted was first to raise the temperature of the specimen to a definite value, and while this value was kept as steady as possible, to vary the magnetizing force.

In the first set of experiments with nickel, a vacuum was not necessary, and the operations were as follows :—the nickel having been demagnetized by reversals and removed from the solenoid, the compensating coil C_2 (fig. 1) was adjusted until a current in the solenoid ceased to affect the magnetometer. The heating current was then allowed to run for 10 minutes, when observations of platinum resistance were made, and repeated at intervals of 3 or 4 minutes. As soon as a steady temperature was attained, the magnetizing current was started, and increased step by step, the corresponding magnetic deflexions being noted. The nickel was then taken through several complete cycles, during the last of which were recorded the values of current and magnetic deflexion. Finally, another set of resistance measurements were taken.

The temperature was calculated from the mean value of the resistance before and after the magnetic observations.

In cases where a vacuum was employed the glass tube N could not be removed from the solenoid; hence the specimen was demagnetized by raising its temperature above the critical point, while compensation was made for the direct action of the solenoid when the metal was in the neutral state consequent on its high temperature.

The intensity of magnetization I corresponding to a magnetic force H being known, values of the susceptibility $\kappa \left(= \dfrac{I}{H} \right)$ were calculated, and the permeability could then be deduced from the equation,

$$\mu = 4\pi\kappa + 1.$$

In fig. 4, each separate curve shows the relation between permeability and magnetic force for a particular temperature,

Fig. 4.

Magnetic Force.
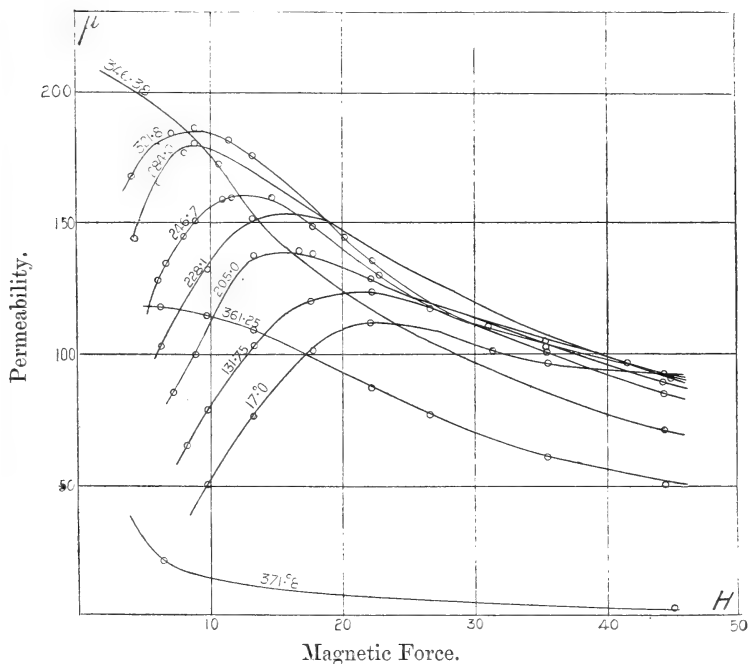
and there is drawn one H curve for each temperature at which an experiment was made. In order to obtain from this series of curves the relation between permeability and temperature for any particular field, it is merely necessary to draw an

ordinate corresponding to the field, and then to read off values of μ at the points in which this ordinate is cut by the curves.

Each value of μ obtained in this way corresponds to a definite temperature, and a curve showing the variation of permeability with temperature can be constructed (figs. 5 & 6).

Fig. 5.

Temperature Centigrade.

Fig. 6.

Temperature Centigrade.
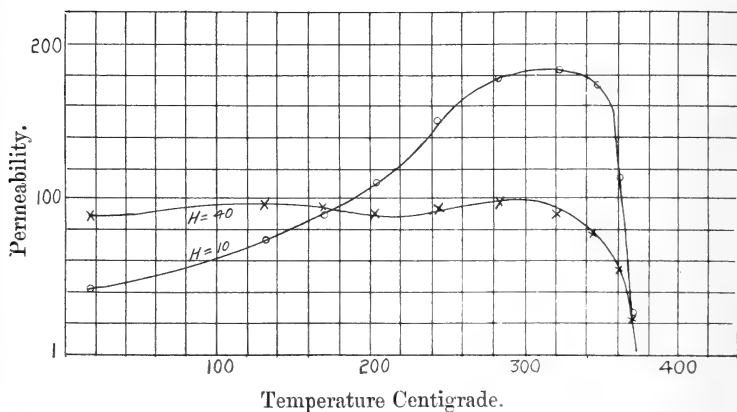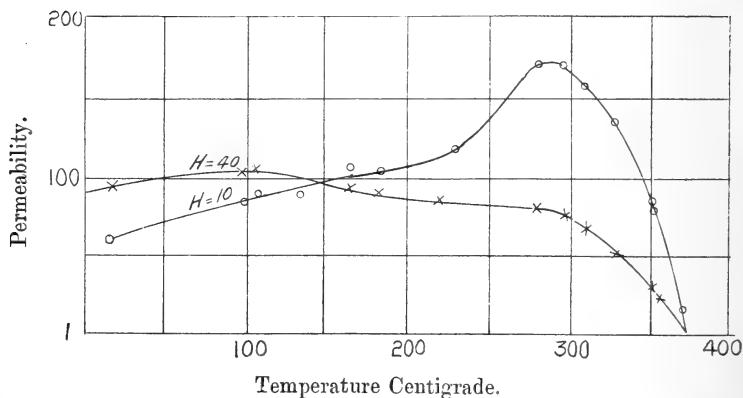
In view of the work of Morris and others on iron, it was thought unnecessary to examine that metal in such detail as nickel. Hence the experiments on iron were chiefly confined to a determination of the critical temperature for the specimen
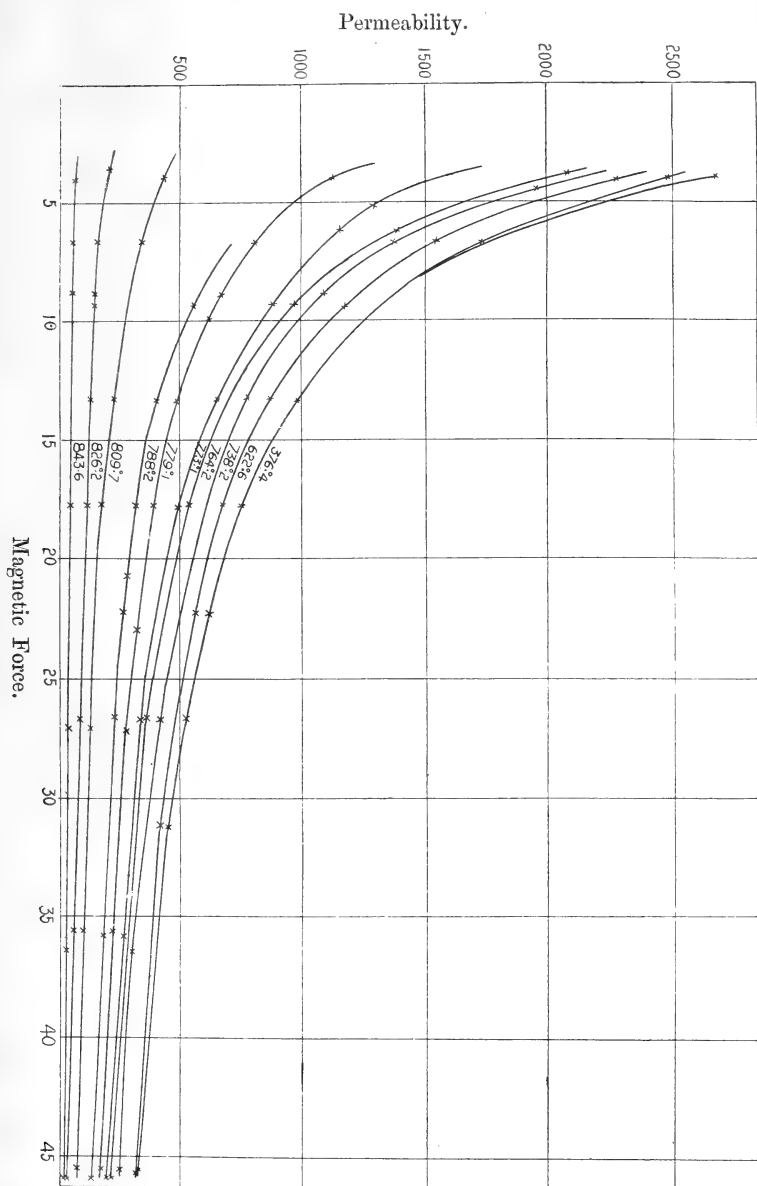
Fig. 7.

previously employed in the thermoelectric experiments, and to measurements of the variation of μ with temperature for fields not less than 10 C.G.S. In most cases, therefore, it will be seen by fig. 7 that the magnetic force has not been sufficiently reduced to exhibit the bend in the μH curve which corresponds to maximum permeability.

Fig. 8.

Temperature Centigrade.
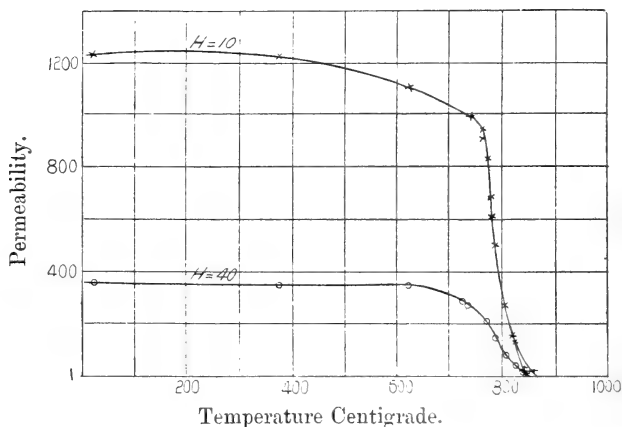
Estimated Accuracy of the Measurements.

Reading the scale to $\frac{1}{10}$ mm., the maximum error in temperature at $500°$ is $0°\cdot5$ C. And for fields of 40 C.G.S units an accuracy of 1 part in 500 is attained on the intensity of magnetization.

Discussion of Results.

NICKEL.

If we consider IH curves for nickel at any two temperature below the critical point, "the curve for the lower temperature lies at first below, and afterwards above the other" *.

In the series of results given in Tables I. & II. (pp. 197–200), and represented graphically in fig. 4, the effect of temperature on permeability is well brought out.

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* Ewing, 'Magnetic Induction in Iron, and other Metals.' Electrician Series.

(1) For a high temperature, the maximum permeability in any one curve occurs at a smaller value of the field than for a lower temperature.

(2) The increase in susceptibility under small fields as the temperature is raised is notable, although it is relatively much less than in the case of iron.

(3) At temperatures which approach the critical point there is more susceptibility to weak than to strong fields, while at lower temperatures the reverse is the case.

Figs. 5 and 6 show how the permeability varies with temperature for different values of the field. Curves I. and II. are derived from observations under fields of 10 C.G.S. and 40 C.G.S. respectively (Table IV., p. 203).

For temperatures below 180° the permeability under a 10 C.G.S. field is less than that under a field of 40 units. Up to 300° the permeability under a high field is fairly constant, but above that temperature it begins to fall off, until at 374° it is practically reduced to unity.

For the lower field, μ reaches a maximum of about 180 at a temperature of 320°, after which it falls off more suddenly than in the previous case, reaching a minimum value at the same temperature as in curve I.

The critical temperature (374°), at which the magnetism of this specimen of nickel practically vanishes, is independent of the strength of the field to which the metal was subjected.

The curves in fig. 6 show μT curves for a specimen which was annealed at a slightly higher temperature.

The hysteresis loops shown in fig. 9 were obtained under magnetizing forces of $+45$ C.G.S. units. They are plotted from values given in Table III. (pp. 200–202). In fig. 10 are shown curves representing the change in "Coercive force" and "Residual magnetism," with temperatures. Both quantities gradually decrease as the temperature rises, and they both vanish at the "Critical temperature" 374°. The values are plotted from Table V. (p. 203) which was compiled from the complete cycles of Table II., Set II.

Fig. 11 (p. 194) and Table V. represent the temperature variation of the hysteresis loss in ergs per cubic cm. per cycle, calculated from Set II.

This curve also cuts the axis of temperature at a point a little below the critical point, the hysteresis undergoing a rather more rapid reduction above 250° C. Up to this temperature the curve is a straight line.

Fig. 9.
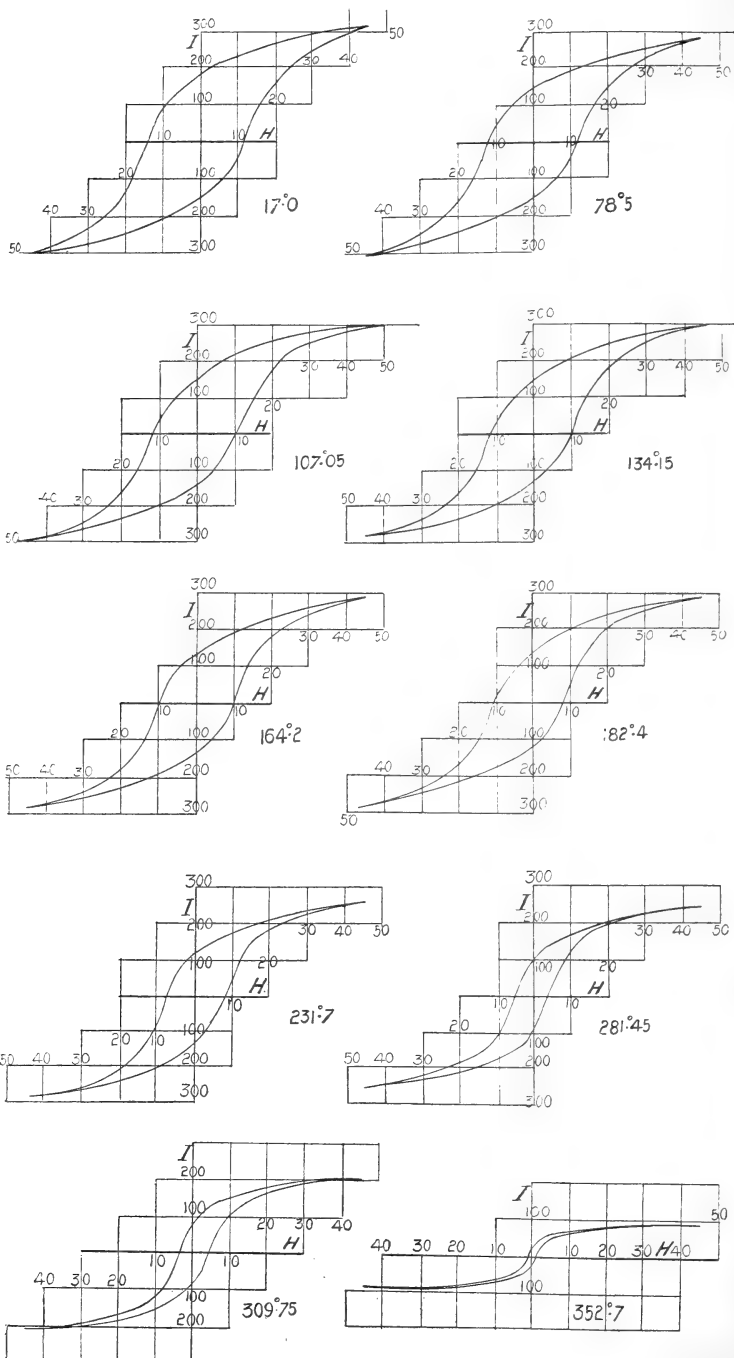
Fig. 10.
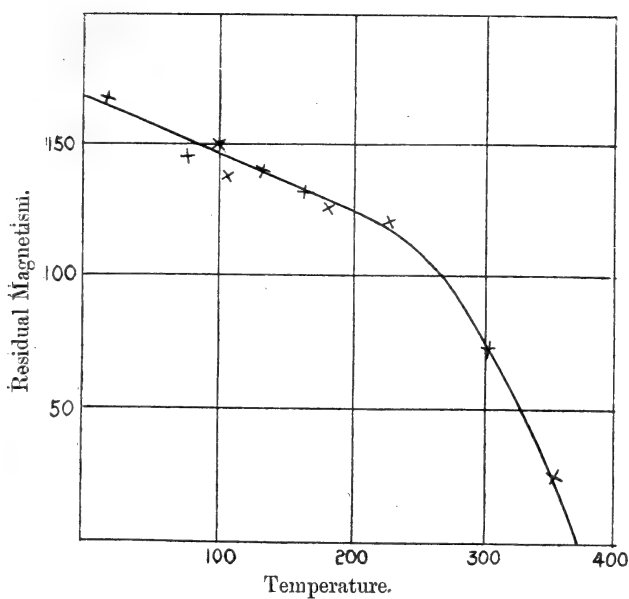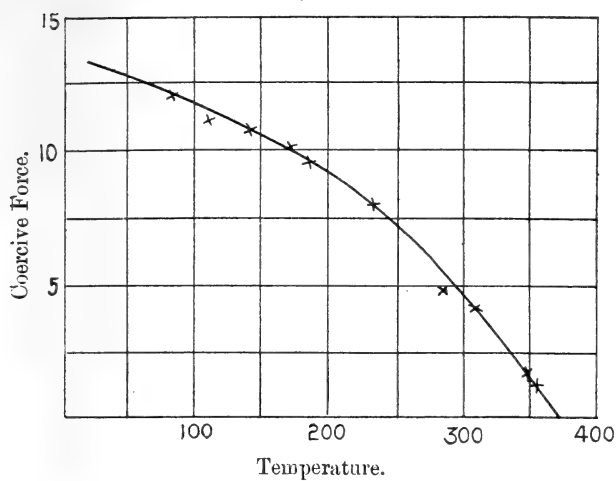
Fig. 11.
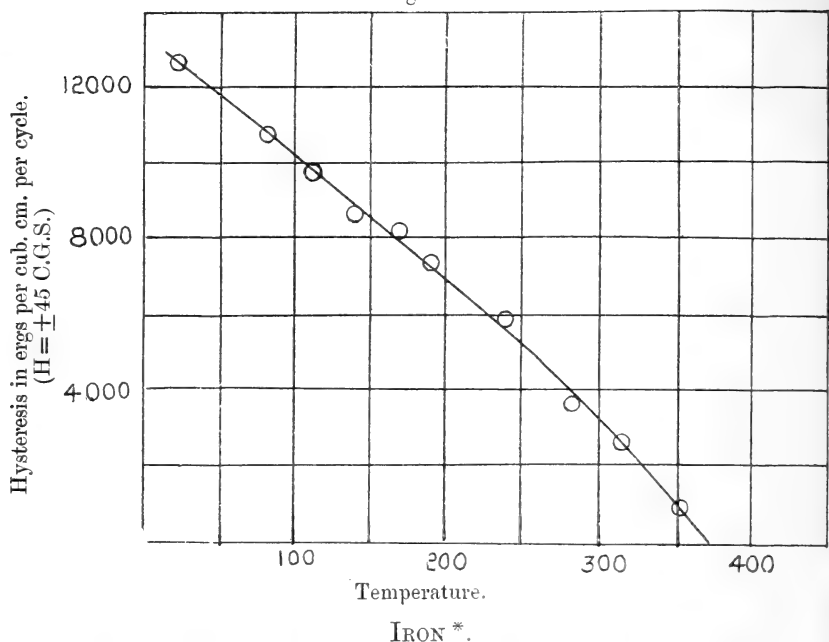
IRON [*].

In fig. 7 (p. 189) are shown μH curves for iron at different temperatures, and the μT curves for fields of 10 and 40 C.G.S. units are given in fig. 8 (p. 190). From this last, it is seen that a sudden drop in permeability occurs at about 760°, while the metal ceases to be appreciably magnetic at 855° [†].

Hysteresis with respect to Temperature.

If the magnetizing field be kept constant while the temperature is varied over a range which includes the critical point, the magnetism vanishes during heating at a somewhat higher temperature than that at which it reappears when cooling. The difference in temperature between the two critical points, as determined for this specimen of iron, was about 12°, and is exhibited on the curve for H = 10 (fig. 8).

The actual temperatures found at which magnetic quality vanished and reappeared were as follows :—

     Magnetism disappeared at 862° for rising temperature.
     Magnetism reappeared at 850° for falling temperature.
                 Difference 12°.

In experiments on electrolytically deposited iron, it is noteworthy that Osmond ‡ observed an absorption of heat

* See Tables VI. aud VII.   † See next Section.
‡ Osmond, " Transformations du fer et du carbone," *Mémorial de l'Artillerie de la Marine*, 1888.

during heating at a temperature of 867°, and an evolution of heat during cooling at about 855°.

No such effect as has been described in the last section was observed in the case of nickel.

It appears that as the quantity of impurity in a metal is reduced, these two critical points approach one another. No doubt the fact that the sample of nickel used in these experiments is purer than the iron is sufficient to account for the absence of any temperature-hysteresis effect in the former metal.

Above the chief critical point in iron, Morris observed another maximum in the curve representing the variation of *maximum permeability* with temperature. The author finds no such increase in permeability above the critical point, but, as is seen in fig. 8, the drop in permeability becomes less rapid above 830° (*i. e.*, $\frac{d\mu}{dT}$ becomes smaller).

This bending outwards in the μT curve is also remarked on by Pitcher (*loc. cit.*). It is evidently not directly due to the alteration in temperature coefficient of the iron as Pitcher suggests, since in the present experiments temperature was not deduced from the resistance of the iron, but from that of the platinum tube.

General Conclusions.

For the purpose of correlating these magnetic changes with variations that occur in other physical properties of iron and nickel at high temperatures, it will be found advisable not to confine attention to the actual critical temperature, but to regard as a *critical interval* the range of temperature extending from the sudden drop in magnetic intensity to its vanishing point *.

The following values were obtained for this critical interval, in three separate experiments for nickel, and one for iron.

Metal.	Temperature of sudden drop in permeability.	Critical Temperature.	Critical Interval.	Temperature at which specimen was annealed.
	320·0	374·0	54·0	500
Nickel .	300·0	373·0	73·0	600
	310·0	374·0	64·0	600
Iron ...	760·0	†855·0	95 0	1000

* See the articles by Du Bois and Warburg, tome 2, *Rapports présentés au Congrès International de Physique*, Paris, 1900.

† 855° is the mean of the two critical temperatures referred to in the previous section.

The lower limit of the interval is rather indefinite. If, however, we compare the magnetic critical interval with the range of temperature in which thermoelectric changes occur *, it is seen that the Peltier coefficient for nickel undergoes a change between the temperatures 290° and 375°, the alteration in slope of the curve at the latter point being very abrupt.

The change in slope in the resistance-temperature curve for the same specimen occurs between the temperatures 320° and 375°.

For iron, the thermoelectric change occurs between 750° and 860°, while an alteration in its coefficient of resistance extends over the interval between 750° and 850°.

Thus it is seen that for both nickel and iron there is a very good agreement between the magnetic critical interval and the range of temperature within which thermoelectric and resistance changes take place. The agreement is rather more complete in the case of nickel than it is in the case of iron, probably on account of the greater purity of the nickel specimen employed. The fact that the critical temperature for nickel is about 50° higher than that usually quoted for the metal is also probably to be accounted for in the same way.

Since the completion of this paper, Messrs. Honda and Shimizu † also find the magnetic critical point for a specimen of nickel to be "just under 400°." They also show that the " contraction under magnetization " vanishes asymptotically as the temperature approaches the critical point.

Part of the heating apparatus and the solenoid used in these experiments were made at University College, London, where the general method was suggested by Professor Callendar, F R.S., to whom I desire to acknowledge my indebtedness. I also wish to express my thanks to Professor H. F. Weber, for many suggestions given and kindly interest shown during the course of this research.

Physik Gebaüde,
    Zürich.

NUMERICAL RESULTS.

TABLE I.—*Nickel.*   Set I.   Annealed at 500°.

Intensity of Magnetization.	Magnetizing Force.	Permeability.	Intensity of Magnetization.	Magnetizing Force.	Permeability.
Temperature 131°·75.			Temperature 170°·4.		
8·36	3·56	30·5	8·36	2·22	48·4
12·54	5·32	30·6	20·9	5·32	50·4
29·2	7·1	52·8	33·4	7·11	60·0
41·7	8·89	60·0	54·4	8·89	77·9
65·2	10·65	78·0	81·9	10·65	97·6
108·7	13·3	103·4	104·4	12·43	106·5
167·1	17·8	119·2	115·2	13·34	109·6
217·2	22·2	123·7	150·4	16·0	119·3
246·5	26·7	117·1	186·3	18·62	126·7
271·9	31·1	110·9	217·3	22·23	123·8
292·2	35·6	104·3	246·4	26·68	117·15
305·2	40·0	96·8	267·6	31·13	109·1
322·0	44·47	92·0	284·2	33·58	107·4
			309·3	44·47	88·3
Temperature 205°·0.			Temperature 246°·7.		
15·055	3·55	54·3	25·1	3·55	89·9
33·4	5·32	79·6	41·8	5·32	99·6
48·5	7·1	86·8	62·7	6·22	128·0
70·2	8·89	100·2	91·9	8·0	145·6
100·3	10·66	119·4	106·1	8·89	151·1
145·4	13·34	138·0	142·1	11·57	155·3
192·2	17·79	136·9	167·1	13·34	158·5
225·8	22·23	128·6	193·1	15·1	161·7
250·7	26·68	119·2	210·7	17·79	150·0
270·0	31·13	110·1	232·3	20·5	143·7
284·2	38·4	94·1	258·2	22·24	135·5
292·5	44·47	83·8	265·9	26·68	126·1
			276·0	31·13	112·3
			288·2	35·58	102·8
			334·3	44·47	95·6
Temperature 284°·13.			Temperature 321°·8.		
50·2	5·11	124·4	75·2	5·32	178·3
98·6	7·23	172·1	108·8	7·32	187·5
127·0	8·89	180·7	128·8	8·90	183·1
150·5	10·65	178·4	167·1	11·57	182·5
177·2	13·34	168·0	186·3	13·34	176·6
204·0	16·0	161·2	219·9	17·79	157·3
225·8	18·64	153·1	234·0	22·24	133·1
246·5	22·24	140·3	257·6	27·60	118·27
271·8	26·68	129·0	277·8	35·60	99·0
303·6	36·5	105·58	292·4	44·47	83·8
319·7	44·47	91·4			

Table I.—*Nickel*.   Set I. (*cont.*).

Intensity of Magnet-ization.	Magnetizing Force.	Per-meability.	Intensity of Magnet-ization.	Magnetizing Force.	Per-meability.
Temperature 346°·38.			Temperature 361°·25.		
71·1	4·45	201·8	8·4	0	...
102·8	6·4	202·7	58·5	6·2	119·2
104·5	7·1	185·9	69·4	8·0	109·98
131·2	8·9	186·3	83·57	8·89	119·3
146·2	10·7	173·0	88·58	9·8	114·9
158·9	13·3	150·4	100·3	11·6	110·2
188·0	17·8	133·8	117·0	13·3	111·1
210·7	22·2	120·0	133·8	17·8	95·5
224·0	26·7	116·6	152·1	22·2	86·9
235·8	35·6	84·3	160·4	26·7	76·5
244·0	44·5	70·0	167·14	35·6	59·9
			173·9	44·5	50·1
Temperature 371°·7.					
7·42	5·34	18·47			
16·97	8·9	24·98			
29·6	13·34	28·92			
44·5	17·8	32·42			
48·75	22·2	28·56			
58·4	26·7	28·49			
74·2	31·1	30·95			
79·6	40·02	25·98			
84·8	44·47	24·98			

Table II.—*Nickel*.   Set II.   Annealed at 600°.

Intensity of Magnet-ization.	Magnetizing Force.	Per-meability.	Intensity of Magnet-ization.	Magnetizing Force.	Per-meability.
Temperature 17°·0.			Temperature 99°·3.		
0	0	...	0	0	...
10·4	2·2	...	17·75	1·78	...
18·8	4·0	60·1	31·32	4·0	...
23·0	6·23	47·2	41·75	7·29	73·0
39·7	9·34	54·4	57·4	9·34	78·2
80·4	13·35	76·7	102·3	13·35	97·4
144·1	17·87	102·4	177·5	18·67	120·4
195·3	22·3	110·9	219·3	22·3	124·6
250·5	31·4	101·0	248·5	26·75	117·9
315·5	44·91	89·3	302·8	35·8	107·3
			334·2	44·91	94·5

Table II.—*Nickel.* Set II. (*cont.*).

Intensity of Magnetization.	Magnetizing Field.	Permeability.	Intensity of Magnetization.	Magnetizing Field.	Permeability.
Temperature 106°·3.			Temperature 133°·9.		
0	0	...	0	0	...
10·44	1·57	...	10·44	2·2	...
20·88	4·0	66·6	18·8	4·0	60·1
36·6	6·23	74·8	33·4	6·23	68·5
62·6	9·34	85·1	57·4	9·34	78·25
104·4	13·35	99·4	109·7	13·35	104·2
188·0	17·87	119·0	162·0	17·87	114·7
268·4	26·75	127·2	205·7	22·3	116·7
308·1	35·8	109·2	256·0	31·4	103·3
334·2	44·91	94·6	289·3	40·29	91·2
			296·5	44·91	84·0
Temperature 164°·0.			Temperature 182°·8.		
0	0	...	0	0	...
13·58	2·22	77·9	13·58	1·78	...
31·32	4·0	(99·5)	20·88	4·0	66·6
43·8	6·23	89·25	38·65	6·23	78·9
73·1	9·34	99·5	71·0	9·34	96·4
127·3	13·34	121·0	123·3	13·35	117·0
174·3	17·87	123·4	168·1	17·87	119·2
208·8	22·3	118·8	198·3	22·3	112·6
238·2	26·75	113·0	227·8	26·75	108·0
274·7	35·8	97·5	261·0	35·8	92·6
299·8	44·91	84·9	288·3	44·91	81·6
Temperature 231°·7.			Temperature 281°·45.		
0	0	...	0	0	...
17·75	2·22	...	20·9	2·22	119·2
24·0	4·0	76·5	41·8	4·0	132·2
42·8	6·23	87·3	93·96	7·29	163·0
80·4	9·34	109·2	125·3	9·34	169·7
118·0	11·74	127·2	146·2	11·74	157·3
137·9	13·35	130·8	156·7	13·35	148·3
184·8	17·87	131·0	193·2	17·87	136·9
205·8	22·3	117·0	214·0	22·3	121·5
224·6	26·75	106·4	219·3	26·75	104·0
250·6	35·8	89·0	242·2	35·8	86·1
269·5	44·91	76·4	247·6	44·91	70·2

Table II.—*Nickel.*   Set II. *(cont.).*

Temperature 310°·05. / Temperature 328°·1.

Intensity of Magnetization.	Magnetizing Field.	Permeability.	Intensity of Magnetization.	Magnetizing Field.	Permeability.
0	0	...	0	0	...
22·98	1·78	163·0	43·8	2·22	...
60·6	4·0	191·0	49·1	4·0	...
91·9	6·23	186·4	83·6	7·29	145·1
114·9	9·34	156·8	99·2	9·34	134·4
137·8	13·35	130·7	109·7	13·35	104·2
158·7	17·87	112·6	125·3	17·87	89·25
172·3	22·3	98·0	137·9	26·75	65·8
179·7	26·75	85·5	149·3	35·8	53·5
196·3	35·8	70·0	156·8	44·91	44·8
203·8	44·91	58·1			

Temperature 353°·05. / Temperature 371°·8.

Intensity of Magnetization.	Magnetizing Field.	Permeability.	Intensity of Magnetization.	Magnetizing Field.	Permeability.
0	0	...	0	0	...
33·4	2·7	...	10·44	2·2	...
47·0	4·0	148·7	11·49	4·0	36·3
55·4	7·29	96·4	10·44	6·23	22·1
62·6	9·34	85·1	10·44	9·34	15·1
67·8	13·35	64·8	12·01	44·91	4·36
75·2	17·87	53·8			
77·3	22·3	44·6			
83·6	31·4	34·4			
93·0	44·91	27·0			

TABLE III.—*Nickel.*   Set II.   Complete Cycles.   Annealed at 600°.

Temperature 17°·0.

Intensity of Magnetization.	Magnetizing Field. (Positive values.)	Intensity of Magnetization.	Magnetizing Field. (Negative values.)
−156·7	0	156·7	2·2
−133·8	2·22	152·4	4·0
−123·2	4·0	132·7	6·23
− 86·7	7·29	99·2	9·34
− 62·7	10·18	16·7	13·35
73·1	13·35	− 99·2	17·87
117·0	17·87	−172·2	22·3
184·8	22·3	−245·6	31·4
224·5	26·75	−267·4	35·8
280·0	35·8	−303·0	44·91
315·5	44·91	−285·2	35·8
302·7	35·8	−266·2	26·75
282·0	26·75	−237·0	17·87
261·0	17·87	−197·2	9·34
227·7	9·34	−170·3	4·0
205·8	4·0	−146·2	0
177·5	0		

Temperature 78°·5.

Intensity of Magnetization.	Magnetizing Field. (Positive values.)	Intensity of Magnetization.	Magnetizing Field. (Negative values.)
−148·3	0	122·2	2·67
−127·4	2·2	117·0	4·0
−114·9	4·0	101·2	6·23
− 85·6	6·23	69·0	9·34
− 50·2	9·34	− 23·0	13·35
44·9	13·35	−124·3	17·87
135·7	17·87	−182·8	22·3
217·2	26·75	−216·1	26·75
260·9	35·8	−241·2	31·4
292·3	44·91	−267·5	35·8
271·4	35·8	−286·1	44·91
250·5	26·75	−277·8	35·8
227·6	17·87	−240·1	26·75
198·5	9·34	−215·1	17·87
179·6	4·0	−185·9	9·34
148·3	0	−159·8	4·0
		−135·76	0

Table III.—*Nickel.*   Set II.   Complete Cycles (*cont.*).

Temperature 107°·05.   Temperature 134°·15.

Intensity of Magnetization.	Magnetizing Field.	Intensity of Magnetization.	Magnetizing Field.	Intensity of Magnetization.	Magnetizing Field.	Intensity of Magnetization.	Magnetizing Field.
	Positive values.		Negative values.		Positive values.		Negative values.
−138·38	0	118·6	2·22	−125·28	0	111·8	4·0
−111·2	2·67	110·3	4·0	−102·3	3·11	79·4	6·23
− 96·6	4·0	90·4	6·23	− 93·96	4·0	45·9	9·34
− 75·7	6·23	55·8	9·34	− 66·8	6·23	− 62·6	13·35
− 41·3	9·34	− 48·6	13·35	− 28·2	9·34	141·0	17·87
60·0	13·35	−133·1	17·87	83·5	13·34	183·9	22·3
146·7	17·87	−187·5	22·3	162·0	17·87	236·0	31·4
195·9	22·3	−246·0	31·4	203·7	22·3	273·7	44·91
250·0	31·4	−281·6	40·29	252·8	31·4	260·1	35·8
284·7	44·91	−292·4	44·91	292·3	44·91	239·3	26·75
282·4	35·8	−270·0	35·8	276·8	35·8	219·3	17·87
268·8	26·75	−248·1	26·75	261·2	26·75	181·8	9·34
237·6	17·87	−222·9	17·87	235·0	17·87	159·8	4·0
206·1	9·34	−191·6	9·34	200·5	9·34	125·3	0
177·1	4·0	−170·7	4·0	177·5	4·0		
138·4	0	−140·5	0	149·3	0		

Temperature 164°·2.   Temperature 183°·4.

Intensity of Magnetization.	Magnetizing Field.	Intensity of Magnetization.	Magnetizing Field.	Intensity of Magnetization.	Magnetizing Field.	Intensity of Magnetization.	Magnetizing Field.
	Positive values.		Negative values.		Positive values.		Negative values.
−131·6	0	103·4	4·0	−120·2	0	88·8	4·62
−103·3	2·67	82·5	6·23	− 85·6	4·0	49·1	7·29
− 92·8	4·0	34·6	9·34	− 52·2	6·23	22·98	9·34
− 68·9	6·23	− 82·4	13·35	− 12·5	9·34	− 89·8	13·35
− 12·5	9·34	−153·6	17·87	−114·8	13·34	153·5	17·87
− 97·2	13·35	−194·1	22·3	177·5	17·87	193·3	22·3
166·2	17·87	−223·6	26·75	214·2	22·3	220·4	26·75
199·6	22·3	−248·2	35·8	237·2	26·75	251·8	35·8
228·7	26·75	−277·8	44·91	261·0	35·8	279·0	44·91
264·2	35·8	−261·2	35·8	282·0	44·91	246·5	35·8
290·5	44·91	−240·2	26·75	268·4	35·8	235·0	26·75
276·0	35·8	−209·8	17·87	252·7	26·75	204·7	17·87
257·0	26·75	−179·6	9·34	229·8	17·87	176·5	9·34
225·7	17·87	−154·6	4·0	190·1	9·34	148·3	4·0
189·0	9·34	−131·6	0	166·2	4·0	123·2	0
171·4	4·0			131·7	0		
131·6	0						

Table III.—*Nickel.*   Set II. (*cont.*).

Temperature 231°·7.

Positive values. Intensity of Magnetization.	Magnetizing Field.	Negative values. Intensity of Magnetization.	Magnetizing Field.
−120·7	0	64·2	+·62
− 82·0	2·67	58·9	7·29
− 69·6	4·0	− 60·1	9·34
− 37·18	6·23	−140·5	13·35
− 3·8	9·34	−186·5	17·87
126·9	13·35	−217·9	22·3
170·6	17·87	−237·8	26·75
194·6	22·3	−264·8	35·8
212·5	26·75	−272·2	44·91
242·8	35·8	−263·9	35·8
257·5	44·9	−240·9	26·75
241·8	35·8	−216·8	17·87
221·8	26·75	−184·5	9·34
202·1	17·87	−157·3	+·0
163·3	9·34	−129·0	0
147·8	4·0		
120·6	0		

Temperature 281°·45.

Positive values. Intensity of Magnetization.	Magnetizing Field.	Negative values. Intensity of Magnetization.	Magnetizing Field.
− 90·3	0	53·8	2·22
− 48·6	2·22	37·0	4·0
3·6	4·0	− 41·3	6·23
84·0	7·3	−105·0	9·34
110·2	9·34	−156·1	13·35
158·2	13·35	−170·8	17·87
189·5	17·87	−195·8	22·3
203·2	22·3	−222·0	31·4
207·4	26·75	−249·0	44·91
223·0	35·8	−237·6	35·8
233·4	44·91	−226·2	26·75
221·0	31·4	−206·2	17·87
191·5	22·3	−168·7	9·34
170·7	13·35	−144·7	4·0
129·0	4·0	−111·2	0
90·4	0		

Temperature 309°·75.

Positive values. Intensity of Magnetization.	Magnetizing Field.	Negative values. Intensity of Magnetization.	Magnetizing Field.
− 91·4	0	− 2·6	+·31
− 41·2	2·67	− 77·8	7·29
− 6·7	4·0	− 96·6	9·34
52·8	6·23	−136·3	13·35
94·5	9·34	−158·2	17·87
129·0	13·35	−178·1	26·75
157·2	17·87	−195·8	35·8
172·9	22·3	−202·1	44·91
188·5	31·4	−193·6	35·8
201·2	44·91	−185·3	26·75
196·9	35·8	−160·3	17·87
186·5	26·75	−135·2	9·34
170·9	17·87	−103·8	4·0
148·9	9·34	− 72·5	0
131·1	4·0		
91·5	0		

Temperature 352°·7.

Positive values. Intensity of Magnetization.	Magnetizing Field.	Negative values. Intensity of Magnetization.	Magnetizing Field.
−28·74	0	−22·5	1·78
23·4	2·22	−40·2	4·0
39·2	4·0	64·3	8·36
54·8	6·23	71·6	10·94
67·3	9·34	75·8	13·35
65·2	13·35	83·0	17·87
75·6	17·87	87·3	26·75
86·1	26·75	84·1	35·8
86·1	35·8	89·3	44·91
91·4	44·91	80·9	35·8
86·1	35·8	85·1	26·75
80·9	26·75	82·0	17·87
80·9	17·87	65·3	9·34
67·3	9·34	51·71	4·0
54·8	4·0	28·74	0
28·7	0		

TABLE IV.

Relation between Permeability and Temperature,
for different Fields.

Nickel. Set I.

Field of 10 C.G.S.	Field of 40 C.G.S.	Temperature.
44·0	89·8	18·0
74·2	98·0	131·75
91·1	94·6	170·4
111·8	91·2	205·0
155·0	95·0	246·7
179·0	99·0	284·13
184·4	91·0	321·8
176·0	77·1	346·38
115·1	54·2	361·25
27·0	25·0	371·7

Nickel. Set II.

Field of 10 C.G.S.	Field of 40 C.G.S.	Temperature.
59·5	92·0	17·0
83·0	100·05	99·3
87·5	102·5	106·3
85·5	91·0	133·9
102·5	91·0	164·0
99·98	87·0	182·8
107·0	83·2	231·7
167·0	78·0	281·45
153·0	64·0	310·05
130·0	49·0	328·1
81·5	29·0	353·05
73·0	21·0	356·1
14·0	4·0	371·8

TABLE V.—*Nickel.*

Temperature.	Residual Magnetism.	Coercive Force.	Hysteresis loss in ergs per cub. cm. per cycle. H = ±45 C.G.S.
17·0	168·5	13·2	12700
78·5	145·0	12·0	10700
107·05	137·5	11·0	9600
134·15	140·0	10·75	8500
164·2	130 0	10·0	8200
182·4	125·0	9·5	7300
231·7	120·0	8·0	5800
281·45	92·5	4·75	3400
309·75	75·0	4·12	2600
352·7	25·0	1·25	450

TABLE VI.—*Iron.*

Inten. of Magn.	Magn. Field.	Perm.	Inten. of Magn.	Magn. Field.	Perm.	Inten. of Magn.	Magn. Field.	Perm.
Temperature 18°·0			Temp. 376°·4.			Temp. 622°·6.		
803	4·0	2523	876	4·45	2476	825	6·67	1553
928	6·67	1746	933	6·67	1761	878	8·89	
977	8·89	1381	972	8·89	1377	919	13·3	867
1038	13·34	977	1020	13·34	962	960	17·8	679
1062	17·79	751	1053	17·8	745	1037	31·1	420
1107	26·75	521	1072	22·2	607	1093	48·9	282
1131	36·02	395	1113	31·1	448			
1152	44·5	326	1154	44·5	327			
			1173	48·9	302			

Intensity of Magnetization.	Magnetizing Field.	Permeability.	Intensity of Magnetization.	Magnetizing Field.	Permeability.
Temperature 738°·2.			Temperature 764°·2.		
693	4·4	1978	690	6·23	1393
742	6·67	1396	719	9·34	967
820	13·34	773	733	13·35	692
832	17·79	588	753	17·87	532
883	36·46	305	773	26·75	364
893	49·36	229	782	35·8	275
			789	44·9	222
Temperature 773°·1.			Temperature 779°·1.		
615	6·23	1152	372	4·45	1055
651	9·34	877	424	6·67	800
679	13·35	641	470	8·89	665
694	17·87	489	505	13·34	476
709	26·75	334	543	17·79	385
730	35·8	257	579	27·13	269
734	44·91	206	593	35·58	211
			632	44·47	179

Table VI.—*Iron* (cont.).

Intensity of Magnet-ization.	Magnetizing Field.	Per-meability.	Intensity of Magnet-ization.	Magnetizing Field.	Per-meability.
Temperature 809°·7.			Temperature 818°·7.		
139	4·0	436	95·2	4·89	251
178	6·67	337	105	6·67	199
214	9·34	289	119	8·89	164
232	13·34	219	133	13·34	126
242	17·79	172	119	17·79	84
266	27·13	124	129	26·68	62
274	35·58	97	135	35·58	48
294	44·47	84	143	44·47	40
Temperature 820°·4.			Temperature 843°·6.		
79	3·56	150	19·8	4·1	61
105	6·67	150	22	6·67	42
149	8·87	106	30	8·89	43
155	17·79	74	59	17·79	43
143	26·68	51	79	27·13	38
159	35·58	46	89	36·46	32
			99	44·91	29

TABLE VII.—*Iron*.

Temperature.	Permeability for Field of 10 C.G.S.
18·0	1230
376·4	1231
622·6	1100
738·2	1000
764·2	910
773·1	830
779·1	610
809·7	270
818·7	150
820·4	135
840·0	20
843·6	41

XXI. *On the Rate of Recombination of Ions in Gases. By* GEORGE W. WALKER, *M.A., A.R.C.Sc.; Fellow of Trinity College, Cambridge, and Lecturer in Physics in the University of Glasgow* *.

THE importance of this subject, at which Mr. McClung has been an enthusiastic worker for some time, will hardly be questioned.

In examining carefully his papers † I have observed what appear to me to be somewhat serious discrepancies in the results, which require explanation before one can accept the conclusions at which he arrives.

The following criticisms are not made from lack of appreciation of the enormous experimental difficulties; for I was working in the same laboratory when the later experiments were made, and saw Mr. McClung very frequently. At the same time, the important generalizations made from the experiments demand for proof substantial agreement between theory and experiment, whatever the difficulties may be.

We shall first recapitulate the fundamental assumptions, but shall assume that the reader has the papers at hand.

If q is the rate of production of ions (in this case by Röntgen rays) per unit volume, and n is the number of ions per unit volume; then, when a steady state is reached,

$$q = \alpha n^2, \quad \ldots \quad \ldots \quad \ldots \quad (1)$$

where α is the coefficient of recombination.

Again, when the rays are cut off,

$$\frac{1}{n_t} - \frac{1}{n_0} = \alpha t, \quad \ldots \quad \ldots \quad (2)$$

where n_0 is the number of ions per unit volume when the rays cease, and n_t is the number per unit volume which remain after a time t.

It is shown that

$$\alpha = \mathrm{K} \cdot \mathrm{Q}/\mathrm{N}^2, \quad \ldots \quad \ldots \quad (3)$$

where Q is the total rate of production in the given volume, and N is the total number in the given volume when the steady state is reached, and therefore at the instant the rays cease.

K is a constant depending on the dimensions of the apparatus.

We may call N_t the number of ions existing in the given volume after a time t.

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* Communicated by the Author.

† Phil. Mag. vol. iii. pp. 283–305 (1902), vol. vi. pp. 655–666 (Dec. 1903), vol. vii. pp. 81–95 (Feb. 1904).

In the experiments N_t and N were measured. It is further assumed that the saturation current gives a measure of Q.

The absorption of the rays is neglected as small.

From the measurements of Q and N, McClung concludes that α as given by the formula (3) is independent of the pressure from $\frac{1}{4}$ atmosphere to 3 atmospheres. He also concludes that Q is proportional to the pressure, the intensity of the rays remaining constant. If this is so, then N^2 should be proportional to the pressure, the intensity of the rays being constant.

In testing the formula (1), it appears to me that he assumes the relation between N_t, N, and t to be of the same form as (1). This, as I shall show, is not the case in his experiments. It would be true if the beam of rays were cylindrical.

He does not think the measurements of N_t, N, and t at different pressures can be relied on to find how α depends on the pressure, urging that the intensity of the rays may vary between the experiments at different pressures. I am unable to follow this argument. α is a property of the gas, and must be independent of the means by which the N ions are produced. It is quite true that both Q and N depend on the intensity of the rays; but if the theory as proposed is correct, the values of α, whether obtained from (3) or from the relation between N_t and N, must be the same, and quite independent of the intensity of the rays, provided that the intensity is the same for any one determination of N and Q.

We shall now establish the relation between N_t, N, and t. Using the same notation as McClung, we have

$$q_0 = \frac{Q}{A_1 l} = \frac{\alpha N^2}{l^2 A_1^2 \left(1 + \dfrac{l}{2d}\right)^2},$$

$$n_t = \frac{n_0}{1 + n_0 \alpha t},$$

and

$$n_0 = \frac{d}{x}\sqrt{\frac{q_0}{\alpha}}.$$

Hence the total number in the given conical volume after a time t is

$$N_t = \int_d^{d+l} n_t \frac{A_1 x^2}{d^2}\, dx$$

$$= N\left\{1 - \frac{\mu}{\left(d + \dfrac{l}{2}\right)} + \frac{\mu^2}{l\left(d + \dfrac{l}{2}\right)}\log_e\left(1 + \frac{l}{d + \mu}\right)\right\}$$

where

$$\mu = \lambda t,$$

and

$$\lambda = d\sqrt{q_0 \alpha}.$$

Hence

$$\alpha = \frac{\lambda}{N} \frac{l\left(1 + \frac{l}{2d}\right) A_1}{d}, \quad \cdots \quad (4)$$

or

$$\alpha = \frac{\lambda^2}{Q} \cdot \frac{A_1 l}{d^2}, \quad \cdots \cdots \quad (5)$$

or

$$\alpha = \frac{Q}{N^2} l A_1 \left(1 + \frac{l}{2d}\right)^2. \quad \cdots \cdots \quad (6)$$

If we use the values of l and d given in the paper, we get

$$\frac{N_t}{N} = 1 - \frac{\mu}{35 \cdot 07} + \frac{\mu^2}{701 \cdot 4} \log_e \left(1 + \frac{20}{25 \cdot 07 + \mu}\right). \quad \cdots \quad (7)$$

It is quite easy to test the results. We have only to substitute a value of μ, and calculate the value of the expression on the right-hand side. Then, since we know N, we can calculate the value of N_t; and hence from the experimental curve we get the value of t, and hence of λ. The values of λ for a given pressure should agree. Having obtained λ, we may get the absolute value of α by any of the three formulæ (4), (5), and (6); and the values so obtained should be the same. As I have already pointed out, these values cannot be affected by the intensity of radiation.

With a view to testing these points, the following table was prepared from the results given in McClung's paper (Phil. Mag. March 1902, pp. 293–296). In preparing it the values of μ were substituted in the expression on the right-hand side of (7), and then the values of t obtained from the experimental curves.

Pressure in Atmospheres.	μ.	$\frac{N_t}{N}$.	t.	$\lambda = \frac{\mu}{t}$.	Deflexion for N.	$\frac{\lambda}{N}$.
3	100	·266	·41	244	236	
3	50	·417	·20	250	236	1·06
2	100	·266	·47	212	196	
2	50	·417	·23	217	196	1·10
1	100	·266	·54	185	181	
1	50	·417	·28	178	181	·98
·5	100	·266				
·5	50	·417	·34	147	154	·95
·25	100	·266				
·25	50	·417	·53	94	136	·69
·125	100	·266				
·125	50	·417	·47	106	124	·83

The agreement between the values of λ for any one pressure is very satisfactory. But if α is independent of pressure, $\dfrac{\lambda}{N}$ should be constant. There is considerable deviation at the lower pressures. It cannot be held to be due to experimental error without detracting seriously from the value of the conclusion. It may be due to a difference in the sensibility of the electrometer; and if so, Mr. McClung may be able to explain the deviation if he has a record of the sensibility.

The next table is prepared from the same experimental results, and gives the values of α calculated by the three formulæ (4), (5), and (6). The values are expressed as multiples of e, the charge on an ion.

Pressure in Atmospheres.	λ.	Defl. for N.	α_N.	Defl. for Q.	α_Q.	$\dfrac{N^2}{Q}$.	$\alpha_{\frac{Q}{N^2}}$.
3	250	236	$1916\,e$	143	$974\,e$	389	$3940\,e$
2	217	196	$2060\,e$	112	$937\,e$	343	$4466\,e$
1	178	181	$1829\,e$	51	$1384\,e$	642	$2387\,e$
·5	147	154	$1776\,e$	41	$1170\,e$	578	$2652\,e$
·25	94	136	$1286\,e$	26	$757\,e$	711	$2156\,e$

The numbers are reduced by the data given on p. 300 of the same paper; and we may note that the values of N and Q given there give $N^2/Q = 453$.

In the table the values of α for any given pressure are quite discordant, and the values of α calculated by any one method cannot be said to be independent of pressure.

It may be argued, since the values of N^2/Q differ, that this is due to a change of sensibility of the electrometer. If so, the values of N, Q, and $\dfrac{N^2}{Q}$ would have to be changed in the same proportion for any one pressure. This, however, would not improve the relative values of α_N and α_Q at that pressure. For example, at 3 atmospheres α_{Q/N^2} would have to be reduced while α_N and α_Q would have to be increased in the same proportion, and thus α_{N/Q^2} would be brought nearer α_N or α_Q, but α_N and α_Q would not be brought nearer. Again, at ·5 atmos. α_{N/Q^2} would have to be increased and α_N and α_Q diminished in the same proportion. Thus I do not think that a change of sensibility would improve matters.

The discordance seems to me to be very serious, and requires explanation before we can accept either the theory or the conclusions.

The following table was prepared from the numbers on p. 298 of the paper, from which Mr. McClung showed that $\frac{Q}{N^2}$ was independent of pressure.

P. Pressure in Atmospheres.		Defl. for N.	Defl. for Q.	$\frac{N^2}{P} \times 10^{-2}$.	$\frac{Q}{P}$.	$\frac{N^2}{Q}$.
A.	1	121	34·4	146	34·4	425
	3	193	79·7	124	26·5	467
B.	1	67·9	16·6	46·1	16·6	278
	2·5	103·2	35·5	42·6	14·2	300
C.	1	70·4	15·2	49·5	15·2	326
	2	96·0	26·5	46·1	13·2	348
D.	1	128·6	38·2	165	38·2	433
	1·5	148·7	49·4	147	32·9	448
E.	1	110	29·3	121	29·3	413
	·5	93·5	20·7	174	41·4	422
F.	1	70	23·5	49·0	23·5	208
	·25	43·5	9	75·7	36	210
G.	1	61·9	22·7	38·3	22·7	168
	·125 ...	29·9	5·1	71·5	40·8	175

If α is independent of pressure and Q is proportional to the pressure, then N^2 is proportional to the pressure.

It would be unfair to compare values of N^2/P from different sets, say A and F, as the intensity of the rays might be very different. This, however, does not apply to the values of N^2/Q, which must be independent of the intensity. Nor does it apply to observations of any one set, *e. g.* F, where the intensity must be approximately constant if the numbers are to mean anything at all.

Was there, then, any difference in sensibility of the electrometer in A, B, F, and G?

The agreement between the values of N^2/P or Q/P in A, B, C or D, although not very good, may be accepted as approximately proved. But in E, F, or G there cannot be said to be substantial agreement.

The questions which naturally arise are : (1) Is the proposed theory correct? For the experiments cannot be held to support it. And (2) Do the measurements actually give the values of N, N_t, and Q?

In connexion with the second question, I have considerable

doubt whether the saturation current gives a measure of Q. I think it gives a measure of the excess of the rate of production of ions over the rate of recombination ; and I see no reason for assuming that the latter is negligible. It is equal to αN_s^2, where N_s is the number of ions per unit volume when the steady state, with the saturation current passing, is reached. It certainly cannot be zero, and may be comparable with Q.

In conclusion I should like to express my great obligation to my colleague Mr. James Gray, for his kindness in revising the tedious arithmetical calculations.

Physical Laboratory,
The University, Glasgow.
25th March, 1904.

XXII. *Reply to Mr. G. W. Walker's Paper on the " Rate of Recombination of Ions in Gases."* By R. K. McCLUNG, *M.A., Trinity College, Cambridge* *.

THROUGH the courtesy of Mr. Walker I am enabled to make a reply to the criticisms of my experiments on the rate of recombination of ions contained in his paper, which appears in the present number of this Magazine. These criticisms are evidently due to a want of knowledge of the exact conditions under which the experiments, described in my paper, were made, and the relation of the different experiments to one another. I am very sorry that Mr. Walker has wasted so much valuable time in making what must have been such very tedious arithmetical calculations, for the numbers obtained by him are quite valueless, and consequently the criticisms based upon these numbers are quite beside the mark.

Although at the beginning of his paper he mentions three of my papers which have appeared in the Philosophical Magazine, yet he confines his criticisms to the one on the effect of pressure on the rate of recombination of ions. I shall therefore also confine my remarks to the experiments described in that paper.

In the first place, Mr. Walker makes the following statement in the first part of his paper :—" In testing the formula (1) it appears to me that he assumes the relation between N_t, N, and t to be of the same form as (1). This, as I shall show, is not the case in his experiments. It would be true if the beam of rays were cylindrical." In deriving formulæ (1) and (2) no assumption whatever is made as to the shape of the beam of

rays or the volume of the gas ionized. These formulæ are quite general, and are independent of any such consideration; and I cannot therefore see his point in regard to the shape of the beam of rays, or how it affects the question under consideration.

Mr. Walker takes the curves which I gave in my paper on pp. 293–295, and calculates from them values for $\frac{\lambda}{N}$, and also for α by different formulæ, and finds discordant results. I am not at all surprised that he does so. To make such a determination and criticism of any value it is necessary to ascertain what relation the experiments, from which these curves were obtained, bear to one another, and in how far the comparison of them is justifiable. As a matter of fact, the experiments described in my paper extended over a considerable time, and the curves given there were nearly all obtained at separate times. Each curve was an entity in itself, and bore no special relation to any of the other curves. The electrometer used in these experiments was one whose sensitiveness varied from day to day. Each curve involved an entirely separate set of experiments made for the sole purpose of showing that at that particular pressure the law of recombination was represented by the formula $\frac{1}{n_t} - \frac{1}{n_0} = \alpha t$. This was the only purpose for which these curves were intended, and I think under the conditions the only legitimate one to which they may be put. No particular precautions were taken to keep such conditions as the sensibility of the electrometer the same in the different cases, nor were the experiments intended to be compared with one another. Consequently any comparison between the values of α obtained from the different curves is quite valueless, and no conclusion can be drawn from it. These remarks apply also to the values given in the last column of Mr. Walker's second table ; for, although they were not obtained directly from the curves, they were derived from observations taken at the same time as the observations for the curves.

In addition, these curves are quite unsuitable, especially at low pressures, for the purpose of making absolute determinations as Mr. Walker has done. Theoretically formulæ (4) and (5) are all right, but in actual experiment they are quite unsuitable for making absolute determinations. The intensity of the rays is an important factor here. Mr. Walker says in his paper that he does not see how the intensity of the rays can affect the determination of α. He says: " If the theory,

as proposed, is correct, the values of α, whether obtained from (3) or from the relation between N_t and N, must be the same and quite independent of the intensity of the rays, provided that the intensity is the same for any one determination of N and Q;" and, he might have added, of N_t and N. I admit that the determination of α ought not to be affected by the intensity of the rays so long as the intensity remains constant during the measurement of *all* the quantities involved; but suppose the intensity of the rays when determining N_t is different from the intensity when determining N, then the variation *certainly would* affect the result. Formulæ (4) and (5), which Mr. Walker used in determining α_N and α_Q, involve the quantity λ which depends upon t, which in turn depends upon N_t. Now suppose that at any point in the experiments necessary for one of these curves, which involved a long series of readings, the intensity of the rays should slightly increase and maintain this increased intensity for the remainder of the curve, then the curve would become slightly too flat, and the value of t obtained from it for any given value of N_t might be, especially at the lower pressures, considerably too large. The value of λ thus obtained would be smaller than the true value of λ corresponding to N. This would of course cause a much greater error in formula (5), since λ appears in the second power instead of the first. This is quite apparent in the values obtained by Mr. Walker, for the values of α_Q are all still farther from the true value than those for α_N.

These two methods of determining α also involve the absolute measurement of a very short time t with considerable accuracy, to do which elaborate precautions are necessary. In my experiments I do not claim to have attained this necessary accuracy in the measurement of the time intervals. The relative value was of more importance than the absolute value.

Besides this, since these methods of determining α_N and α_Q involve N_t, it means the measurement of much smaller effects than in the case of N and Q. One of the great difficulties of the experiment was to obtain an apparatus to give large effects for measurement so that the results would be more reliable. The results from the formula which I used involving Q and N are, since N and Q are large, much more to be relied upon than those which depend upon the determination of smaller quantities.

Besides these facts, Mr. Walker has made his determinations of α by all three formulæ in every case from single isolated experiments. As any one who has ever had any experience

with measurements of ionization by Röntgen rays knows, a single experiment can very rarely, if ever, be depended upon for an accurate absolute determination of a quantity. In making my determination of α I made a large number of experiments, and the value which I gave in my paper was the mean value obtained from about nine different experiments which gave fairly consistent results. I think the accuracy of my method and the absolute value obtained is pretty well confirmed by the close agreement of this value with those obtained by Prof. Townsend * and M. Langevin †. Prof. Townsend obtained the value $3420 e$ and M. Langevin the value $3200 e$, while the result of my experiments gave the value $3384 e$, where e is the charge on an ion. These three values were obtained by entirely different methods, and considering the experimental difficulties involved, the agreement is remarkably good; and I think it leaves little room for doubt in regard to these being very near the true value of α.

Again, in regard to the dependence of α upon pressure, it appears to me that the method which I adopted for comparing α at the different pressures is a far more definite and reliable one than that which Mr. Walker proposes. In my comparison of α at any two pressures, as described on pp. 297–298, the measurements were made within a comparatively short interval of time, and all the conditions were kept as nearly constant as possible; while Mr. Walker's method involves the comparison of entirely independent experiments which extended over an interval of about ten days, and in which no precautions whatever were taken to keep the conditions constant from day to day. I cannot see why he discards my method in preference to the other.

In preparing his third table of numbers he has again fallen into the error of comparing independent experiments which have no particular connexion with one another, without having all the necessary data at his disposal. Mr. Walker has apparently ignored the fact that to compare the values of $\frac{N^2}{Q}$ one must know the capacity of the apparatus unless it remains constant throughout. In the experiments an adjustable condenser was used in parallel with the main apparatus; and as it was only my purpose to compare the observations for one pair of experiments, such as in A, the capacities were not known for this lot of experiments. All that concerned me in

* Phil. Trans. Roy. Soc. A, p. 157 (1899).
† *Thèse présentée à la Faculté des Sciences*, p. 151. Paris, 1902.

taking these observations was that these capacities were unaltered during the time of any one comparison. Consequently any comparison of the readings in the different sets A, B, C, &c., is quite absurd, and of no value whatever.

Mr. Walker seems to think that I tried to prove from the results given in my paper that Q was proportional to the pressure. In regard to this I might state that I made no such statement in that paper, nor drew any such conclusion from the results given there. This question has been independently investigated by Prof. Perrin * and by Rutherford and McClung †; and I would refer Mr. Walker to the papers where these experiments are described. I might also remind him that there is the question of secondary ionization from metals coming in here, especially at low pressures.

I hope, therefore, that the above remarks will set aside all doubts in Mr. Walker's mind as to whether the theory of recombination is supported by experiment. In regard to the question as to whether the saturation-current gives a measure of Q, I might just remind him what is meant by saturation current. By saturation current we mean the current produced by all the ions in the gas reaching the electrodes under the influence of the electric field before they have had time to recombine. Q is the total number of ions in the gas; therefore I cannot see how the saturation current can be anything else than a measure of Q.

Cavendish Laboratory, Cambridge.
May 20th, 1904.

XXIII. *On a simple Graphical Method of treating the Impact of Smooth Elastic Spheres.* By CHARLES H. LEES, *D.Sc.*‡

AS the methods of teaching mechanics are being discussed at the present time, it seems worth while to call the attention of teachers to a geometrical method of treating the impact of smooth elastic spheres which is so simple that it has probably occurred to many, although there is no mention of it in any of the text-books with which I am acquainted.

The diagram of velocities may serve as a basis for graphical methods, but they are not so compact as those based on the diagram of momenta, since momentum is conserved on impact. The following method depends on the diagram of momenta.

* *Annales de Chimie et de Physique*, xi. p. 496 (1897).
† Rutherford and McClung, Phil. Trans. A, cxcvi. (1901).
‡ Communicated by the Author.

Case (1). *Direct Impact.*

Let a smooth elastic sphere of mass m_1 moving in a straight line with velocity v_1 impinge on another of mass m_2 moving in the same straight line with velocity v_2.

Calculate m_1v_1 and m_2v_2 the momenta of the two spheres.

From some point A_1 (figs. 1 and 2) draw A_1B in direction and magnitude equal to m_1v_1 the momentum of the mass m_1.

Fig. 1.
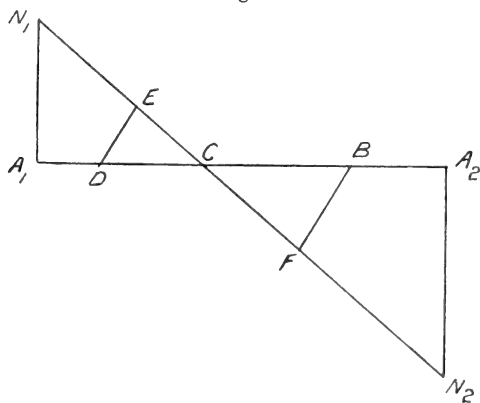
Fig. 2.
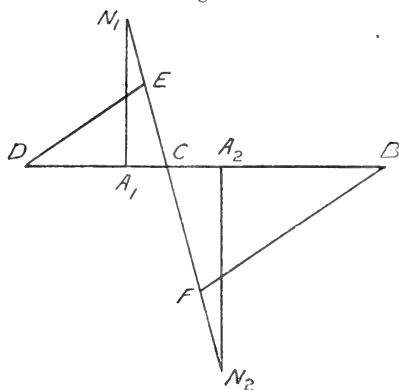
From B draw BA_2 equal in direction and magnitude to m_2v_2 the momentum of the mass m_2. From A_1 and A_2 draw on opposite sides of A_1A_2 two perpendiculars to A_1A_2 and measure off $A_1N_1 = m_1$, and $A_2N_2 = m_2$. Join N_1N_2 and let it cut A_1A_2 in C. Then, since $\dfrac{A_1C}{m_1} = \dfrac{CA_2}{m_2}$, A_1C is the momentum of m and CA_2 that of m_2 at the end of the first portion of

the impact, when the spheres are moving with the same velocity.

To find the momenta after separation of the spheres, since the change of momenta during the second portion of the impact is e times that during the first portion, e being the coefficient of restitution, measure off from C along N_1N_2 in the direction of B a length CF $= 1$ on any convenient scale, and in the opposite direction a length CE $= e$. Join BF and draw through E, ED parallel to BF cutting A_1A_2 in D. Then, since CD $= e$BC, A_1D is the momentum of m_1, and DA$_2$ that of m_2 after impact.

The velocities after impact may be found from these momenta by dividing by the masses.

Case (2). *Oblique Impact.*

If the impact is oblique, then, since the components of the momenta perpendicular to the line of centres are unaffected by the impact, while the components of the momenta along the line of centres are affected, the latter are determined and treated as in the previous case.

Fig. 3.
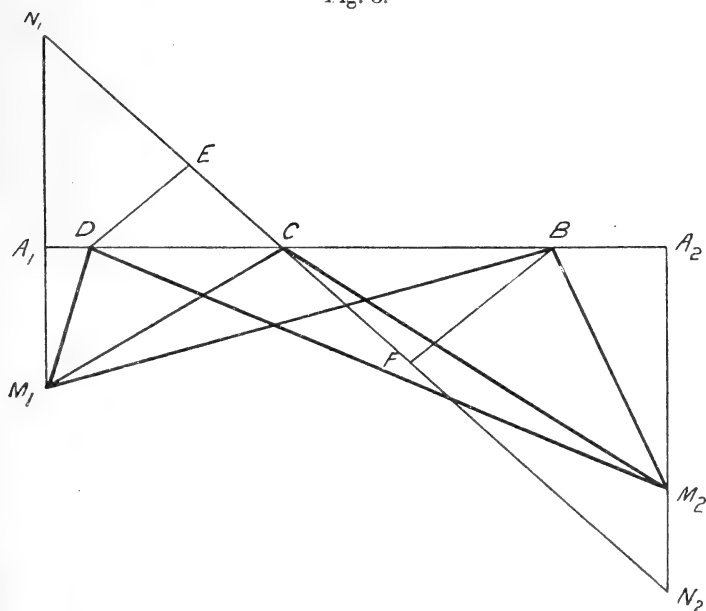
Let M$_1$B represent in magnitude and direction the momentum m_1v_1, BM$_2$ the other m_2v_2. Draw through B a line parallel to the line of centres and let the perpendiculars on

to it from M_1 and M_2 cut it in A_1 and A_2. Cut off from the perpendicular through A_1 a length $A_1N_1 = m_1$ and from that through A_2 (produced if necessary) a length $A_2N_2 = m_2$ in the opposite direction to A_1N_1. Join N_1N_2 and let it cut A_1A_2 in C. Then, as before, M_1C and CM_2 are the momenta of the spheres at the end of the first portion of the impact.

Measure from C along N_1N_2 in the direction of B a length $CF = 1$, and in the opposite direction $CE = e$. Join BF and draw ED parallel to BF. M_1D and DM_2 are in direction and magnitude the momenta of the two spheres after impact.

The velocities are found from them by dividing by the masses.

XXIV. *A new Automatic Gas-Pump.* By C. E. S. Phillips[*].

THIS apparatus is constructed upon a plan which enables the pump, when once set in operation, to continue automatically and to produce as perfect a Torricellian vacuum as is possible.

It has been devised with a view to providing a comparatively portable machine suitable for special laboratory work or for researches requiring prolonged pumping, and consists of three distinct parts, viz. :—a small motor-driven mechanical pump, a four-way control valve, and a modified Toepler apparatus by which the final vacuum is obtained. The action is as follows :—A vessel (say a Röntgen-ray lamp) having been sealed on to the Toepler pump, the motor is set in motion, and a vacuum produced in the whole apparatus equivalent to within about ·5 inch of the barometric column.

The mercury now fills the lower chamber L (fig. 1) of the Toepler and completes an electrical circuit, which includes an electromagnet capable of so moving the controlling valve that it allows air at atmospheric pressure to enter and drive the mercury slowly into the upper chamber U. The Toepler pump operates in the usual manner. At the instant the quantity of mercury, moving as described, diminishes in L, the circuit just completed is broken, but the mercury soon accumulating in the trap T " makes " a second circuit which moves the slider of the valve back to its original position.

The chamber is thus connected with the mechanical pump, the mercury falls, and the cycle is repeated automatically.

The gas removed by the Toepler would accumulate in the trap T were it not that the mechanical pump is periodically connected directly therewith. This is achieved by the control valve, when permitting air to enter L also placing the

Fig. 1.
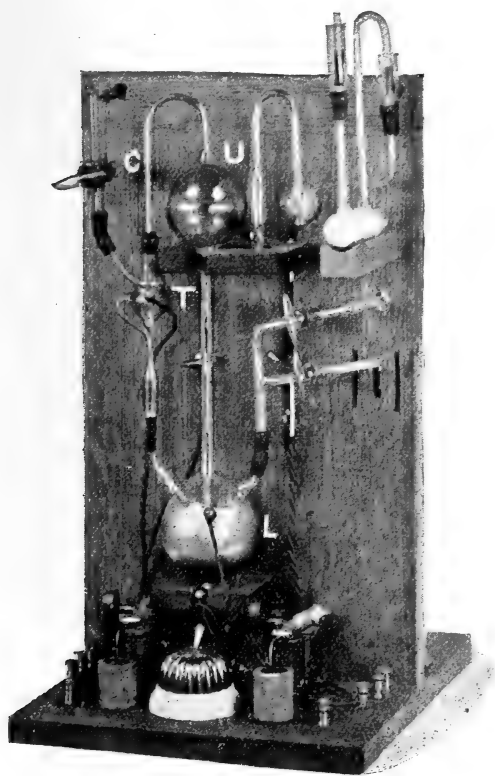
mechanical pump in communication with the trap. The pressure in T is thus kept fairly constant, and the difference between it and that within the Toepler-pump head may at any moment be observed by measuring the height of mercury in the capillary-fall tube C.

The control device consists essentially of a hollow slider moving over a smooth flat bed, and covering or uncovering

ports in the same manner as an ordinary slide-valve, but
since the travel of this slider S (fig. 2) is only one-eighth of

Fig. 2.
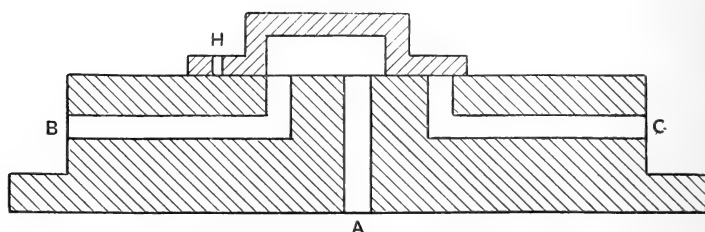
A communicates with the mechanical pump.
B   ,,   ,,   lower chamber L.
C   ,,   ,,   the trap T.

an inch many devices are possible for actuating it; an
electromagnet appears very suitable.

Two relays are introduced, one into each circuit, in order
that the current which operates the control-valve may be
broken at external mercury cups.

In the circuit, the completion of which moves the slider
into the position for allowing air to enter L, the action of
the relay is retarded, for the purpose of enabling the mercury
in the Toepler pump to remain in the lower chamber during
a distinct interval of time before ascending.

A small hole H bored through the slider-flange ensures
that the inrush of air into the lower chamber may be checked,
and the upward movement of the mercury therefore take
place slowly. The ideal conditions for efficiently working
the Toepler pump are therefore attained, for the mercury
rises slowly, sweeping out before it very efficiently the gas
which fills the upper chamber; on the other hand, it falls
rapidly (so that no time may be lost) and remains in the
lower chamber for a fixed period before again ascending.

Experiment has shown that the apparatus is fairly rapid
in its action. Careful tests under specific conditions are in
progress, but not yet sufficiently complete for publication.
In a preliminary trial, however, the capacity of the chamber
U being 160 c.c., 5 lbs. of mercury were raised three times
per minute, with the result that a Röntgen-ray bulb of
200 c.c. capacity was exhausted in half-an-hour. The glass
work is easily removable for cleaning or repairs, and the wood
supports fitted with adjustable brackets. The height of the
complete apparatus is 18 inches.

XXV. *A Model illustrating the Propagation of a Periodic Electric Current in a Telephone Cable, and the Simple Theory of its Operation.* By J. A. FLEMING, *M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London* *.

[Plate IV.]

THE propagation of alternating currents of electricity along linear conductors has been the subject of much mathematical investigation. Its importance in reference to the practical operation of submarine cables, telephone-lines, and more recently of the radiating antennæ in Hertzian wave telegraphy, is well understood. Nevertheless, practical telegraph and telephone engineers have not always shown readiness to assimilate the ideas elaborated by theorists, and in some cases unsound theories of the phenomena have been promulgated. The assistance which practical men derive from considering the operation of a working model when appropriating physical ideas is considerable, and the author has found that for teaching purposes the model here described is of great use in making plain the meaning of mathematical expressions.

The simple theory of it may first be given. The object is to explain the propagation of a periodic or alternating electric current in a telegraph or telephone cable having resistance, inductance, capacity, and leakage, or insulation conductance of a certain value per mile or knot.

The discussion necessarily proceeds on the assumption that the currents and electromotive forces with which we are concerned are of simple harmonic form, and accordingly the only mathematical symbolisation necessary is that required for representing a simple harmonic motion or simple sine function and its time-rate of change. This is most readily achieved by the use of the complex quantity to denote the maximum value and phase of the periodic quantities with which we are concerned. The representation of a vector quantity by the symbol $a + jb$, where a and b are lengths of lines and j is the *sign of perpendicularity* denoting that b is at right-angles to a is now so universally understood that no further explanation of it is necessary.

Consider, then, in the first place, an electric cable indefinitely extended in one direction, and at the terminated end a simple periodic electromotive force applied. Let the cable have a capacity C farads, an inductance L henrys, a

copper resistance R ohms, and a dielectric or insulation resistance *r* ohms, all per unit of length.

If we take any element of length in the run of the cable, then there is a difference between the potential and the current at the two ends of this element. Let δv and δi be this difference, *v* and *i* being the potential and current at the generator end of the element. Then the symbolical expression for the manner in which this difference of potential and of current is created, is given by the familiar equations :

$$L\delta x\frac{di}{dt} + R\delta x\, i = \delta v, \quad \cdots \quad (1)$$

$$C\delta x\frac{dv}{dt} + K\delta x\, v = \delta i, \quad \cdots \quad (2)$$

where K is written for $1/r$. Hence

$$L\frac{di}{dt} + Ri = \frac{dv}{dx}, \quad \cdots \quad (3)$$

$$C\frac{dv}{dt} + Kv = \frac{di}{dx} \quad \cdots \quad (4)$$

If *i* and *v* both vary in a simple harmonic manner, and if I and V are their maximum values during the period ; then, since the maximum values of di/dt and dv/dt considered as vectors are at right angles to the maximum values of *i* and *v*, we may write (3) and (4) in the form

$$\frac{dV}{dx} = (R + jpL)I, \quad \cdots \quad (5)$$

$$\frac{dI}{dx} = (K + jpC)V, \quad \cdots \quad (6)$$

where *j* is the sign of perpendicularity, and $p = 2\pi$ times the frequency *n* of the current.

Separating the variables by differentiation, we have

$$\frac{d^2V}{dx^2} = (R + jpL)(K + jpC)V, \quad \cdots \quad (7)$$

$$\frac{d^2I}{dx^2} = (R + jpL)(K + jpC)I. \quad \cdots \quad (8)$$

The above are the well-known vector equations for the propagation of a simple periodic current in a cable having the four constants L, R, C, K.

If we write P for $\sqrt{R + jpL} \cdot \sqrt{K + jpC}$, we have the

equations

$$\frac{d^2V}{dx^2} = P^2V, \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$\frac{d^2I}{dx^2} = P^2I, \quad . \quad . \quad . \quad . \quad . \quad (10)$$

as the differential equations for the potential and current at any point in the cable.

An obvious solution of the above equations is

$$V = a\epsilon^{+Px} + b\epsilon^{-Px}. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

$$I = \frac{P}{R+jpL}\left\{a\epsilon^{+Px} - b\epsilon^{-Px}\right\}, \quad . \quad . \quad (12)$$

where a and b are constants of integration, and ϵ is the base of Napierian logarithms. The quantity P is clearly a complex quantity and can be expressed in the form $\alpha + j\beta$.

Hence if $\sqrt{R+jpL}\sqrt{K+jpC} = \alpha + j\beta$, we must have :—

$$\alpha^2 + \beta^2 = \sqrt{R^2+p^2L^2}\sqrt{K^2+p^2C^2}$$

and

$$\alpha^2 - \beta^2 = RK - p^2LC,$$

or

$$2\alpha^2 = \sqrt{(R^2+p^2L^2)(K^2+p^2C^2)} + (RK - p^2LC), \quad (13)$$

$$2\beta^2 = \sqrt{(R^2+p^2L^2)(K^2+p^2C^2)} - (RK - p^2LC). \quad (14)$$

The above equations are well-known expressions. We shall call α and β the secondary constants of the cable.

Returning, then, to the case of the semi-infinite cable, we shall take the origin of coordinates at the end at which the simple periodic electromotive force of maximum value E is applied.

Hence for $x = 0$ we have $V = E$, and for $x = \infty$ we have $V = 0$.

Since, however, in obtaining the original differential equations, we reckoned x in the direction in which potential and current *increase* in value, we have to write $-x$ for x in the general solutions (11) and (12) before we can apply them to the above particular case. Making this change and determining the constants a and b from the terminal conditions, we arrive at the particular solutions of (9) and (10) applicable to the case of a semi-infinite cable having a periodic electromotive force E applied at one end. The potential V and the current I at any point in the cable at a distance x from the generator end is then given by the equations

$$V = E\epsilon^{-Px}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

$$I = E\epsilon^{-Px}\frac{\sqrt{K+jpC}}{\sqrt{R+jpL}}. \quad . \quad . \quad . \quad (16)$$

It should be clearly understood that in speaking of the potential or current at any point in the cable we mean the maximum value of the potential or current regarded as a vector.

Since $P = \alpha + j\beta$ and $\epsilon^{\pm j\beta x} = \cos \beta x \pm j \sin \beta x$, we can write (15) and (16) in the form

$$V = E\epsilon^{-\alpha x}(\cos \beta x - j \sin \beta x), \quad . \quad . \quad . \quad . \quad (17)$$

$$I = E \frac{\sqrt{K + jp\mathrm{C}}}{\sqrt{R + jp\mathrm{L}}}\epsilon^{-\alpha x}(\cos \beta x - j \sin \beta x). \quad . \quad (18)$$

Let us notice the physical signification of the above expressions. Equation 17 shows us that the potential V in magnitude and direction at any point at a distance x in the cable, reckoned from the generator end, is obtained by multiplying the magnitude of the terminal applied voltage E by two factors, (i.) by the attenuation factor $\epsilon^{-\alpha x}$, and (ii.) by the phase factor $(\cos \beta x - j \sin \beta x)$. At any distance x from the origin, V is less than E in magnitude in the ratio of $\epsilon^{-\alpha x}$ to 1 and is shifted backwards through an angle βx relatively to E as regards phase.

This is easily seen to be the case if it is remembered that $\cos \theta + j \sin \theta$ is a rotating operator, and when it operates on a vector $a + jb$ it gives as a result a vector of the same size, viz. $\sqrt{a^2 + b^2}$, but shifted forwards through an angle θ. The size of the vector $(a + jb)(\cos \theta + j \sin \theta)$ is $\sqrt{a^2 + b^2}$, but its slope ϕ is such that $\phi = \tan^{-1} \frac{b}{a} + \theta$ where $\tan^{-1} \frac{b}{a}$ is the slope of the original vector $a + jb$. Hence the expression (16) merely means that the potential at any point in the cable whose distance from the origin is x is less than E in magnitude in an assigned ratio, and is shifted backward in phase by an assigned angle.

If x' is the distance from the generator end at which the maximum voltage has fallen to half the numerical value at the origin, then x' is given by the equation

$$\tfrac{1}{2} = \epsilon^{-\alpha x'},$$

or

$$x' = \frac{0 \cdot 7033}{\alpha}. \quad . \quad . \quad . \quad . \quad (19)$$

Again, since $\cos \beta x = \cos (\beta x + 2\pi)$ it is obvious that the wave-length of the potential wave is $2\pi/\beta$, and therefore the velocity of propagation of the potential wave is p/β.

These facts may be presented graphically as follows:—

Take a line OX (see fig. 1, Pl. IV.) to indicate the cable, and set up a perpendicular OE to represent in magnitude and direction, the voltage at the generator end. Then at equidistant points draw other lines decreasing in length in geometrical progression, and each shifted backwards or forwards in direction relatively to the preceding line by an equal angle. If we suppose these lines to revolve with equal angular velocities round their ends as centres situated at equidistant intervals on the line OX ; then their projections at the same instant on vertical lines drawn through their centres will represent at that instant the actual voltage at these points in the cable. The periodic change with time and distance may be represented by a working model made in the following manner :—
On a long steel axle AA′ are fastened a number of eccentric pulleys E_1, E_2, E_3, &c. (see fig. 2, Pl. IV.). The eccentricities of these wheels decrease in geometric progression, and each eccentric is set in phase backward behind its preceding neighbour by an equal angle. These wheels are embraced by cords C_1, C_2, C_3, &c. attached to balls or blocks of metal P_1, P_2, P_3, &c. sliding on vertical rods R_1, R_2, R_3, &c. placed below each eccentric wheel. These cords are all of the same length.

When the axle carrying all the eccentrics is revolved by a handle W, the blocks P_1 &c. will rise and fall with a nearly simple harmonic motion, and at any instant all the blocks will be situated on a sinuous curve of continually decreasing amplitude. As the eccentric axle revolves the motion of the balls will depict the progression of a wave of potential along a cable having capacity, inductance, resistance, and leakance.

Returning, then, to the expressions (13) and (14) for the value of α and β, we may give these more convenient forms.

Taking the formulæ (13) and (14), add to, and subtract from, the quantity under the radical the term $2p^2\text{CLKR}$, and after rearranging terms we obtain

$$2\alpha^2 = \sqrt{(\text{KR} + p^2\text{LC})^2 + p^2(\text{LK} - \text{CR})^2} + (\text{KR} - p^2\text{LC}) \quad (20)$$

$$2\beta^2 = \sqrt{(\text{KR} + p^2\text{LC})^2 + p^2(\text{LK} - \text{CR})^2} - (\text{KR} - p^2\text{LC}) \quad (21)$$

If the cable constants have such relation that $\text{LK} - \text{CR} = 0$ or $\text{L/R} = \text{C/K}$, we have Mr. Oliver Heaviside's distortionless cable, and then obviously when this is the case

$$\alpha = \sqrt{\text{KR}}, \quad \beta = p\sqrt{\text{CL}}. \quad . \quad . \quad . \quad (22)$$

Hence we have for the abscissa of half voltage x' and the potential wave-velocity w the values

$$x' = \frac{0.7033}{\sqrt{\mathrm{KR}}}; \quad w = 1/\sqrt{\mathrm{CL}}. \quad . \quad . \quad . \quad (23)$$

Both these quantities are independent of the periodicity, and hence for this cable all potential waves travel at the same rate and are attenuated in the same ratio.

If, therefore, such a cable could be practically constructed with a small enough value for $\sqrt{\mathrm{KR}}$, telephonic speech would be possible through it with results not possible in the case of non-distortionless cables.

The model shows clearly that the attenuation of the simple periodic wave depends on the value of α, that is on the variation of eccentricity of the wheels, and that the wave-length is determined by the value of β.

It is not then difficult even for a non-mathematician to comprehend that if a complex periodic electromotive force acts at one end, which may be resolved into the sum of a number of simple periodic electromotive forces, the result in general will be to produce corresponding simple periodic waves which travel at different speeds and degrade at various rates. Hence, in a comparatively short distance the initial complex wave form is so altered that it becomes unrecognizable. Herein, therefore, resides the cause of the practical limitations of telephony. The remedy for it rests in the alteration of the primary constants of the cable so as to bring about the necessary changes in the secondary constants.

There are, however, necessary practical limits to these changes. In overhead lines where capacity is relatively small, it is not difficult to introduce sufficient inductance (as Prof. Pupin has done) in the form of separated inductance-coils, and provided these are sufficiently close together within the limits of a wave-length the result is equivalent practically to continuous inductance. The same remedy for distortion could no doubt be applied to underground telephone trunk-lines, although the writer is not aware that it has been tried.

On the other hand, an addition to the inductance is not easily made in the case of a submarine cable. Any interference with the continuity of size or great increase in bulk or weight would hardly be permitted.

Hence, in the case of cables the favourite prescription for the defect has been that of creating a large dielectric conductance by artificial leaks. The effect of any such proposed added leakage can easily be foretold.

Taking the expressions for $2\alpha^2$ and $2\beta^2$ in (13) and (14)

we may write them in the following form:

$$2\alpha^2 = p\mathrm{L}\left(1 + \frac{\mathrm{R}^2}{p^2\mathrm{L}^2}\right)^{\frac{1}{2}} p\mathrm{C}\left(1 + \frac{\mathrm{K}^2}{p^2\mathrm{C}^2}\right)^{\frac{1}{2}} + (\mathrm{RK} - p^2\mathrm{LC}) \quad (24)$$

$$2\beta^2 = p\mathrm{L}\left(1 + \frac{\mathrm{R}^2}{p^2\mathrm{L}^2}\right)^{\frac{1}{2}} p\mathrm{C}\left(1 + \frac{\mathrm{K}^2}{p^2\mathrm{C}^2}\right)^{\frac{1}{2}} - (\mathrm{RK} - p^2\mathrm{LC}). \quad (25)$$

If we employ any such frequency as 100 to 200, then p is a number of the order of 1000. In all actual well-insulated submarine or underground cables, the quantity $\mathrm{K}^2/\mathrm{C}^2$ per mile is a small quantity of the order of 10^{-4}, whilst $\mathrm{R}^2/\mathrm{L}^2$ is a large number of the order of 10^4 or 10^6. Hence, if the frequency employed is anything of the order of that of the waves of the speaking voice, the quantity $\mathrm{K}^2/p^2\mathrm{C}^2$ can be neglected, whilst the value of $\mathrm{R}^2/p^2\mathrm{L}^2$ may approximate to unity, or say to a number such as 2, 3, or 4 near to unity. Accordingly, in practice we might have for the value of $\left(1 + \frac{\mathrm{R}^2}{p^2\mathrm{L}^2}\right)^{\frac{1}{2}}$, reckoned per mile or per knot, a number of the order of, say, 2.

Hence, in this case, the values of α and β per mile would reduce to:—

$$2\alpha^2 = p^2\mathrm{LC} + \mathrm{RK} \quad . \quad . \quad . \quad . \quad (26)$$

$$2\beta^2 = 3p^2\mathrm{LC} - \mathrm{RK} \quad . \quad . \quad . \quad . \quad (27)$$

Again, in the case of all well-insulated cables the value of RK is very small compared with $p^2\mathrm{LC}$ for such frequencies as above mentioned, and hence as a first rough approximation we can calculate the values of α and β for most actual submarine cables from the expressions

$$\alpha = \frac{p\sqrt{\mathrm{LC}}}{\sqrt{2}}, \quad \beta = p\sqrt{\frac{3\mathrm{LC}}{2}}.$$

In this case α is a function of the frequency, but the wave-velocity p/β is independent of the frequency. Accordingly there is attenuation depending on frequency. For any well-insulated cable, however, we have the value of α nearly given by the equation,

$$2\alpha^2 = p^2\mathrm{LC}\left(1 + \frac{\mathrm{R}^2}{p^2\mathrm{L}^2}\right)^{\frac{1}{2}} + \mathrm{RK} - p^2\mathrm{LC}; \quad . \quad . \quad (28)$$

and from this equation combined with (19) we can always calculate the distance for drop to half initial voltage or current, which may be roughly said to represent the limiting distance of good telephony.

Suppose, for instance, that we consider as an example any

long submarine cable, say the Ireland-Newfoundland (1880) cable. The length (l) is 1423 knots $= 264 \cdot 2 \times 10^6$ centims. The resistance per knot (R) $= 4 \cdot 161$ ohms, the capacity per knot (C) $= 0 \cdot 315$ microfarad, the insulation resistance per knot (1/K) $= 400$–500 megohms*.

The value of the inductance per knot (L) is not so definitely known. The copper core weighs 300 lbs. per knot, and is equivalent in section to a round wire of $0 \cdot 34$ centim. in diameter. As a first approximation, if we calculate the total inductance L from the formula for the inductance of a straight single wire,

$$L = 2l \left(\log_\epsilon \frac{4l}{d} - 1 \right) \quad . \quad . \quad . \quad . \quad (29)$$

we find $L = 11 \cdot 5$ henrys, from which we deduce the average inductance per knot (L) to be $0 \cdot 008$ henry.

But the iron armour may increase this value 100 or at most 200 times. Hence the value of L/R per knot may be as low as $0 \cdot 002$, or perhaps as great as $0 \cdot 02$ or $0 \cdot 4$. On the other hand, the value of C/K per knot is 150 nearly, and hence the cable is very far from being distortionless.

If we calculate α corresponding to a value of $n = 170$, $p = 1000$, and an inductance $0 \cdot 008$ henry per knot, then we have $\alpha = 0 \cdot 012$, and the distance for drop to half initial voltage is 60 knots. If we attempted to cure distortion by decreasing the dielectric resistance per knot to 1/150 of a megohm, or about 6000 ohms, then we could make α independent of wavelength, but it would be increased in magnitude to a value $0 \cdot 021$, and the attenuation would be worse. On the other hand, if we could increase the inductance per knot to $0 \cdot 8$ of a henry, the dielectric resistance being only reduced to 2/3rds of a megohm per knot, the distortion would be cured, and the value of α reduced to $0 \cdot 0021$.

The distance for decay to half initial voltage would be raised to 350 knots, and a great improvement effected. This is only an illustration of the importance of inductance on which Mr. Oliver Heaviside has insisted for so many years. On the other hand, the difficulty is to apply the theoretical remedy practically in the case of submarine cables, and it is not easy to see how it is to be done.

Suppose, however, that we could make the resistance per knot

---

* The above data are taken from Munro and Jamieson's 'Pocket-book of Electrical Rules and Tables,' 4th ed. p. 286. The term *naut* is used by some writers to denote a nautical or sea mile, a length of 2029 yards, whilst others employ the term *knot* for the above length. Strictly speaking a knot is the name for a speed, viz. one nautical mile per hour.

3 to 4 ohms, the capacity per knot 0·3 microfarad, the insulation resistance 300 megohms per knot, and the inductance say 300 henrys. We should then have $L/R = C/K = 100$. Hence we should have $\alpha = 0·0001$ and $\beta = 10^5$ corresponding to $p = 1000$. Accordingly the distance for decay to half voltage would be some 7000 knots. Such an achievement may be quite beyond the reach of practical work, but an approximation to it is perhaps not impossible. It cannot be done, however, merely by increasing the iron armour or bringing it nearer to the copper, because in that case the capacity is increased very considerably. The annulment of distortion by creating lateral leakage either through inductive or non-inductive leaks always leads to large values of α. In other words, to great attenuation. Hence, although the distortion of the complex potential or current wave may be cured, the decay of amplitude would extinguish audibility as far as telephonic transmission is concerned within quite moderate distances, and far less than that which could be obtained by the right application of inductance.

As a small contribution to this practical consideration of the problem, the model here described and method of discussing the theory may perhaps be useful.

Note added July 9th, 1904.

In reference to the above method of discussing the problem, it has been represented to the author that it would assist some mathematical readers if the steps by which the usual algebraical expressions for the potential and current at any point in the cable can be deduced from equations (17) and (18), were set out more at length.

The equations (17) and (18) are expressions showing the manner in which the potential and current at any distance x along the cable, considered as vectors, are related to the vectors representing the potential and current at the origin at the same instant.

If we desire to ascertain the state of affairs at any time t we have to consider these vectors as rotating round their extremities with a periodic time T. Accordingly the proper expression for the vector representing the potential at the origin at any time t from the beginning of the epoch will then be Ee^{jpt}, where $p = 2\pi/T$. Hence the corresponding expression for the vector representing the potential at any distance x along the cable at the same instant will be obtained by multiplying equation (17) by ϵ^{jpt}, and will therefore be

given by the equation

$$V = E\epsilon^{jpt}\,\epsilon^{-(\alpha + j\beta)x}$$

or by $V = E\epsilon^{-\alpha x}\left\{\cos(pt - \beta x) + j\sin(pt - \beta x)\right\}.$ (17′)

If, therefore, the numerical value of the potential at the origin at any instant is represented by the real part of $E\epsilon^{jpt}$, viz. by E cos pt, then the numerical or algebraic value of the potential at the same instant but at the distance x along the cable will be given by the real part of (17′), viz. by

$$v = E\epsilon^{-\alpha x}\cos(pt - \beta x).$$

The above equation is the ordinary algebraic expression for the potential in a semi-infinite cable at any time t and any distance x from the origin. Under the above conditions the time t would be reckoned from the instant when the potential at the origin has its maximum value.

The last equation is the usual expression for a wave of decreasing amplitude and wave-length $2\pi/\beta$ moving with velocity p/β.

Similar expressions can be obtained for the numerical or algebraic value of the current in the cable at any place and time.

XXVI. *On the Discharge of Electricity from a Nernst Filament.* *By* GWILYM OWEN, *B.A.* (*Cantab.*), *M.Sc.* (*Vict. and Liverpool*); *Christ's College, Cambridge;* 1851 *Exhibition Research Scholar of the University of Liverpool* *.

I T is well known that from incandescent metals and carbon filaments there is a discharge of electricity both positive and negative. The experiments of Prof. J. J. Thomson† on the discharge from a carbon filament have shown that the carriers of the negative electricity at very low pressures are corpuscles. The carriers of the positive electricity, on the contrary, are of atomic dimensions, their masses being usually many times that of the atom of hydrogen.

The present paper contains an account of some experiments made to see if such a discharge of electricity takes place in the case of the glowing filament of a Nernst lamp. While the experiments were in progress, I found that E. Bose ‡

mentions incidentally in a paper in the *Annalen der Physik* that he obtained a current of 10^{-4} ampere between a Nernst filament glowing in a vacuum and a metal electrode 10 cms. distant.

It may be mentioned that the Nernst filament is composed of certain oxides of metals (so-called " rare earths ") and has the property of being able to stand an exceedingly high temperature.

In all the experiments the lamps used were of the B type, adapted to carry half an ampere when connected to a 100-volt alternating supply. The filaments were 0·75 mm. in diameter, the length of the glowing portion being 8 mms. The iron wire resistance (20 ohms) supplied with the lamp was always kept in series with the filament.

The experimental difficulties in this investigation were somewhat greater than in investigations on leaks from hot wires, insomuch as the Nernst filament has to be made red-hot by some external source of heat before an electric current can be sent through it, the substance of which the filament is made being a non-conductor at temperatures below a red-heat. This difficulty was, however, got over by making the electrodes by which the electricity discharged from the filament was collected serve the further purpose of acting as the " heater." The preliminary apparatus in which this plan was put into execution may be briefly described, as it affords a convenient way of showing the leak from a Nernst filament. The filament fixed to its porcelain base was erected inside a lamp chimney. The original heater surrounding it had been removed, and in its place a spiral of thick platinum wire was fixed in such a way as to be insulated from the filament. Owing to the danger of breaking the fragile filament in endeavouring to coil a spiral of thick platinum around it, the spiral was made in the following way :—A flat grating was first made. This was then bent round a pencil so as to be nearly closed. The gap left was just wide enough to allow the filament to pass through. The spiral was attached to its copper leads in such a way that the filament lay along its axis. A current of about 10 amperes was sufficient to raise the spiral to a bright yellow heat. In a few seconds the temperature of the filament was raised high enough for it to conduct. When this took place the current through the spiral was cut off, and the latter connected to earth through a sensitive galvanometer. The currents for heating both the spiral and the filament were got from the secondaries of a transformer, the primary of which was connected to the 100-volt alternating town mains. The primary had 166

turns. The secondary employed to supply the current through the filament had 151 turns, while that used to heat the spiral contained 50 turns. As the secondaries were fairly well insulated, the filament could be raised to any desired potential by connecting it to one pole of a battery of small storage-cells, the other pole being earthed.

By means of this preliminary apparatus it was found that there was a large negative leak from the glowing filament at atmospheric pressure, together with a much smaller positive leak. It was further found that the filament could be kept glowing in a vacuum without much vapour being evolved, provided that a large resistance (about 100 ohms) was inserted in series with the filament so as to prevent the temperature reaching too high a value. If the temperature of the filament exceeded a certain value, large quantities of some metallic vapours were evolved (at low pressures) which condensed in a thick black deposit on the walls of the tube. In all the experiments to follow, the temperature of the filament was kept too low for this evolution of vapour to take place to any considerable extent.

It may be mentioned that the filament could not be made to start glowing in a vacuum, the heat *radiated* from the incandescent spiral not being sufficient to raise the temperature of the filament to the critical value at which conduction commences. A pressure of 2 or 3 mms., however, was enough to supply the necessary *conduction* of heat. The platinum heater was therefore never heated except at fairly high pressures, so that the purity of the air in the apparatus was not impaired to any extent by the gases given out from the glowing platinum.

Having found that a good vacuum could be maintained when the filament is glowing, a new apparatus was made which would enable the value of e/m for the carriers of the electricity discharged from the filament to be determined. All the results given in the paper were obtained with this new apparatus, which is shown in figs. 1 and 2. Fig. 1 gives a front view of the tube L, a side view of which is shown in fig. 2. The filament F, after having been cut together with its short copper leads from its porcelain base, was firmly fastened to iron wires rigidly connected together by a bar of lead glass (not shown in fig. 1) melted onto them. The iron wires were in their turn attached to copper leads CC. AA are the wires of the thermal junction which served to measure the temperature of the filament. D is an earth-connected brass cylinder having two narrow slits parallel to the axis and reaching from one end of the cylinder

to about the middle. The slits were just wide enough to allow the cylinder being slipped round the filament and the electrodes when the former was in position. In fig. 1 the wires of the thermal junction are shown passing through the cylinder D. The cylinder, however, was only employed in the experiments on the magnetic deflexion of the ions. In those experiments the use of the thermocouple was dispensed with. The filament was placed mid-way between two circular disks of platinum foil, 2·8 cms. in diameter and 7 mms. apart. A plan of the disks is shown in fig. 3. The black part of the disk represents a gap about 1 mm. wide formed by cutting a strip out of the platinum. To the points O, O′, thick platinum wires were welded, and these were fastened to the copper leads M, M (fig. 2). Only one lead for each disk is shown in the diagram. By means of a current of about 8 amperes the central U-shaped part of the disks could be raised to a bright red heat. By this means it was possible to "light" the filament. When the filament had been got to glow, the current through the disks was cut off and the disks were connected to earth through a galvanometer. The disks thus served the double purpose of acting as the "heater" and as the electrodes to collect the electricity discharged from the filament. The shaded portions in figs. 1 and 2 represent sealing-wax joints which were made air-tight by coating with shellac varnish. The wires forming the thermocouple were pure platinum and 10 per cent. platinum-rhodium, 0·0035 cm. in diameter. The junction was made by fusing the wires together in the oxyhydrogen flame. In order to standardize the thermocouple, a junction of the same wires as those used during the experiments was attached to a piece of stout platinum wire, which could be heated by means of an electric current. Very small grains of potassium sulphate were placed on the stout wire near the junction, and the current through the wire was increased until the grains of salt (observed through a microscope) just melted. The thermo-

electric force for this temperature (1066° C.) was found to
be 10280 microvolts. The current through the wire was then
increased until the thick platinum wire just melted. The
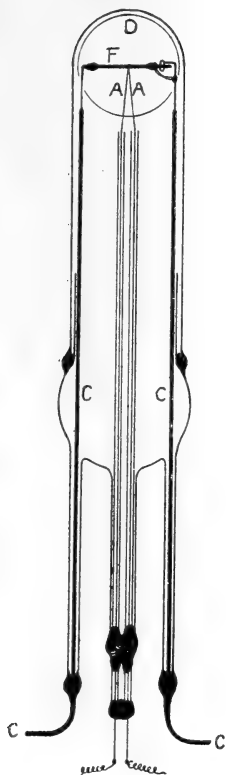
Fig. 2.

Fig. 3.
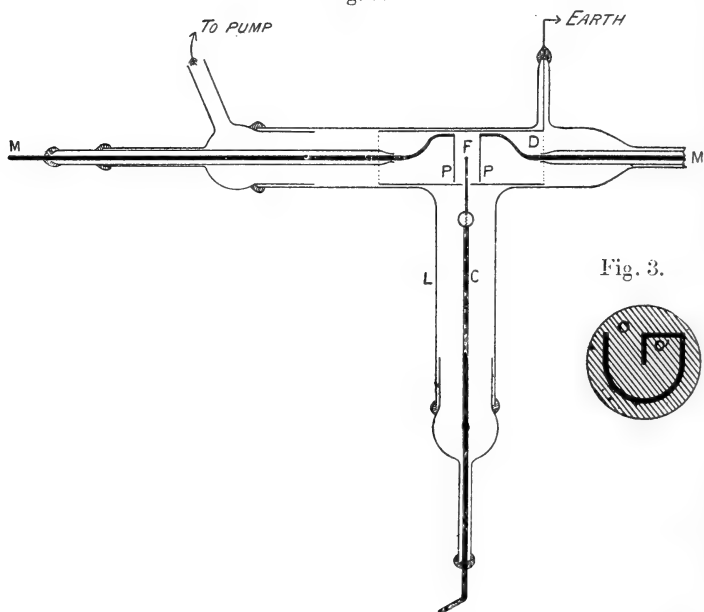
latter will melt before the wires of the couple, as the tem-
perature of the couple, owing to conduction along the wires,
must be somewhat lower than the actual temperature of the
melting platinum. The thermoelectric force at the melting-
point of the platinum was found to be 18290 microvolts.
The values of the thermoelectric force for the same couple at
the same temperatures (taking the melting-point of platinum
as 1780° C.) found by Le Châtelier are 10560 and 18200
respectively.

The Nernst filament passed between the wires of the couple
which were drawn as tightly as they would bear, the junction
being symmetrically placed on the upper side of the fila-
ment. The galvanometer used to measure the thermoelectric
current gave 1 division for $4·8 \times 10^{-8}$ amperes. This method
of measuring the temperature of the filament can hardly
claim for itself great accuracy in regard to giving the *actual*
temperature of the filament, as the temperature indicated by
the couple will depend largely upon the way the junction lies
on the filament. It is, however, a very convenient way of
keeping the temperature of the filament constant, and of

investigating the variation of the leak with the temperature if the *same* couple be used throughout the set of observations.

The currents for the filament and the heating electrodes were obtained from the secondaries of a transformer as described above.

The temperature of the filament could be varied by placing different resistances in series with the filament. Leaks greater than 10^{-8} amperes were measured by means of a D'Arsonval galvanometer, which gave 1 division for $7\cdot5 \times 10^{-9}$ amperes. Smaller currents were measured by means of a quadrant-electrometer which gave 1 division in 1 minute for $2\cdot6 \times 10^{-11}$ amperes when connected to $0\cdot1$ microfarad. Care was taken to see that there was no leak when the filament charged to a high potential was cold. One end of the filament was always connected to one of the terminals of a Weston voltmeter, the other terminal of which was connected to earth. To charge up the filament the same end as above was connected to one pole of a battery of small storage-cells, the other pole being earthed. Owing to the high resistance of the filament there was a drop of potential of some 70 volts between its ends when fully glowing. One end of the filament was therefore either at zero potential or at some definite potential, V say. The potential of the other end had the alternating values $V \pm 70$ volts. In the Tables which follow what is meant by no potential on the filament is that one end of the filament is earthed ; a potential of $+200$ volts on the filament means that the same end is at a potential of $+200$ volts.

The paper is divided into the following sections :—

(1) Discharge of electricity from the filament at atmospheric pressure. Variation of the leak with the potential.

(2) Variation of the negative leak with the potential at different pressures ; ionisation by collision.

(3) Variation of the positive leak with the potential at different pressures.

(4) Variation of the negative leak with the temperature in a vacuum.

(5) (*a*) Variation of the positive leak with the temperature in a vacuum.

    (*b*) Relative magnitudes of the positive and negative leaks at different temperatures in a vacuum.

(6) Variation of the leaks with the time at atmospheric pressure and in a vacuum.

(7) The nature of the carriers of the electricity discharged from the filament at very low pressures.

(1) *Discharge of Electricity from the Filament at Atmospheric Pressure; Variation of the Leak with the Potential.*

At atmospheric pressure the positive and negative leaks increase with the potential put on the filament. The results obtained at atmospheric pressure are given in Table I. During the whole course of the experiments several filaments were used. The number of the filament used in this series of

<div align="center">TABLE I.</div>

Filament I.		Negative Leak.		Positive Leak.	
Date 1904.	R. ohms.	Volts on Filament.	Current. $1 = 7\cdot5 \times 10^{-9}$ amp.	Volts on Filament.	Current. $1 = 7\cdot5 \times 10^{-9}$ amp.
Feb. 27 ...	100	0	13	+800	5
		−290	158	+600	$4 (= 3 \times 10^{-8})$
		−410	198	+400	2
		−575	$213 (= 1\cdot5 \times 10^{-6})$		
	60	0	200	+ 80	0
		− 40	600	+600	$14 (= 1\cdot05 \times 10^{-7})$
		− 80	1,250	+200	7
		−120	1,750		
		−200	2,350		
		−320	3,000		
		−600	$4,100 (= 3\cdot07 \times 10^{-5})$		
	30	0	800	+600	$66 (= 4\cdot9 \times 10^{-7})$
		− 80	5,800	+400	52
		−160	11,050	+200	32
		−240	15,500	+ 80	15
		−400	24,000		
		−600	$29,500 (= 2\cdot2 \times 10^{-4})$		
		+ 40	155		
	0	0	2,000	+800	$251 (= 1\cdot8 \times 10^{-6})$
		− 80	16,000	+600	181
		−160	39,000	+400	99
	Filament fully glowing.	−280	72,500	+200	70
		−400	99,000	+ 80	17
		−600	127,500		
		−800	$\{ 210,000$ $(= 1\cdot5 \times 10^{-3})$		
		+ 40	339		
March 1...	0	0	2,000	+600	60
		− 80	15,500	+400	45
		−195	46,500	+200	31
	Filament fully glowing.	−275	66,000	+120	23
		−390	85,000	+ 80	11
		−505	100,000		
		−580	112,500		
		−710	132,500		
		−790	150,000		

Diagram 1.

Leak at atmos. pressure. Feb: 27
30 ohms in series with filament

Negative leak. I=7.5×10⁻⁶ amp.

Positive leak

I=7.5×10⁻⁸ amp:

Current.

Volts.

Diagram 2.

Filament fully glowing at atmos. press. March 1st

negative leak   I=7.5×10⁻⁶ amperes

Positive leak I=7.5×10⁻⁹ amps.

Current

Volts.
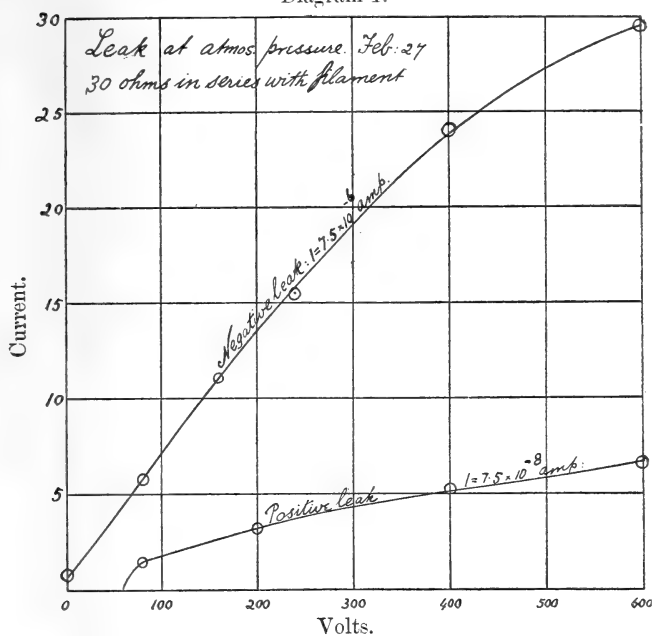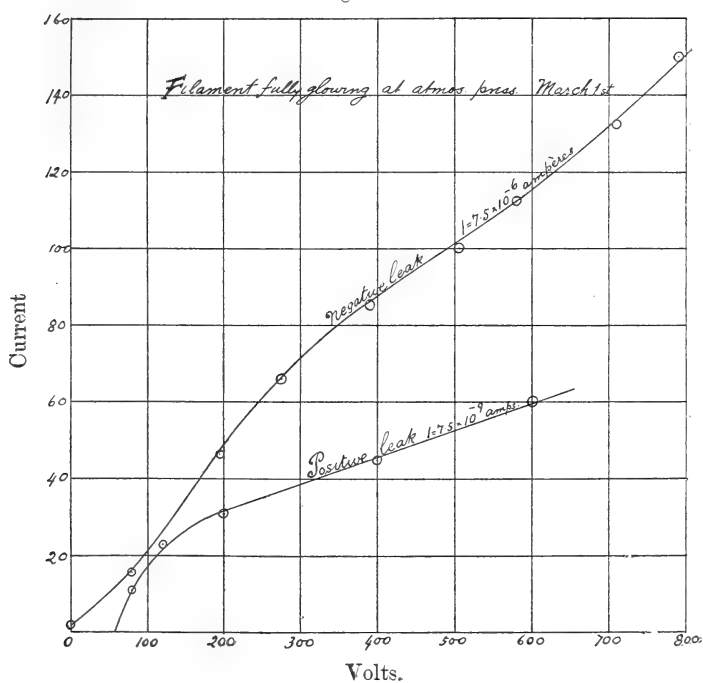
observations is given in the table. The second column gives the resistances inserted in series with the filament in addition to the 20-ohm iron wire resistance supplied with the lamp.

The results obtained for R=30 ohms, and for R=0 on March 1st, have been plotted and are shown in diagrams 1 and 2 respectively.

Table II. has been drawn up from Table I. and diagrams 1 and 2 to show how the ratio of the positive and negative leaks changes with increasing temperature.

TABLE II.

Potential on Filament= ±600 Volts.		Ratio of Negative to Positive Leak.
Date.	R ohms.	
Feb. 27	100	53
,, ...	60	286
,, ...	30	449
,, ...	Filament fully glowing.	833
Feb. 29	do.	1250
March 1	do.	2000

The third column shows that the negative leak increases with the temperature much more rapidly than the positive leak. The last three numbers in the same column point also to a rapid decay of the positive leak with continued heating. The latter point will be more fully discussed in section 6.

It will be noticed in Table I. that a *negative* leak is obtained with a *positive* potential of 40 volts on the filament. With the filament fully glowing at atmospheric pressure, it was found that +68 volts were just sufficient to stop the negative leak. A potential of +70 volts gave a positive leak. The reason for this is that there was an alternating potential-difference of about 70 volts between the ends of the filament, together with the fact that the negative leak was much greater than the positive leak.

(2) *Variation of the Negative Leak with the Potential at Different Pressures* ; *Ionisation by Collision.*

On March 9th a series of observations were made to determine the variation of the negative leak with the potential at pressures ranging from atmospheric pressure to a high vacuum. The temperature was maintained constant throughout the series by means of a thermocouple attached to the filament.

The results obtained are given in Table III. The numbers within the brackets in the first column refer to the order in which the observations were made.

TABLE III.

Filament I.   Temperature of Filament = 1430° C.

Pressure, mms. of Mercury.	Potential on Filament.	Current $1=7\cdot5\times10^{-9}$amp.	Pressure, mms. of Mercury.	Potential on Filament.	Current $1=7\cdot5\times10^{-9}$amp.
mm. 770	0	1	mm. 2·2	0	70
	−120	4		− 80	369
(16)	−200	10	(4)	−160	972
				−200	1687
79	0	2		−240	2713
	− 80	7			
(14)	−160	9	1·0	0	76
	−400	35		− 80	410
	−280	27	(5)	−160	896
				−240	1482
43	0	3		−280	2156
	− 80	13			
(12)	−160	19	0·7	0	82
	−240	35		− 80	380
	−320	70	(6)	−160	744
	−400	195		−240	1154
				−280	1388
34	0	7			
	− 80	20	0·32	0	87
(15)	−160	36		− 80	322
	−240	72	(7)	−160	509
	−320	170		−240	685
	−400	398		−280	773
				−320	890
26	0	7			
	− 80	22	0·16	0	70
(13)	−160	45		− 80	246
	−240	105	(8)	−160	339
	−320	322		−240	421
	−400	1084		−320	480
				−360	539
11	0	21		−400	615
	− 80	96			
(1)	−160	322	0·09	0	76
	−240	1142		− 80	216
	−280	2285	(9)	−160	281
				−240	316
4·8	0	41		−320	345
	− 80	228		−400	398
(2)	−160	726			
	−200	1429	0·035	0	70
	−240	2543		− 80	193
			(10)	−160	228
3·2	0	52		−240	246
	− 80	293		−400	252
(3)	−160	890			
	−200	1535	0·008	0	71
	−240	2472		− 80	176
			(11)	−160	216
				−240	226
				−400	266

At pressures of a few millimetres it was necessary to be very careful not to put a potential on the filament high enough to produce a luminous discharge. A delicate test for the presence or absence of a luminous discharge was afforded by the temperature of the filament. If a luminous discharge took place the temperature of the filament rose when the potential was put on. The constancy of the temperature of the filament was, therefore, a guarantee that no luminous discharge occurred.

Diagram 3.

The results tabulated above are represented diagrammatically in diagram 3.

The scale of the diagram only allows currents below 1600 divisions to be plotted.

A similar set of observations was made on March 14 with a new filament (II.) at a considerably higher temperature (1605° C.). The results of this set are given in Table IV. and are shown in diagrams 4 and 5.
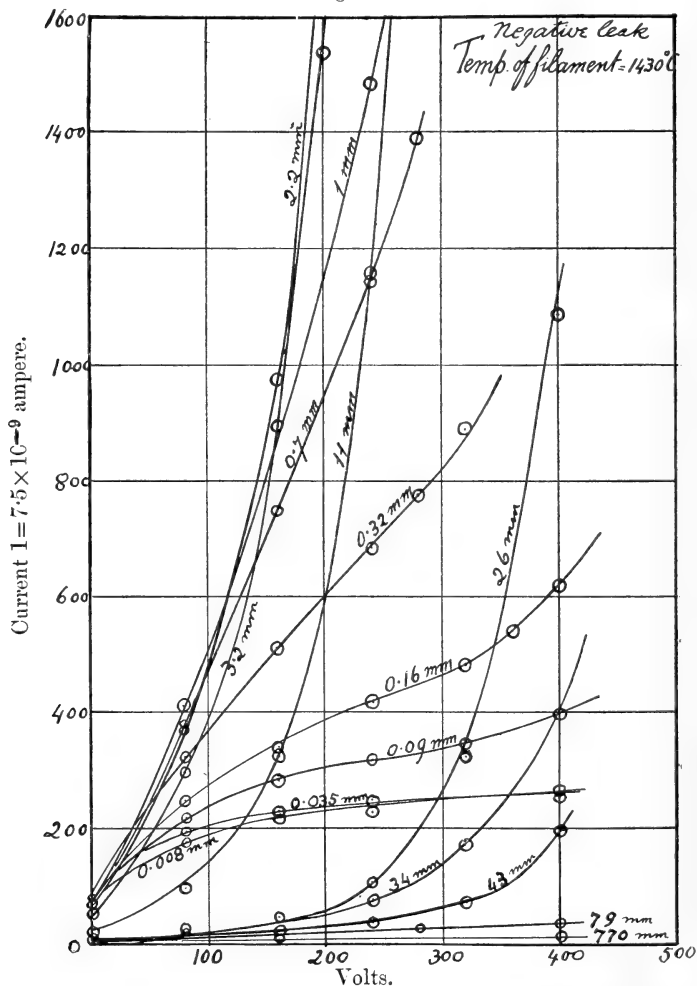
TABLE IV.

Filament II.   March 14th.   Temperature of filament 1605° C.

Pressure, mms. of mercury.	Potential on filament.	Current $1=7 \cdot 5 \times 10^{-9}$amp.	Pressure, mms. of mercury.	Potential on filament.	Current $1=7 \cdot 5 \times 10^{-9}$amp.
756	0	2 (positive)	2·7	0	5500 (negative)
	− 80	11 (negative)		− 80	30000
	−160	16 ,,		−160	57500
	−240	22 ,,		−200	77500
	−320	23 ,,		−240	107500
	−400	26 ,,			
190	0	14 (positive)	0·27	0	5000
	− 80	46 (negative)		− 80	17500
	−160	71 ,,		−160	27500
	−240	79 ,,		−240	32500
	−320	96 ,,			
	−400	131 ,,	1·2	0	5000
141	0	2 (positive)		− 80	23500
	− 80	140 (negative)		−160	45000
	−160	193		−240	67500
	−240	246			
	−320	304	0·42	0	4500
	−400	498		−40	14000
90	0	121		−80	22000
	− 80	473			
	−160	715	0·14	0	4500
	−240	990		− 40	10000
	−320	1665		− 80	14500
	−360	2365		−120	17750
54	0	250			
	− 80	800	0·06	0	4500
	−160	1300		− 40	9000
	−240	2500		− 80	12000
	−320	6000		−160	15750
	−360	10250			
32	0	550	0·032	0	4150
	− 80	1900		− 80	9750
	−160	4500		−160	12000
	−240	13250		−240	12750
	−280	48000		−400	13500
8	0	3000			
	− 80	25000			
	−160	60000			
	−200	80000			
	−240	102500			

From Tables III., IV., and diagrams 3, 4, 5 the curves given in diagram 6 have been drawn. These curves show

Diagram 4.
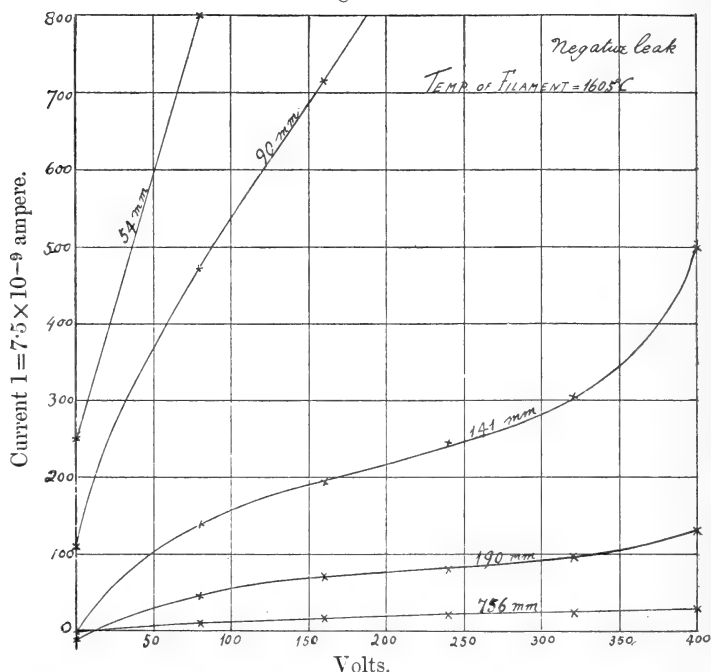
the variation of the current with the pressure under a potential of −200 volts. The values of the current at the higher temperature have been divided by 50 in order to make it possible to plot both curves on the same diagram.

Discussion of the Results represented by the Curves in Diagrams 3, 4, 5, 6.

It will be noticed from the curves that the leak under any given potential (say 200 volts) increases at first as the pressure is diminished. This, however, does not take place indefinitely, for at a pressure of 2·5 mm. at the lower temperature, and of about 5·2 mm. at the higher temperature, the leak attains a maximum value. The leak now diminishes with decreasing pressure until a pressure of about 0·035 mm. is reached. Exhausting still further produces but little diminution in the current. The leak was approximately saturated only at pressures greater than 200 mm., and less than about one-tenth of a millimetre. The curve at a pressure of 141 mm.

(diagram 4) and neighbouring curves show three distinct
stages. In the first stage the current increases rapidly with

Diagram 5.
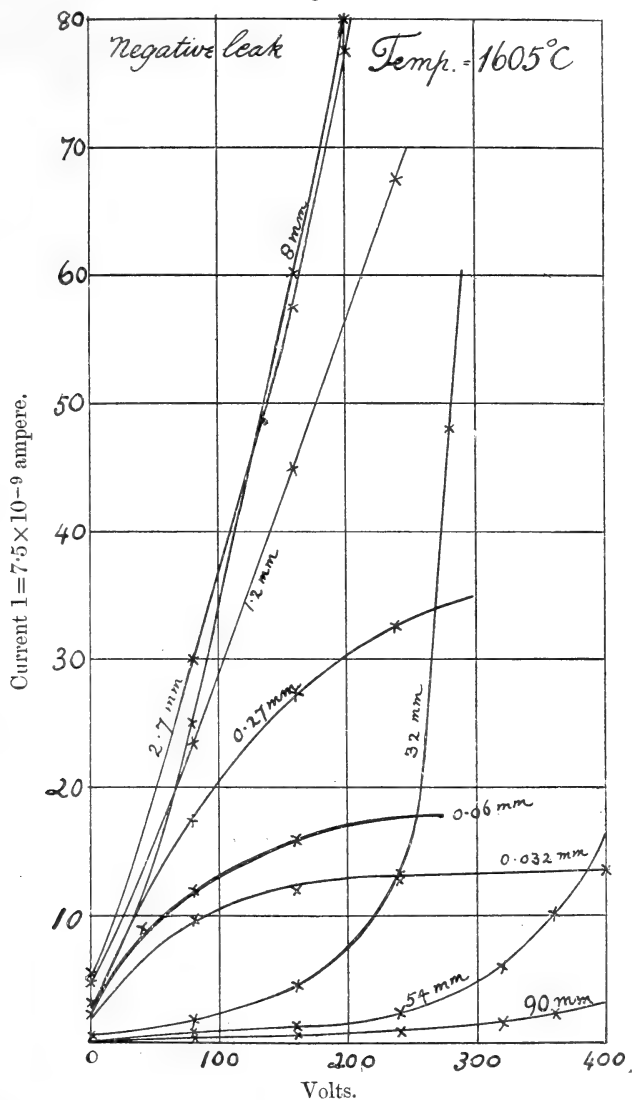
the potential. As the potential is increased further the rate
of increase of the leak diminishes. In the second stage the
current increases only very slowly for a considerable range
R 2

of potential.   The third stage is characterized by the current
suddenly beginning to increase very rapidly with the poten-
tial.   The first two stages follow directly from the ionic theory

Diagram 6.

Ionisation & Collision

B

A

$V = -200 \text{ volts}$ $Temp. = 1605° C$

$V = -200 \text{ volts}$ $Temp. = 1430° C$

Current.

Pressure in millimetres.
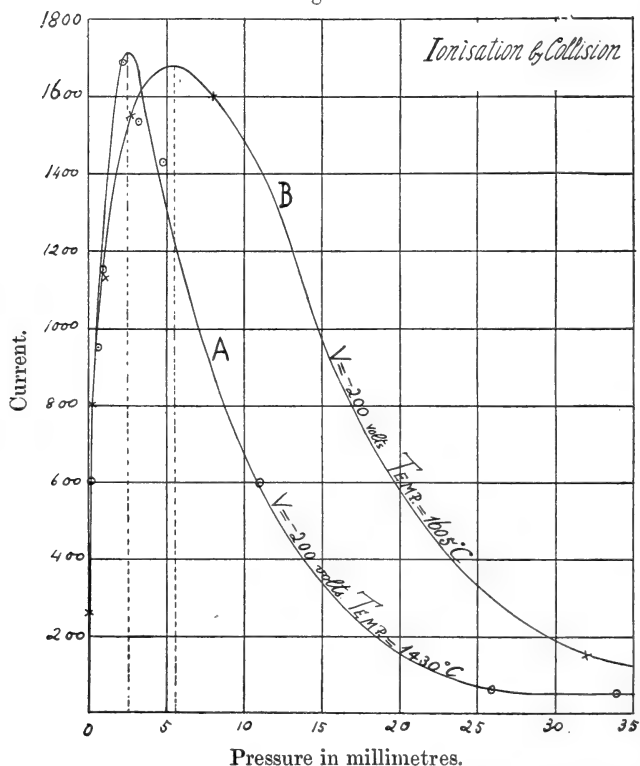
of conduction through gases; while the third stage can be
easily explained on the theory of *ionisation by collision**.
This theory assumes that when negative ions move through
a gas under the action of an electric force they can, under
certain conditions, produce fresh ions by colliding with the
molecules of the gas.   In order to ionise a molecule the ion
must possess a definite kinetic energy.   The kinetic energy
of an ion under a given electric force X is Xel, where e is
the charge on the ion and l its mean free path.   Now l is
inversely proportional to the pressure.   Therefore as we
diminish the pressure we increase the kinetic energy of the
ion.   When this energy exceeds a certain value fresh ions will
be produced by collision between the ions and the molecules

* J. J. Thomson, Proc. Camb. Phil. Soc. Feb. 5, 1900; "Conduction
of Electricity through Gases," pp. 228–234.

of the gas. The rapid increase of the current with the potential at pressures lying between 1 mm. and 200 millimetres is explained as being due to this cause. At a pressure of 141 mm. (diagram 4) ionisation by collision only began to any extent when the filament was charged to 350 volts. The secondary ionisation produced by collisions will not, however, increase indefinitely with diminishing pressure owing to a compensating effect coming into play. As the pressure is diminished the chance of a collision taking place is also lessened, and so less secondary ionisation is produced. This explains why the curves below a certain pressure in diagrams 3 and 5 gradually descend with diminishing pressure. In a vacuum, since no ionisation by collision occurs, perfect saturation curves are there obtained.

The mean free path at any pressure increases with the temperature of the gas. Hence we should expect the maximum effect of ionisation by collision to occur at greater pressures the higher the temperature of the filament. That this was found to be the case may be seen from the curves in diagram 6. Curves similar to those given in diagrams 3 and 5 have been obtained by Dr. H. A. Wilson[*] in the case of the negative discharge from platinum, and by Dr. W. Mansergh Varley[†] in his experiments on the photo-electric discharge from metallic surfaces.

It will be seen from Table III. that the current with no potential on the filament increases continuously with diminishing pressure until a pressure of about 2 mm. is reached. The current now remains nearly constant as the exhaustion is continued right down to a low vacuum. A similar result was obtained at 1605° C. with filament II., as may be seen from the following table. The readings are given in the order in which they were taken.

TABLE IV. A.

Filament II.　　March 14.　　Temperature = 1605° C.

Pressure, mms. of mercury.	Current with no potential on filament.	Pressure.	Current with no potential on filament.
756	2 positive	7·0	4750 negative
190	14 ,,	3·4	4600 ,,
141	2 ,,	1·6	4500 ,,
90	121 negative	0·42	4500 ,,
40	500 ,,	0·14	4500 ,,
29	750 ,,	0·06	4500 ,,
13	2800 ,,	0·032	4150 ,,

* Phil. Trans. A. vol. ccii. pp. 243–275 (1903).
† Ibid. pp. 439–458 (1903).

At this higher temperature the leak attains its steady value at a pressure of about 7 mm. The small positive leak at the three highest pressures is due to the fact that this filament had just been newly set up. A new filament discharges for a short time after it is set up a copious supply of positive electricity. This point will be discussed in Section 6. Dr. H. A. Wilson also found the negative leak from a platinum wire under a potential of 40 volts to fall off as the pressure increases. He suggested as a probable explanation that the presence of the gas hinders the escape of the negative corpuscles from the wire. The same explanation may be applied to the above in the case of the Nernst filament.

There is a further point of some interest in connexion with the leak at low pressures that may be shown from a consideration of the curves in diagram 3. The following table gives the leak under a potential of 350 volts at four consecutive pressures taken from that diagram.

<div align="center">TABLE V.</div>

Pressure.	Leak.
0·035 mm.	260
0·09 ,,	360
0·16 ,,	520
0·32 ,,	980

If these values of the pressure and the corresponding leaks be plotted, the curve passing through the four points is very nearly indeed a straight line. This shows that at low pressures for a considerable range the current is proportional to the pressure.

<div align="center">(3) <i>Variation of the Positive Leak with the Potential at Different Pressures.</i></div>

Table **VI.** gives the results of observations made at various dates on the variation of the positive leak with the potential at different pressures.

The currents on the 22nd and 25th of March were measured by means of an electrometer. That on March 8th, however, was measured by means of a galvanometer. As the deflexion

TABLE VI.

Date 1904.	Filament.	Pressure in mms. of mercury.	Temperature of filament or Resistance in series with it.	Potential, volts.	Positive Current, $1 = 10^{-8}$ amp.
March 8 .	I.	0·22	1430° C.	+400	1·5
March 22 .	II.	0·018	1540° C.	+ 40 + 80 +160 +240 +400	0·56 1·41 1·77 1·72 1·67
March 25 .	III.	764	100 ohms	0 + 80 +160 +240 +400	(negative leak) 2·8 4·9 5·3 6·4
		67	150 ohms	0 + 80 +160 +240 +400	(negative leak) 9·5 10·1 9·4 10·3
		32	150 ohms	0 + 80 +160 +320	(negative leak) 5·7 7·3 7·0

of the galvanometer was only about 2 divisions, too much importance must not be attached to the value $1·5 \times 10^{-8}$ ampere given to the current in the table. In measuring this leak on March 8th it was noticed that after the potential of +400 volts had been on for a few seconds, the discharge suddenly changed from a steady current of 2 divisions to a luminous discharge. This result, combined with the other observations given in Table VI., shows that the positive leak can be saturated at all pressures provided no luminous discharge takes place. The positive ion, therefore, does not acquire so readily as the negative ion the kinetic energy necessary to ionise a molecule.

(4) *Variation of the Negative Leak with the Temperature in a Vacuum.*

Table VII. shows the way in which the saturation current from the filament charged to a negative potential of 160 volts varied with the temperature. The currents were measured by means of a galvanometer.

TABLE VII.

February 23.   Filament I.   Potential on Filament
$= -160$ volts.

Pressure, millimetres.	Temperature, ° C.	Saturation Current. 1 division$=7{\cdot}5\times10^{-9}$ amp.
0·006	1120	10
0·006	1150	18
0·005	1182	42
0·005	1205	52
0·006	1260	131
0·008	1296	197
0·008	1330	322
0·009	1353	480
0·009	1385	850
0·009	1399	1150
0·013	1432	3450
0·012	1456	5950

The above results are shown in diagram 7.

Diagram 7.

Saturation Current.

Temperatures Centigrade.
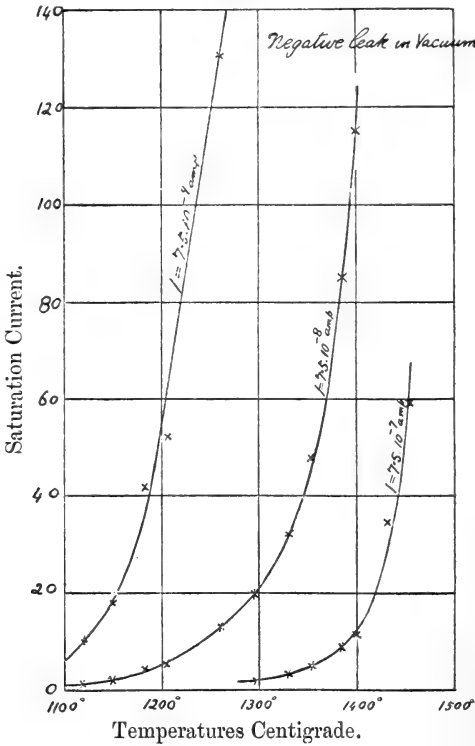
It will be seen from the curves that the negative leak in a vacuum increases very rapidly with the temperature. Precisely similar curves have been obtained by O. W. Richardson * representing the variation with the temperature of the saturation negative leak at low pressures from platinum and carbon filaments.

(5) (a) *Variation of the positive leak with the temperature in a vacuum.*

(b) *Relative magnitudes of the positive and negative leaks at different temperatures in a vacuum.*

(a) In these experiments on the positive leak the currents were measured by means of an electrometer, the currents being usually too small to be appreciable on a galvanometer.

Table VIII. shows the variation of the positive leak with the temperature in a vacuum.

TABLE VIII

March 22. Filament II. Potential on filament = + 200 volts.

Pressure, millimetres.	Temperature, ° C.	Saturation Current. $1 = 10^{-10}$ amperes.
0·013	1250	4·8
	1285	11
0·007	1305	15
	1357	21
0·006	1400	33
	1445	55
	1475	71
	1520	123
0·009	1550	158
	1615	267

The results given in Table VIII. are shown diagrammatically in curve A, diagram 8.

It is seen from this curve that the positive leak in a vacuum increases rapidly with the temperature. Comparing this curve, however, with those in diagram 7, we notice at once that the rate of increase of the positive leak with the temperature is much smaller than that of the negative leak.

(b) The absolute values of the positive and negative leaks at the various temperatures given in Tables VII. and VIII. may not be compared, for the filament and the thermocouple were not the same in the two cases. In order to see how the ratio of the negative to the positive leak varied with the

* Phil. Trans. A. vol. cci. pp. 497–549 (1903).

temperature, a new series of observations was made in which the negative and positive leaks were measured, the latter

Diagram 8.

Temperatures Centigrade.
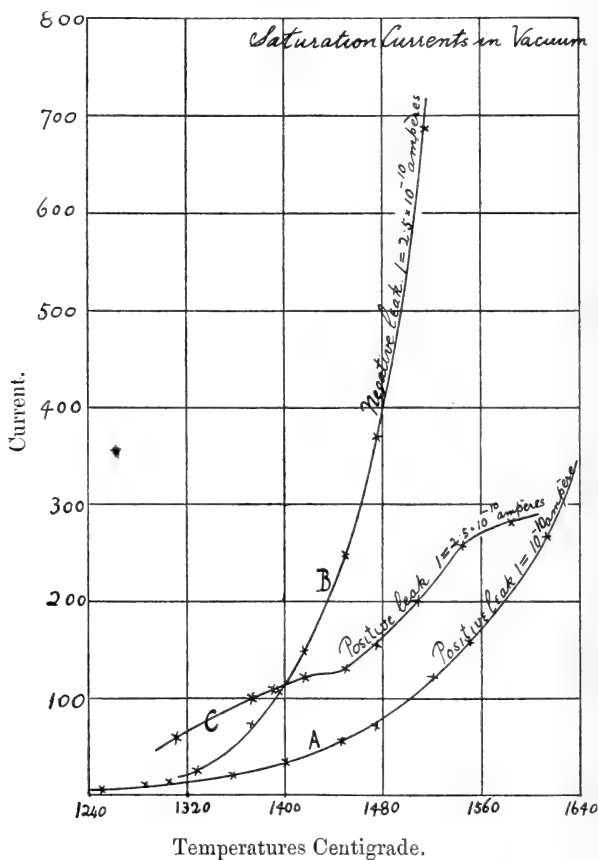
directly after the former, at various temperatures. The results are given in Table IX. in the exact order in which they were taken.

Curves B and C in diagram 8 have been obtained from the numbers given in Table IX. From these curves it will be seen that below 1400° C. the positive leak is larger than the negative leak. Above this temperature the negative leak is the bigger, and the ratio of the negative leak to the positive increases rapidly with the temperature. At the highest

TABLE IX.

Filament IV.   April 22nd.

Pressure.	Potential.	Temperature.	Current. $1 = 2.5 \times 10^{-11}$ amp.
mm.	volts.	° C.	
0·008	−200	1327	261
	+200	1310	600
0·009	−200	1372	732
	+200	1372	1008
......	−200	1395	1088
	+200	1390	1098
0·012	−200	1415	1494
	+200	1415	1212
0·010	−200	1450	2480
	+200	1450	1304
0·0097	−200	1475	3680
	+200	1475	1560
0·0097	−200	1515	6840
	+200	1510	2016
0·011	−200	1540	11760
	+200	1545	2592
......	−200	1585	44000
	+200	1585	2800

temperature tried (1585° C.) the negative leak was nearly 16 times the positive.

It will be noticed that there is a considerable difference between the shapes of the curves A and C.

This is explained in the next section, as the said difference is a direct consequence of the effect described in that section, namely, the variation of the leak with the time.

(6) *Variation of the Leak with the Time.*

When a new filament is set up and made to glow at atmospheric pressure without being charged to any definite potential, it will at first discharge positive electricity. This positive leak diminishes, however, rapidly with the time, and soon vanishes. The leak with no potential now becomes negative, and remains so at all pressures. This is well shown by the *first* observations made with filament III. The observations, which were taken on March 25, are given in Table X.

Table X.

Pressure = 764 mms.   100 ohms in series with the filament.

Time in minutes (approx.).	Leak.
0	+55
15	+13
35	−30

The initial positive leak with no potential on the filament does not come back on allowing the filament to remain in the tube for some days. Positive leaks may, however, always be obtained by charging up the wire to potentials greater than about + 70 volts. The " steady " positive leak thus obtained also decreases slowly with long-continued heating of the filament. The negative leak, on the contrary, diminishes but very little with the time. From the following Table (**XI.**) the rates of the decrease of the positive and negative leaks may be compared. The leaks were measured at the commencement of sets of observations taken on each of the dates given.

Table XI.

Filament I. fully glowing, at atmospheric pressure.

Date, 1904.	Positive Leak under +600 volts.	Negative Leak under −600 volts.
February 27	136×10^{-8}	95×10^{-5}
February 29	75×10^{-8}	92×10^{-5}
March 1	45×10^{-8}	90×10^{-5}

Between Feb. 27th and March 1st the positive leak has thus diminished 67 per cent. In the same interval the negative leak has only diminished about 5 per cent. It is of course to be understood that the initial positive leak with no potential on the filament had been once and for all destroyed by continuous heating on dates previous to Feb. 27th.

The positive leak decreases also with the time in a vacuum, as may be seen from Table **VI.**, where the variation of the

positive leak with the potential at 0·018 mm. pressure is given. We are now in a position to explain the difference in the shapes of the curves A and C (diagram 8), representing the variation of the positive leak with the temperature in a vacuum. Curve A was obtained with filament II., with which long sets of observations extending over several days had been previously made. For this filament, therefore, the positive leak would diminish only very slowly with the time. The filament for which curve C was obtained was, on the contrary, a new filament, and had never been kept glowing for a long time at atmospheric pressure. The rate of diminution of the positive leak with the time for the latter filament would accordingly be much greater than in the former case.

Some observations made on March 22nd with filament II. suggest that the positive leak diminishes much more rapidly with the time when the filament is kept positively charged than when uncharged. Further, charging the filament negatively was found to stop the diminution of the positive leak with the time. The observations indicating the existence of this effect are given in Table XII. The filament was kept at a constant temperature throughout.

TABLE XII.

Pressure.	Temperature of Filament.	Time (approx.).	Potential on Filament.	Potential on Filament during the interval between observations.	Positive Leak.
mm. 0·018	° C. 1540	h m 7 0	+400	+400	168
		7 10	+400		121
		7 15	+400	0	145
		7 20	+400	−400	169

This effect may be due to some sort of polarization of the filament. The author hopes to make at an early date some further experiments on this point, and also to see if the positive leak can be completely destroyed by keeping the filament glowing for a very long time.

(7) *Nature of the Carriers of the Electricity discharged from the Filament at very Low Pressures.*

Information on this point has been obtained by finding the value of $\frac{e}{m}$ for the carriers of the electricity. The method adopted for this determination was that used by Prof. J. J. Thomson * for the ions discharged from a carbon filament. The principle of the method is to subject the ions to the joint action of electric and magnetic forces, the directions of the forces being at right angles to one another. It is shown in the paper referred to that the value of e/m is given by the expression $\frac{2V}{H^2 d^2}$, where d is the distance from the filament to the electrode, H the magnetic force, and V the largest potential-difference between the filament and electrode under which any diminution of the leak due to the magnetic field can be detected. This is obviously the same as finding the smallest magnetic force which produces a diminution in the leak under a given potential-difference V. The latter method was used in the experiments described in this section.

In order to swamp the alternations of potential on the filament, the latter was charged to very high voltages. The leak without any magnetic field was first noted. Magnetic fields of various magnitudes were then put on by sending currents through an electromagnet, between the poles of which the filament was placed, and finally the leak without the magnetic field was once more observed.

The results obtained on March 5th for the negative leak in a vacuum are given in Table XIII. The filament was midway between the platinum disks which were 7 mms. apart.

It will be noticed that weak magnetic fields *increase* the leak slightly. The same effect was noticed by Prof. J. J. Thomson in his experiments on the magnetic deflexion of the corpuscles from a carbon filament. Theoretically, if the carriers are particles of a uniform mass starting at the same distance from the electrodes, then a magnetic field smaller than a certain value will produce no diminution in the leak, while a magnetic field greater than this critical value will stop the leak entirely. This, however, is never realized in practice ; but the transition from no effect to a reduction of the leak by 80 per cent. is very fairly abrupt, as may be seen from the curves in diagram 9 (p. 256), which represent the results given in Table XIII.

* Phil. Mag. xlviii. pp. 547–567 (1899).

TABLE XIII.

1 ampere through magnet = 59 lines per sq. cm.

Pressure.	Potential.	Resistance in Series with Filament.	Current, Magnet off, $1 = 7\cdot5 \times 10^{-9}$.	Current through Magnet.	Current, Magnet on.	Percentage stopped.	$\dfrac{e}{m}$.
mm. 0·005	−400	16 c.p. lamp + 80 ohms (A)	(131 Mean = 132 133	amperes. 5·3 6·2 7·2 9·6 15·5	141 88 48 11 3	...... 33 % 61 92 98	6·0 × 10⁶
0·005	−600	16 c.p. lamp + 80 ohms (C)	(144 Mean = 142·5 141	7·2 8·2 11·4 15·5	147 83 17 7	...... 42 % 88 95	5·0 × 10⁶
0·007	−400	16 c.p. lamp. (A′)	(533 Mean = 518 503	5·2 5·9 6·4 8·2 15·5	580 451 334 93 17	...... 14 % 36 82 97	6·0 × 10⁶
0·010	−400	119 ohms (B)	(6200 Mean = 5975 5750	5·8 6·5 8·2 11·5 15·5	5950 4700 1500 350 150	0·5 % 21 75 94 98	5·6 × 10⁶

Mean value of $\dfrac{e}{m} = 5\cdot65 \times 10^6$

The points where the curves cut the axis of the current give the values of H taken to calculate e/m. The two sets of observations (A) and (A′) lie practically on the one curve A. The other curve B for a potential of −400 volts does not quite coincide with the curve A. This is probably due to the current dealt with in set (B) being very much larger than those in the sets (A) and (A′). The mean value of e/m found, namely $5\cdot65 \times 10^6$, agrees sufficiently closely with the values obtained by Prof. J. J. Thomson for the negative ions discharged from a plate illuminated by ultra-violet light and from a carbon filament ($7\cdot3 \times 10^6$ and $8\cdot7 \times 10^6$

respectively) to show that the carriers of the negative electricity from a Nernst filament at low pressures are the " *corpuscles*."

Diagram 9.

Current through Magnet.   1 ampere=59 lines / sq. cm.
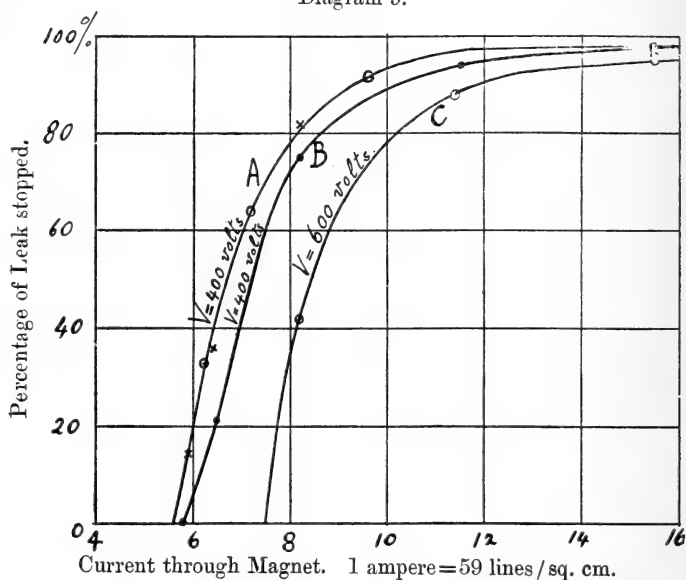
As is seen from the curves, the effect of the magnetic field approximates more and more to a saturation value as the magnetic field is increased. This is probably due to the ionisation not being confined to the gas in contact with the filament. Even at these low pressures there will be a certain amount of ionisation by collision; and so a few ions will start towards the electrodes from a distance much smaller than 3·5 mms. These ions will therefore require much stronger magnetic fields to deflect them before they reach the electrodes. A curve showing this saturation effect would also be obtained if a small percentage of the corpuscles were shot out of the filament with a considerable velocity.

With a potential of −200 volts on the filament 99·7 per cent. of the negative leak was stopped by a magnetic field of a little over 800 lines per sq. cm. There is thus every reason for believing that the carriers of the negative electricity from a Nernst filament at low pressures are *all* corpuscles without any admixture of heavier particles.

On subjecting the *positive* leak to a magnetic field not the slightest diminution in the current could be produced by the

strongest fields at my disposal (900 lines per sq. cm.), the filament being charged to + 200 volts.

Thus the carriers of the positive electricity discharged from a Nernst filament at low pressures are particles much heavier than corpuscles. In all probability they are bodies of molecular dimensions.

In connexion with the magnetic deflexion of the negative leak from the filament, it remains to add that I have found it of the utmost importance to surround the filament and electrodes by an earth-connected metal tube. If this be not done, the walls of the glass tube get charged up, and, when dealing with large currents, this may prevent the magnetic field from producing any diminution in the leak.

The author hopes at an early date to make some experiments on the discharge from a Nernst filament in different gases. It will be interesting to see if an increase in the leak takes place when hydrogen is substituted for air, corresponding to what happens in the case of platinum.

Summary of the principal results.

(1) From a glowing Nernst filament there is at all pressures a discharge both of positive and negative electricity.

(2) The negative leak behaves in a perfectly regular way, diminishing only slowly with the time, and is not liable to those sudden variations which occur in the case of the negative leak from platinum. During the whole course of the experiments the negative leak did not suddenly change without any apparent cause more than three or four times.

(3) The negative leak at certain pressures shows the effect of ionization by collision.

(4) The negative leak when no ionisation by collision occurs increases as the pressure is diminished until a pressure of a few millimetres is reached. It then remains constant down to a low vacuum.

(5) The negative leak increases rapidly with the temperature of the filament.

(6) At low pressures for a considerable range the negative leak under a high potential is proportional to the pressure.

(7) From a filament which has not been heated before, there is a large initial positive leak which rapidly dies away. After long continued heating of the filament the positive leak becomes fairly steady, but still continues to diminish with the time.

(8) The positive leak increases with the temperature of the filament, but not so rapidly as the negative leak.

(9) The carriers of the negative leak in a vacuum are

corpuscles, while the carriers of the positive leak are particles of molecular dimensions.

These experiments were carried out at the Cavendish Laboratory during the earlier months of this year, and, in conclusion, I wish to express my sincere thanks to Professor Thomson for his kindly interest and for many valuable suggestions while the experiments were in progress.

Christ's College, May 10th, 1904.

XXVII. *The Hard and Soft States in Metals.*
By G. T. Beilby *.

[Plates V.–IX.]

Introductory.

1. THERE does not hitherto appear to have been any serious attempt to arrive at a single guiding principle which will harmonize the many isolated phenomena of the solid state which are met with in the practice of the metalworking arts, and in the systematic testing of materials in physical and engineering laboratories.

2. The hardening and re-softening of metals in annealing, forging, rolling, wire-drawing, and stamping, as these are practised in the arts; the flow of metals dealt with by Tresca and by Spring; viscosity and elastic fatigue in metals and other solids, as observed by Kelvin; mechanical fatigue and disruption in metals exposed to frequent alterations of stress, investigated by Wohler, Baker, Bauschinger, Unwin, and others; the deformation of crystalline grains and their restoration and growth by heat-annealing, studied by Ewing and Rosenhain; and the character of distortion and rupture of iron and soft steel, recently discussed by Osmond, Frémont, and Cartaud—make a sufficiently varied, though far from exhaustive list of subjects, which, in spite of much special study, still remain in comparative isolation from each other.

3. Looking broadly over this wide range of subjects, it is seen that two complementary phenomena stand out prominently, round which the others may be naturally grouped. (1) The deformation of a metal by stresses beyond the elastic limit invariably tends to alter the character of the metal in one definite direction—that is, towards a diminished capacity for being deformed by such stresses. (2) The plastic quality can be restored to the deformed metal by raising it to a suitable temperature.

4. In reviewing the researches on the subject of hardening and annealing, it is found that, with very few exceptions, they have had special reference to these phenomena as they occur in iron and steel. This has naturally resulted from the great importance of these materials in engineering and construction, but there can be no doubt that from the point of view of a broad study of the subject, this choice of the material for investigation has to some extent been unfortunate, inasmuch as the properties of iron and its alloys are very much more complicated than those of any other metal in ordinary use. In a steel containing a minute percentage of carbon it is possible to identify five or six primary constituents whose appearance depends almost entirely on the heat treatment to which the specimen has been subjected. In addition to this, it is believed that iron itself can appear in two or three allotropic modifications, each of which has its definite transformation temperature. The student of this subject is therefore confronted with a set of complicated problems, the elements of which it has been all but impossible to isolate from each other.

5. For the study of the phenomena which are to be here discussed, it has appeared to me desirable to detach these phenomena as far as possible from complications of the kind above referred to. With this object, while the metals and other substances examined have been of many widely different types, the ultimate reference has always been made to those in which the behaviour is simple as well as characteristic.

6. At an early stage in this study I was led to the conclusion that nothing was to be gained by attempting to stretch our ideas of the crystalline state so that room might be found within that state for all the varied phenomena presented in the mechanical and thermal treatment of metals. A crystalline aggregate must owe its origin to some inherent quality of its molecules in virtue of which *they take up and retain fixed positions with reference to each other*. The molecular theory of magnetism supplies an illustration of a simple form of polarity in which the molecules are arranged end to end to form strings or rows. The compounds of the asymmetric carbon atom supply a more complete illustration of the direct influence of the molecular polarity on the form of the crystal; for here the definite space relations of the molecule find their counterpart in the geometrical form of the crystal. *The strictness of these molecular and geometrical relations must be regarded as a characteristic feature of the crystalline state.*

7. Up to its limit of elasticity a crystal may be strained without its ceasing to be a crystal; but when this limit is

passed permanent deformation is produced, some of the molecules being moved into new positions relatively to each other, positions which are not in strict accord with the geometrical molecular arrangement.

8. But in considering the transition from the liquid to the solid state we are not necessarily limited to the crystalline form of aggregation. In a former paper I have shown that the forms assumed by the surface layer of a solid metal during annealing are controlled by surface tension rather than by crystallic force*. In this case the ordinary forces of cohesion determine the positions taken up by the molecules relatively to each other. Throughout a layer, which is probably many molecules in thickness, the molecules have come to rest in the midst of the activity of the liquid state, their mutual relations in that state being stereotyped at the moment of cooling.

9. The passage from the liquid to the solid state consists essentially in the arrest or the limitation of the freedom of movement of the molecules relatively to each other. This freedom of movement is determined by the temperature of the substance. When the temperature is reduced by the withdrawal of heat, the molecules must come to rest in the solid state. If the arrest is instantaneous, then the molecules will have no opportunity to arrange themselves according to their polarity and the solid state which results will be a heterogeneous assemblage of molecules which will bear somewhat the same relation to the homogeneous assemblage of a crystal as a crowd of disbanded soldiers does to the same men in the orderly formation of the company or the battalion.

10. A solid of this type may be likened to an instantaneous photograph of the molecules in the liquid state ; but while in the photograph there is only the appearance of fixity, in the congealed liquid the fixity is real.

11. In addition to the inherent polarity of the molecules, crystalline formation requires time, freedom, and space. Time and freedom are necessary to enable the molecules to turn into the proper orientation ; and space may be required if the packing of the molecules in the homogeneous assemblage is more open than it is in the heterogeneous.

12. The rigidity of the solid state is therefore due to the arrest of the molecular movement either in a homogeneous or in a heterogeneous assemblage of molecules. Throughout this paper the term crystalline is applied to the former and amorphous to the latter.

13. We can picture the transition from the liquid to the amorphous solid state as taking place either by a sudden

* Proc. Roy. Soc. vol. lxxii. p. 227.

arrest of molecular movement or gradually through the increasing viscosity of the liquid phase.

14. The stability of an amorphous solid, depending as it does on the curtailment of the free movements of the molecules, is obviously determined by temperature. If that freedom is sufficiently restored by heat to permit the molecules to fall into orderly formation under the direction of polarity, the amorphous state will pass over into the crystalline. That this degree of freedom is obtained at a definite transition-temperature very much below the liquefying-point is abundantly shown by the observations which are presented in this paper. The stability of the crystalline phase persists from the transition-temperature up to the liquefying-point, at which point the movement of the molecules can no longer be restrained even under the influence of their cumulative polarity.

15. The conclusions brought forward in this paper were directly led up to by a study of the micro-structure of metal and other surfaces, some of the results of which have already been communicated to the Royal Society *. In particular the discovery of surface-flow during the polishing of crystalline solids appeared to open up the possibility of a new theory of the hardening of metals by plastic flow †. In following up this matter a number of experimental observations have been made, some of which will be referred to in what follows.

Argument.

16. Metals ordinarily occur in two distinct solid phases : the hardened or amorphous, which will be referred to as the A phase, and the annealed or crystalline, which will be referred to as the C phase. The A phase is transformed into the C phase by the agency of heat, the C phase is transformed into the A phase by mechanically-produced flow. In the transformations A $\rightleftharpoons$ C there are two intermediate mobile phases M and M′; so that the transformations may be written A $\longrightarrow$ M′$\longrightarrow$ C and C $\longrightarrow$ M $\longrightarrow$ A. The argument is based on evidence drawn from :

1. The distinct mechanical properties of the two phases A and C.
2. The micro-structure of these and the evidence which it supplies of the existence of the mobile phases M′ and M.
3. The optical properties of the phases A and C.
4. The electrical properties of the phases A and C.
5. The thermochemical properties of the phases A and C.

* Proc. Roy. Soc. vol. lxxii. pp. 218–225.
† Hurter Lecture, Liverpool, 1903. Soc. Ch. Ind. J. pp. 1166–1177.

Summary of the Evidence.

17. The better to preserve the continuity of the argument, the evidence is briefly summarized at this stage, certain detailed observations being given more fully at the end of the paper.

18. The malleable and ductile metals—gold, silver, platinum, copper, and lead—are all well adapted for the study of the phenomena of hardening and softening. Iron and nickel, while they possess the common characteristics in a very marked degree, have other properties which occasionally render their behaviour less plain and simple.

19. The brittle crystalline metals, antimony and bismuth, show the phenomena of flow in a peculiarly clear way; and their behaviour therefore falls perfectly in line with that of the ductile metals. It is therefore of special value in showing *how flow may take place altogether apart from malleability or ductility.*

20. Among the ductile metals silver occupies a fairly central position in respect to hardness and tensile strength, while its optical, electrical, and thermochemical characters are particularly well marked. I have therefore selected this metal as the most suitable with which to illustrate a summary of the evidence in support of my argument. A fuller description of some of the experiments and observations is given at the end of the paper, and for convenient reference to this portion the paragraphs have been numbered throughout.

The Mechanical Properties of the two Phases in Silver.

21. When work is done on silver by rolling, hammering, or wire-drawing, its hardness and tenacity are raised to a very high point. In this way the tensile strength of soft silver may be raised from under 10 tons per square inch to over 20 tons.

22. A strip of silver-foil may be made so hard and springy by hammering, that it might be used as a vibrating tongue of a reed instrument. After heating to 260° the hardened strip loses all its spring, and becomes so soft that it may be pressed perfectly flat on a smooth surface by the slightest pressure from the finger. (See par. 64.)

23. The contrast between the hard and soft phases in silver is greater than that among the different ductile metals when these are in the soft phase. Silver in the A phase is much more akin to copper in the same phase than it is to silver in the C phase.

The Micro-structure.

24. The micro-structure of annealed silver, if the metal is in a sufficiently massive form, is always crystalline; and consists of skeletons or grains built up of lamellæ of similarly

oriented units. In attenuated forms like leaves or thin films the structure is determined by surface-tension, and has none of the characteristics of crystalline aggregation *.

25. The micro-structure of hardened silver is vitreous looking on the surface, and finely granular immediately below the surface. The forms assumed by surfaces and edges are rounded and smooth, and suggest the flow of a viscous liquid. When the glassy surface is carefully removed by a solvent, the granular structure underneath is more fully disclosed. By further action of the solvent this granular layer may be completely removed, disclosing the crystalline grains or skeletons of the C phase more or less deformed or broken up.

26. It seems probable that the granules which are thickly distributed through the vitreous layer are produced by the breaking down of the lamellæ and the setting free of the units of which they are built up. The granules and their vitreous matrix always appear at surfaces of flow, the thickness of the layer being determined by the thoroughness of the flow at that particular place. The persistence of the units of the C phase in the granules may be accounted for by their being encased in an envelope of the hardened A phase.

27. Within a mass of metal the flow never reaches the same degree of completeness as it does at or near the outer surfaces, the relative amount of the hardened phase which is produced may therefore be smaller ; but it will be found encasing units, lamellæ, or grains at all surfaces at which slipping has occurred, cementing them together, and producing a conglomerate in

Fig. 1.
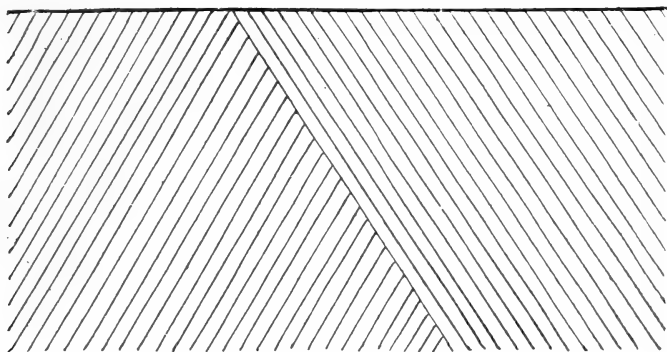
which the hardness is much greater than that of the annealed phase, but less than that of the completely transformed A phase.

28. Figs. 1 and 2 are diagrammatic sketches in which I

* Proc. Roy. Soc. vol. lxxii. p. 232.

have attempted to indicate on a very large scale the steps in the hardening process from the surface inwards. Fig. 1 is a vertical section across a surface at which two crystals of different orientation meet. Fig. 2 shows the same after the surface has been flowed to a considerable depth.

Fig. 2.
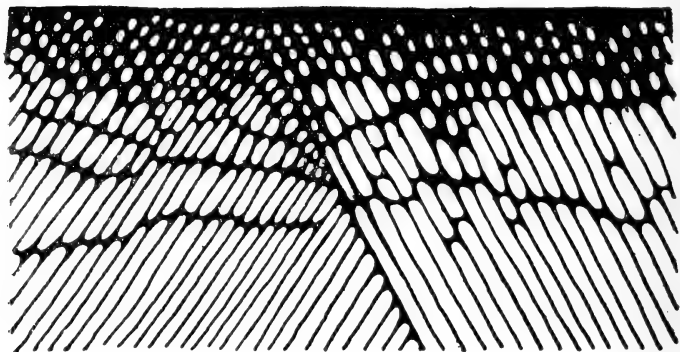
Photo-micrographs illustrating surface-flow in gold, silver, copper, lead, antimony, &c., have already been published; but some of these are reproduced here for convenient reference (figs 9–20).

Figs. 3–8 show the vitreous surface, the granular under surface, and the distorted grains in a specimen annealed, beaten, and then slightly etched in two stages. The evidence afforded by the micro-structure is further discussed in pars. 39, 40, 49, 50, 61, 68, and 71.

Optical Properties of Hardened and Annealed Silver.

29. Even in its moderately attenuated forms, such as beaten leaves and films deposited by chemical means on glass or mica, silver in its hardened state is highly reflecting and only slightly translucent. By heating to from 250° to 300° the leaf or film becomes transparent, and loses much of its reflecting quality.

By flowing or burnishing the annealed leaf or film its opacity and reflecting-power are restored.

These remarkable effects were first observed by Faraday, who concluded that in the two states the metal was in two perfectly distinct forms *. My own observations have confirmed and extended the results obtained by Faraday †.

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* Faraday's ' Researches in Chemistry and Physics,' pp. 398–399.
† Proc. Roy. Soc. vol. lxxii. pp. 226–227.

30. The change of colour and reflecting-power in gold is quite as remarkable as in silver ; and my observations on the changes produced on the surface-layers of these and other metals in the more massive forms serve to connect the phenomena in the attenuated and the massive metal with the same causes.

31. In gold-leaf the state of strain which is left after flowing is always associated with the green translucence and the yellow metallic reflexion of burnished gold.

Electrical Properties.

32. The electrical conductivity of a silver wire in the annealed state is 8 to 10 per cent. higher than that of the same wire in the hardened state. With other metals also the annealed state has the higher conductivity [*].

33. Mirror-silver, in which the metal is in the hardened and strained state, shows an initial potential difference of 0·1 volt against ordinary silver in dilute acids [†]. Lüdtke, who investigated this subject, regarded mirror-silver as an allotropic and colloidal modification of silver.

34. My own observations [‡] have shown that a thermojunction consisting of a hardened and an annealed wire of silver has an E.M.F. of 0·17 micro-volt for 1° C. of difference between the hot and cold junctions. This is a much higher E.M.F. than I have found recorded by other observers (see par. 64). At 260° the E.M.F. falls to zero, as the hardened wire then passes into the soft condition. Copper, iron, platinum, magnesium, all show a measurable E.M.F. between the two states (see par. 64). In each of the above cases there is a well-marked difference between the two states, but the loss of conductivity through hardening seems to me peculiarly significant.

Thermochemical Properties.

35. In etching hardened or polished silver it is found that solvents act first on the hardened phase, removing it and leaving the crystalline phase exposed.

If the hardened metal is merely a slightly compacted form of the annealed, it would naturally be expected that the more open form would be the more readily attacked and dissolved.

On thermochemical grounds Berthelot [§] regarded hammered, crystalline and annealed silver, as well as certain other forms,

[*] 'Practical Electricity,' Ayrton, p. 157.
[†] Lüdtke, *Ann. Phys. Chem.* (2) vol. l. pp. 678–695.
[‡] 'Nature,' vol. lxx. p. 31.
[§] Berthelot, *Compt. Rend.* vol. cxxxii. pp. 234–241 (1901).

as allotropic modifications.   He found that the heat of solution
in mercury of hammered silver was $+2\cdot03$ cal., as compared
with $+0\cdot47$ cal. for silver heated to $550°$, and $+0\cdot10$ cal. for
crystals of electrolytic silver.   My observations show that
$550°$ is an unnecessarily high temperature for annealing silver.
As shown by the thermo-junction (par. 34), the transition
begins about $250°$, and would probably complete itself at
that temperature if sufficient time were allowed.

36. This completes the summary of the evidence in sup-
port of the view that the hardened and the annealed are distinct
forms of metal.   By collecting the hitherto scattered obser-
vations on the mechanical, optical, electrical, and thermo-
chemical properties of the hard and soft forms, it is seen that
these all, without exception, support the view which I first
advanced nearly four years ago, that the micro-structure
shows that there is a well-marked difference between the two
states, the crystalline, and the amorphous or colloidal.

37. The evidence from these various sources also shows
how naturally the varied properties group themselves on
either side of a transition-temperature which is common to
all ; we are therefore justified in regarding these forms of a
metal as two distinct phases.

A Phase Theory of Hardening and Softening.

38. With the recognition of these distinct phases the
softening of metals now for the first time takes its place
among other phase transformations ; there are, however,
certain special features which are peculiar to this trans-
formation.   So far as is yet known, the transformation
from hard to soft is thermally irreversible ; that is to say,
no mere reduction of temperature below the transition-point
is sufficient to bring about the reverse change from soft
to hard.   In the well-known case of the two forms of tin, the
white and the grey, it may be found that the latter is amor-
phous, not crystalline ; but at present there is no evidence
on this point ; and the fact that the white variety expands
considerably in passing to the grey seems to place the
transformation in a different class from that now being
discussed.

39. Though the transformation from soft to hard cannot be
brought about by change of temperature, it is readily effected
mechanically.   Whenever work is done on the C phase,
hardening takes place.   The micro-structure shows that this
hardening is always accompanied by the disappearance of
crystalline and the appearance of vitreous and granular
characters.   Polished or flowed surfaces are covered with a

smooth glassy layer, the surface of which is either as featureless as that of a liquid, or is drawn into smooth ridges and furrows which show unmistakable signs of having been ploughed in a viscous liquid filled with granules. (Figs. 5–14.)

40. By polishing across the ridges and furrows which have been ploughed by fine emery, a smooth covering layer is spread alike over ridges and furrows. By arresting this cross flowing at various stages its progress may be studied, and photographic records may be preserved which confirm the view that an essential feature of all polishing is *the transient existence of a layer of molecules in a state of mobility closely analogous to that of a liquid.* Records of this kind have already been published *. (Figs. 4–12.)

41. By a comparison of the structure which results from polishing with that which is developed by beating, rolling, or pressing, it is found that those effects which in polishing are strictly confined to the surface, penetrate to a greater and greater depth under the more drastic forms of treatment. It is thus made plain that the effects of flow are not confined to the outer surface only, but that they take place at all points where the strain reaches the stage at which mobility of the molecules is induced by the movement of one portion of the substance against another.

42. Accepting the existence of the mobile phase as proved by the records of micro-structure, the complete transformation from soft to hard may be written $C \longrightarrow M \longrightarrow A$; M being the intermediate mobile phase.

43. The condition which approaches most nearly to that of this mobile phase is that existing in an under-cooled liquid in which molecular freedom continues after the temperature has fallen below the solidifying-point. In this case it may be supposed that the molecular activity due to temperature has suffered curtailment without actually disappearing into the comparative fixity of the solid state. Till the temperature has fallen below the transition point the fixity is only comparative ; for we know that at this point there is still sufficient molecular activity to complete the transformation from A to C. In the case of silver the transition-temperature is not higher than $250°$, that is about $700°$ below its liquefying-point. But below the transition temperature the molecules must remain fixed relatively to each other, for otherwise it would be difficult to account for the state of strain which exists in the hardened phase as shown by its optical and electrical properties (pars. 27 to 34 and 70). The mobility of the molecules in the M phase is therefore not due to heat, but to motion

* Proc. Roy. Soc. vol. lxxii. pp. 218–225, figs. 1–12.

directly imparted to them by the movement of one portion of the substance over another.

44. The two kinds of mobility, M and M′ in the transformations C $\longrightarrow$ M $\longrightarrow$ A and A $\longrightarrow$ M′ $\longrightarrow$ C may be compared to the two ways in which the hands of a watch may be moved. In the M′ phase the movement of the hands is due to the internal energy of the watch which is centred in the coiled spring ; in the M phase the movement is imposed on the hands from without by the turning of the setting spindle.

45. The existence of mobility in the transformation from hard to soft through M′ falls perfectly in line with the phase transformations which have been studied by metallurgists for many years past. Barus suggested that " when iron passes through the temperature of recalescence its molecular condition is for an instant almost chaotic." * Roberts-Austin, referring to the case of a glass-hard steel rod suddenly exposed to a temperature of 300°, says "that if all the molecules passed from Osmond's α state to his β state together, the steel would necessarily be a liquid."

46. The existence of a mobile phase of an entirely new kind has been brought to light and rendered practically certain by the microscopic observations to which I have referred. A further observation of a very singular kind which bears directly on this subject is detailed later in par. 71.

Some Applications of the Phase Theory of Flow.

47. The observations of Ewing and Rosenhain on the crystalline structure of metals and the deformation of that structure by overstrain † have demonstrated the important part which is played by crystalline deformation in masses of metal. They have shown that it is possible with the microscope to trace the rougher mechanism of the deformation as it takes place by successive slips of the lamellæ of which the crystalline grains are composed. The phase theory now supplies what was needed for a more intimate explanation of the changes which occur at the gliding surfaces. My observations on surface-flow have shown how slight is the force required to break down the crystalline into the amorphous phase at a surface. It is therefore evident that the lamellæ cannot slip over each other without there being formed between them an exceedingly thin mobile layer which immediately passes on into the hardened phase. By the slipping over each other of grains, of lamellæ, and of

* 'Nature,' vol. xii. p. 369 (1890) ; 'Introduction to the Study of Metallurgy,' p. 170 (1902).

† Phil. Trans. Roy. Soc., A. vol. cxciii. pp. 353–377.

the units of which the lamellæ are built up, exceedingly thin layers of the amorphous phase are formed throughout the whole mass of strained metal. Slipping is easy so long as fresh moving surfaces are forthcoming for the supply of the mobile phase; but when all the available crystalline phase has become encased in the unyielding amorphous phase, plasticity *under these particular stresses* comes to an end.

48. This encasing of the crystalline elements with films of the hardened phase, which occurs whenever movement takes place among these elements, seems for the first time to supply the needed explanation of the passage of a metal through a highly plastic stage to a state of hardness and tenacity much greater than it originally possessed.

49. One of the most remarkable features in hardening is the large effect which is produced even by an apparently very incomplete transformation of the C phase. A bar of metal which has been ruptured by tensile stress shows a microstructure which consists of comparatively large crystalline aggregates, and yet the tenacity has been largely increased by the straining. Even assuming that these crystalline aggregates are very thoroughly interpenetrated by films of the amorphous phase, the greater part of the metal is still visible in the crystalline phase.

50. I have made many attempts to destroy all traces of the C phase by thoroughly beating strips of gold- and silver-foil, but have not yet succeeded in doing so. The result of beating gold-foil 0·42 mm. in thickness till it covered nearly three times its original area was to alter the metal into a hard springy, tenacious state, the surfaces being thoroughly flowed and vitreous looking. Etching with aqua regia disclosed a deep granular layer immediately under the vitreous covering, and by further etching this granular layer was removed, and the disarticulated skeletons of crystals were disclosed. The strip of foil from which the A phase had been removed by etching was soft and pliable, and had completely lost its spring. (Figs. 3–8.)

51. It is well known that as the diameter of a wire is reduced by drawing it through a die, the tensile strength increases in a very marked degree. In this way the tensile strength of Swedish iron may be raised from 20 tons per square inch in the bar form, to 80 tons per square inch in a wire of 0·1 mm. diameter. In view of the observations in the preceding paragraph (50), we may conclude that even in a wire of this degree of fineness a considerable amount of the crystalline phase remains still untransformed. Even the high tensile strength of 80 tons per square inch must therefore fall

very far short of that of the completely transformed amorphous iron, if we were only able to produce it.

52. From quite different considerations from the foregoing many metallurgists are disposed to associate the hardening of iron under thermal treatment with the existence of an allotropic form of the metal itself. From observations made in another connexion *, I am disposed to think there are grounds for believing that metals may be transformed into and retained in the amorphous phase at temperatures above the transition-point by the mechanical action of gases. The suggestion here is that gas molecules passing in and out among the metal molecules may retain the metal in the mobile condition of phase M, and thus effectually prevent its crystalline rearrangement in the C phase. (See also paragraph 71.) It is evident that if this gaseous disturbance were to continue till the metal had cooled to the transition-point, the opportunity for crystalline aggregation would be lost, and the amorphous condition would be stereotyped in the solid metal.

53. Kendal has found that nickel may be kept at a temperature of 1300° for any length of time without losing its tenacity if hydrogen is kept diffusing through the hot metal. It seems not improbable that this is a case in which the mobile and amorphous phases result from the mechanical interference of gaseous molecules with the phase transformation.

54. In a similar way it may be found that to the evolution of hydrogen from a mass of cooling iron which takes place when a temperature of 850° is reached, is due the hardness which is believed by Osmond to be an intrinsic quality of iron above the first arrest-point in the cooling curve.

55. The coexistence of the two phases in the same mass of metal may explain the variety in the texture of specimens of pure metal which have been subjected to different methods of working. In hammering, the crystalline aggregates are flattened and spread out, and with their covering films of the hardened phase may form a laminated or flake-like structure. In rolling or wire-drawing these flakes are elongated longitudinally, thereby producing a more fibrous structure.

56. MM. Osmond, Frémont, and Cartaud †, from a study of the modes of deformation and rupture of iron and soft steel, have been led to the conclusion that the behaviour of these metals under overstrain demands that they should possess simultaneously three types of structure, amorphous, cellular,

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* " The Action of Ammonia on Metals," Beilby and Henderson, Chem. Soc. Journ vol. lxxix. p. 1245.

† *Revue de Métallurgie,* January and April, 1904.

and crystalline. The phase theory of hardening shows that these three types of structure must be present in any mass of metal which has been subjected to overstrain, and that their existence does not necessarily depend on the presence of several crystalline phases such as occur in iron and steel. The granules, which are always present in the most perfectly flowed metal, may be regarded as minute cells encased in the hardened phase. The larger crystalline aggregates may also be so regarded, for they also are encased. The increased rigidity of the hardened metal results from this partitioning up of the whole mass by thin rigid cell-walls which enclose and protect their contents of plastic phase from further deformation and transformation.

57. When a greater stress is applied to the hardened mass, the rigidity of the cellular structure has to be overcome before further yielding can take place. When this structure breaks down, the plastic phase which is thereby reached is set in motion, and forms itself into a new set of protecting partitions. It appears to me that only in this way can we account for the immediate and marked increase of rigidity which results from the transformation of a relatively small amount of the softened phase.

58. The breaking down of tenacity and elasticity under frequent repetitions or reversals of stress is more easy to understand when it is recognized that every movement of the crystalline elements over each other, however slight, must lead to the transformation of the C into the A phase. The hardened surfaces or partitions thus produced, after being frequently broken down and remade, must eventually arrive at a stage when the plastic material in their neighbourhood is all used up. The movement will then tend to open up partings which, in the absence of fresh plastic material, will remain open, and will form the starting-point of fissures at which rupture will ultimately take place.

59. In the commercial forms of iron and steel the presence of other constituents in various phases will complicate the effects; but even in such cases the phase transformation $C \longrightarrow M \longrightarrow A$ must play a leading part in the structural changes which take place. Every crystalline phase, whatever its constituents, must either be rigid enough to resist deformation; or if deformation takes place, it must be partially converted into the amorphous phase. An eutectic in which crystalline grains are embedded may become so hardened by flow, as to supply a rigid cellular structure enclosing the grains. Or the amorphous eutectic may be so fragile as to lead to the breakdown of the whole structure.

60. It is plain that the way in which work is done on the plastic metal must have an enormous influence on its structure and qualities in the hardened state. A wire which has been hardened by stretching is much inferior in tenacity to one which has been hardened by drawing through a die. This must result from a difference in the structural disposition of the two phases in the hardened metal. In the stretched wire the surface-layer during stretching must be alternately made and broken many times, and is finally left rough and open. In drawing, on the other hand, the flow of the surface as the wire is leaving the die ensures that it is strong and unbroken.

61. One result of the intermixture of phases in strained metal is that the crystalline aggregates, though isolated from each other by the amorphous phase, are distributed in a form which ensures the quickest possible transformation of A $\longrightarrow$ C, when the transition temperature is reached. Consider the deformation of a crystalline grain which takes place by flattening. The lamellæ of oriented units are spread out in all directions like the limbs of a disarticulated skeleton separated from each other by the amorphous phase. Each fragment of crystalline aggregate is made up of similarly oriented units, the adjoining fragments being for the most part similarly oriented. When the transition temperature is reached, the crystallic force of these detached aggregates acts across the gaps between them, and compels the molecules of the amorphous phase into the same orientation. A new skeleton with larger limbs and fully articulated is thus formed, and proceeds to clothe itself further by extending the area of the oriented molecules between its branches. In this way it is possible to explain the more vigorous growth of crystals in strained than in unstrained metal, which was supposed by Ewing and Rosenhain to result from a process of electrolysis in which an eutectic or foreign substance played a necessary part [*].

62. When a metal is rapidly cooled by quenching, strains may be set up which lead to a transformation of C into A phase. Muir has found that hardening by quenching results from the overstraining of the successive layers of material in a rod or mass of metal [†]. If the cooling is very rapid, a good deal of the strain will occur after the transition temperature is passed, and its results will remain therefore in a change of structure due to the transformation C $\longrightarrow$ M $\longrightarrow$ A.

63. With regard to the elastic properties of metal in the

* Phil. Trans. A, vol. cxcv. p. 297.
† Proc. Roy. Soc. vol. lxxi. p. 89.

amorphous state, it must be remembered that all the observations on these properties have necessarily been made not on the pure A phase, but *on mixtures of the two phases.* In these observations the *structure* which has resulted from the particular mode of hardening has probably had an important influence on the results obtained. This remark applies also to observations on the electrical and to the other mechanical properties. In every case, however, it can be said that as the transformation into the A phase has been pushed further and further, the special properties associated with it have become more and more marked.

64. The transition temperatures of silver and other metals were determined by a softening test. The metal was beaten into a thin elastic strip, and was wrapped round the bulb of a thermometer which was plunged into an air-bath of which the temperature was gradually raised. From time to time the strip was removed and examined for signs of softening. The lower point of the transition range was taken as soon as any sign of softening was observed. Heating was then continued till the strip became so soft that it could be laid perfectly flat on a glass plate by a slight pressure with the finger. The following results were obtained :—

Silver began to soften at $230°$, and was completely soft at $265°$.

Gold	,,	,,	$250°$	,,	,,	$280°$.
Copper	,,	,,	$250°$	,,	,,	$290°$.
Magnesium		,,	$300°$	,,	,,	$360°$.

The transition temperature of silver by thermo-junction method was $260°$. By quickly raising the temperature of a thermo-junction of hardened and annealed silver to a high temperature, a much higher E.M.F. was obtained than that given at the transition temperature. Calculating this E.M.F. into degrees, the temperature to which the junction had been raised was $750°$. This simply shows that, as is usual in the transformations of solids, the element of time plays an important part.

Thermo-junctions of some other metals heated in this way gave the following maximum E.M.F. :—

Copper, 32 ; iron, 800 ; and platinum, 24 micro-volts.

In 1899 Dr. Magnus Maclean made a series of observations on the effects of strain on the thermo-electric qualities of metals [*]. With copper soft and hard the E.M.F. varied from $0·0027$ to $0·0264$ micro-volts per degree. Steel gave $0·1028$; nickel, $0·3784$; lead, $0·0076$ micro-volt. These observations

[*] Proc. Roy. Soc. vol. lxiv. p. 322, and vol. lxvi. p. 165.

were not made primarily with the object of ascertaining the maximum E.M.F. between the soft and hard varieties, but to compare the effects of different amounts and kinds of strain; the soft metal was therefore not necessarily in the condition to which it would have been brought by raising it to the transition temperature. The highest temperature used in Dr. Maclean's determinations was 100° C.

Note on the Order of Magnitude of Mobile Films and Granules.

68. This aspect of the molecular changes which take place in metal films and on metal surfaces has been pretty fully discussed by me on another occasion * ; but as it has a bearing on the subject of the present paper, it ought to be referred to here. The molecular changes which are due to the formation of the mobile phase occur for the most part in films whose thickness is well within the range of the molecular forces. When it is remembered that the number of molecules in the thickness of a gold-leaf is probably in the region of 900, it is plain that ample material for molecular flow is to be found in films which are only a small fraction of this thickness. In the more easily flowed metals the surface films formed by polishing may vary from 1000 to 5000 $\mu\mu$ in thickness, while in the less easily flowed metals they may be 500 $\mu\mu$ or less. At surfaces which have moved over each other very slightly, films of only a comparatively few molecules in thickness might be formed. In calc-spar it was found that a single stroke of a leather-covered finger firmly drawn across a fresh face was sufficient to break down the crystalline structure for a noticeable depth below the surface; the depth of the disturbance in this case must, however, have been very slight.

The granules which are seen in surface-layers have an apparent size of from 500 to 2000 $\mu\mu$ in diameter; their thickness, however, is often very much less than this.

69. Having regard to the extreme minuteness of the simple molecules in metals, it may be safely assumed that relative movements of the molecules which are so minute as to be far beyond the resolving-power of the microscope, may be sufficient to undo the work of crystalline aggregation, and to transform the C phase into the A. This would hold good even if the molecules which take part in these changes are not simple, but are complexes of relatively large size.

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* Hurter Lecture, Liverpool, 1903; Soc. Chem. Ind. Journ. vol. xxii. p. 1169.

Note on Flow in Hardened Steel.

70. In 1890 Mr. C. A. Carus-Wilson * discussed the hardening of steel by overstrain, and foreshadowed some of the conclusions which are advanced in the present paper. His reading of the stress-strain curve of hardened steel as an indication of *molecular displacement during the progress of viscous flow* ending in the production of greater resistance to stress, which he associated with *molecular strain and atomic displacement*, is particularly interesting in the present connexion. In one passage he says :—" These stress-strain curves indicate the condition of strain as by gradually increased stress the steel is converted from an elastic solid to a viscous fluid." While he clearly recognized a stage of viscous flow ending in a state in which the molecules were in a state of strain, he makes no suggestion as to why the viscous flow should terminate in a condition of greater hardness ; or indeed why it should terminate at all.

Note on the Mobile Phase M.

71. On one occasion I was, through a fortunate chance, able to watch actual movement taking place in a metal surface. A piece of copper which contained from 5 to 7 per cent. of nitrogen had been drawn over the surface of a fine Turkey stone. On examining the streak left by the copper by normally reflected light with a 4-mm. objective of 0·95 M.A. and a high-power eyepiece, it was noticed that movement of some kind was going on on the surface. By careful watching it was seen that the surface was being broken up by the issue from the metal of minute gas-bubbles. The bubbles were detected by the holes which they left on bursting. The holes were from 500 to 1000 $\mu\mu$ in diameter. The process lasted for fully an hour, during which time the appearance of the metal completely changed, the original smooth surface having given place to a spongy structure. Fortunately a careful record of the observation was made at the time, for I have not yet been able to reproduce the particular combination of conditions to which it was due. The mobility of the metal was evidently caused by the passage of the gas from among its molecules (see pars. 46, 52, and 53).

72. In 1893 Mr. Thomas Andrews † made a series of experimental observations on "The Effect of Stress on the Corrosion

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* Phil. Mag. February, 1890, p. 203.
† Proc. Inst. C. E. vol. cxviii. p. 10 (1894).

of Metals." In these the alterations in the mechanical properties which resulted from overstrain were studied side by side with the corresponding alterations in the electro-chemical properties of the metal before and after overstrain. It was found " that an average E.M.F. of 0·016 volt obtained between the strained and the unstrained portion of wrought-iron shafts under the conditions recorded." The electrolyte used was a saturated solution of sodium chloride. The tensile strength of the iron used in these experiments was 22·83 tons per square inch before straining, and 30·02 tons after straining.

XXVIII. *On the Electrical Vibrations associated with thin terminated Conducting Rods. By* H. M. MACDONALD, *F.R.S.*[*]

I VENTURE, with diffidence, to put forward the difficulties which I find in the views adopted by Lord Rayleigh in a recent note (Phil. Mag. July 1904, p. 105). In the first place there is a wide discrepancy between his result and that of experiment, which, so far as I know, does not place an electric loop at the free end of the wire. It is true that Kiebitz[†] has thought that this conclusion is involved in his result that the length of a ring resonator which he found to respond best to a straight wire vibrator is approximately the same as the length of the vibrator, but, as Prof. Pollock has remarked, such an inference does not follow. In fact Kiebitz's result is in complete agreement with that of Sarasin and de la Rive, who found that when waves were being propagated along a straight wire with a free end, the distance of the first node from the free end of the wire was approximately equal to the half circumference of the resonator that responded to these waves. They, however, further found that the distance of the second node from the first was approximately four times the diameter of this resonator (*i. e.*, somewhat greater than $2\frac{1}{2}$ times the half circumference), showing that the free end was not a true loop[‡].

The only suggestion offered by Lord Rayleigh towards bridging the discrepancy, namely, that the conductivity may

not be sufficient, does not appear to meet the case, as experiment would seem to show that the phenomena are the same whatever the material of the wire may be, and that only the distance of the last node is abnormal. Theoretically it is of course beyond doubt, and all experiment confirms the conclusion, that the circumstances of propagation of electric waves of high frequency along a wire are independent of the magnitude or form of its cross section ; the sheet of intense disturbance around the wire is thin, the direction of propagation is along its surface, and the velocity of propagation, therefore, that of radiation in the surrounding medium.

The only difficulty is in the treatment of a free end. According to the ellipsoidal analysis referred to by Lord Rayleigh, the wave is simply reflected at the free end without sensible loss by radiation, the free end being a true electric loop. Such modes of free vibration in terminated wires, if theoretically possible, are thus largely isolated, and have very slight connexion with the surrounding medium. Conversely, they ought to be correspondingly different to excite waves transmitted across this medium ; and it appears open to some question whether they are sensibly excited at all.

The facts seem to be as follows : If a wire is bent round into a resonator with the ends close together, the radiation from these two adjacent ends is of the same type as that from a Hertzian oscillator, but the rate of decay of the oscillations is very small. If the wire is partially straightened out, the vibrations are rapidly damped, but in a way not affected by further change of form : there must in consequence be powerful radiation from the ends. Observation, with Marconi's *antennæ* for example, corroborates this view, that wires carrying waves emit powerful radiation which must come mainly from their free ends.

This facility of egress of radiant energy from a free end seemed to me to be the essential feature of the problem of the electrical oscillations associated with a terminated straight wire. The difficulty lies in deciding what the circumstances of the radiation from a free end are, the character of the disturbance around the end being no longer so obvious as in the case of the resonator.

An important difference between the view supported by Lord Rayleigh and that which regards the effect of the free ends as the essential feature, is that on the former view the damping of free vibrations on a terminated straight wire would not be very rapid, whereas on the latter it would. Now experiment would seem to show that such vibrations

are very rapidly damped ; and in fact theoretical considerations based on experiment indicate that the rate of damping of the free vibrations of longest period associated with a terminated straight wire is not very far removed from the order of magnitude of the known result for a spherical vibrator.

The experiments of Sarasin and de la Rive, referred to above, show that the period of the waves to which a terminated straight wire resonates is approximately that of a nearly closed ring resonator of the same length.   Prof. Pollock's experiments * show that when a ring resonator is opened out the free period at first diminishes, but that, after it has been opened out a certain amount, further straightening of the wire does not alter the free period.   Thus, for a wire 152 cms. long bent into a ring resonator the wave-length was found to be 404 cms., and when it was straightened out the wave-length diminished to 387 cms., remaining at this length when the wire was farther straightened out ; so that it may be concluded that the ratio of the wave-length of the free oscillations of longest period associated with a straight wire 152 cms. long to the wave-length of the oscillations to which it resonates is 387/404.   Now, if we may compare with a simple vibrating system, whose equation of motion is

$$\ddot{x} + 2\mu\dot{x} + \kappa^2 x = 0,$$

the period $2\pi/p_1$ of the oscillations to which it resonates is given by $p_1^2 = \kappa^2 - 2\mu^2$, and the free period $2\pi/p_0$ by $p_0^2 = \kappa^2 - \mu^2$.   Hence, if λ_1 and λ_0 are the corresponding wave-lengths,

$$\frac{\lambda_0}{\lambda_1} = \sqrt{\frac{\kappa^2 - 2\mu^2}{\kappa^2 - \mu^2}} \, ,$$

that is

$$\lambda_0/\lambda_1 = \sqrt{(1 - 1/4\pi^2 n^2)},$$

where n is the number of vibrations executed before the amplitude falls to $1/e$ of its value.   Taking $\lambda_0/\lambda_1 = 387/404$ the value of n is ·55, and it may be inferred that this represents roughly the rate at which free oscillations associated with a terminated straight wire 152 cms. long decay.

* Phil. Mag. June 1904.

XXIX. *On the Atomic Weight of Radium, and on Relationships between the Atomic Weights of the Elements and their Spectra.* By W. MARSHALL WATTS [*].

IN the Philosophical Magazine for December 1903, Prof. Runge criticises adversely some attempts of mine to connect the spectra of certain elements with their atomic weights, and in particular an attempt to determine the atomic weight of radium from spectroscopic data.

Prof. Runge objects that the lines which I have sought to connect together are not homologous, explaining that in order to be homologous the lines must have similar characteristic features, and in particular must behave in the same way in the magnetic field. He gives an example of really homologous lines, 4226 of calcium, 4607 of strontium, and 5536 of barium ; and remarks that " it is to homologous lines like these that we must look in order to show the connexion between the spectra and the atomic weights." Now, the oscillation-frequencies of these lines (in air) are respectively 23657·94, 21703·65, and 18064·59, and from these data we may construct the formula

$$\text{atomic weight} = \sqrt{(24178 \cdot 72 - \text{O.F.}) \times 3 \cdot 08772}$$

(where O.F. is written for oscillation-frequency for the sake of brevity). This formula gives the atomic weights of calcium, strontium, and barium as 40·1, 87·42, and 137·4 respectively ; that is to say, it enables us to calculate the atomic weight of strontium from those of calcium and barium with reasonable accuracy, obtaining 87·4 instead of 87·6. These very lines, therefore, form one instance of the truth of my first proposition, " that *in some cases*, the differences in oscillation-frequency are proportional to the differences between the squares of the atomic weights."

As only a few of the brightest lines have been examined in the magnetic field, this criterion can only be employed in a few cases. I have supposed lines in the spectra of allied elements to be homologous whose oscillation-frequencies can be calculated from the same value of m in the formulæ of Kayser and Runge. In the simpler (arc) spectra of the metals of the alkalies, practically all the lines are accounted for either in the principal series or in one of the two subordinate series which these formulæ represent. The lines of potassium, rubidium, and cæsium, quoted below from my paper in the Phil. Mag. of Feb. 1903, for example, are lines

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[*] Communicated by the Author.

of the principal series for the values 5 and 6, and I therefore consider them to be homologous.

	Cæsium.	Rubidium.	Potassium.
$m=5$	25714·67	27841·66	29006·61
	25794·94	27876·66	29015·03
$m=6$	27646·66	29841·57	31077·52
	27686·72	29860·91	31082·25

Assuming that these lines are homologous, and using them to calculate the atomic weight of rubidium from that of potassium 39·15 and that of cæsium 133, we obtain

$$m=5 ...... \quad 85·15$$
$$85·11$$

$$m=6 ...... \quad 85·75$$
$$\overline{85·70}$$

Mean 85·43 instead of 85·2.

I must, therefore, respectfully maintain that *in some cases* my first law is very nearly true. In other cases it is certainly not true, and must be modified.

In regard to the second law, that the difference in oscillation-frequency of homologous doublets is proportional to the square of the atomic weight, this is also true in some cases, notably with zinc and cadmium. The following table shows the differences between the first and third lines in the corresponding triplets of cadmium and zinc, in which * denotes lines of the first subordinate series, and † lines of the second subordinate series, the figure before the * or † giving the value of m in the formulæ of Kayser and Runge :—

	Cadmium.	Zinc.
3†	1713·0	578·7
4*	1701·3	571·0
4†	1711·2	578·5
5*	1699·3	575·8
5†	1713·0	578·6
6*	1704·0	576·4
6†	1713·2	579·2
7*	1704·7	579·2
7†	1664·9	586·4
8*	1639·4	546·3
8†	1700·5	578·1

The ratios of these intervals are :—

3†	2·960
4*	2·980
4†	2·952
5*	2·951
5†	2·961
6*	2·956
6†	2·958
7*	2·943
7†	2·839
8*	3 001
8†	2·942

The mean of these numbers is 2·948 and the ratio of the squares of the atomic weights is 2·960.

Calculating the atomic weight of zinc from that of cadmium from these data we obtain :—

3†	65·33
4*	65·12
4†	65·35
5*	65·43
5†	64·32
6*	65·37
6†	65·35
7*	65·52
7†	66·71
8*	64·88
8†	65·54

The mean of these numbers is 65·45 : the accepted value of the atomic weight of zinc is 65·4.

This second law must then be admitted to be true in some cases, but it is certainly not true generally. It may be, however, that in other groups of related elements the distances apart of the lines of a pair (or triplet) are proportional to the atomic weight raised to some other power than *two* exactly. Thus, for copper and silver the power is 2·4765 ; for magnesium, calcium, strontium, and barium 1·6214, and for aluminium, gallium, indium, and thallium 2·1127. These numbers are calculated from the differences between the lines of the series dealt with by the formulæ of Kayser and Runge.

The law employed by Runge and Precht in calculating the atomic weight of radium, that the differences of the oscillation-frequencies of homologous doublets is proportional to some power of the atomic weight, or that the logarithm of

the difference is a linear function of the logarithm of the atomic weight, and their formula

$$\text{atomic weight} = \log^{-1}(0.2005 + 0.5997 \log x),$$

where x denotes the difference between the lines of the doublet, are equivalent to the assertion that in the group magnesium, calcium, strontium, and barium, the difference is proportional to the atomic weight raised to the power 5/3.

Any two elements are sufficient for the calculation of such a formula. Taking the differences as given by Runge and Precht, viz. 91·7 for Mg, 223 for Ca, 801 for Sr, 1691 for Ba, and 4858 for Ra, we find the following formulæ :—

From Mg and Ca at. wt.$=\log^{-1}(0.28002 + 0.56088 \log x)$ whence Ra$=222·7$

	Mg	,,	Sr	,,	$=\log^{-1}(0.22788 + 0.59051 \log x)$	,,	Ra$=253·9$
,,	Mg	,,	Ba	,,	$=\log^{-1}(0.22192 + 0.59355 \log x)$	,,	Ra$=257·1$
,,	Ca	,,	Sr	,,	$=\log^{-1}(0.16809 + 0.61110 \log x)$	,,	Ra$=263·6$
,,	Ca	,,	Ba	,,	$=\log^{-1}(0.17563 + 0.60789 \log x)$	,,	Ra$=261·0$
,,	Sr	,,	Ba	,,	$=\log^{-1}(0.19338 + 0.60239 \log x)$	,,	Ra$=259·5$

If we take the differences between the first and second lines of the triplets in zinc, cadmium, and mercury, viz. 386·39, 1159·39, and 4633·27, we obtain from the numbers for Zn and Cd the formula

$$\text{atomic weight} = \log^{-1}(0.5405 + 0.49285 \log x),$$

which gives for mercury 222·48, a result considerably too high.

I conclude, therefore, that this method of calculation has a tendency to give the number too high for the higher atomic weights.

If radium has the atomic weight 225, it no doubt occupies the place below mercury in the vertical column containing Mg, Ca, Sr, Cd, Ba, Hg, in the arrangement of the periodic law : if it were 258 it must find a place either in a different vertical column altogether, or in the same column lower down, for which the atomic weight should not exceed 250, and in this case it should be more closely analogous to mercury and cadmium than to barium and strontium.

If the true weight is 258, and if the chemical determination gives 225 because of contamination of the material with barium, then it must have contained more than 25 per cent. of impurity, and this seems an impossible supposition.

Assuming the truth of Runge and Precht's law we may indulge in some speculations as to the classification of the elements.

In the group B, Al, Ga, In, Tl, taking the differences as

15·4, 112, 826·4, 2213·2, and 7794·2 respectively, we obtain the formula

$$\text{atomic weight} = \log^{-1}(0\cdot47185 + 0\cdot4773327 \log x),$$

which gives for

Boron............	10·83	instead of	11
Aluminium ...	27·65	,,	27·1
Gallium	71·23	,,	70
Indium	113·54	,,	114
Thallium	206·04	,,	204·1

In the group Be, Zn, Cd, and Hg, taking the formula already given, we obtain for

Beryllium	8·8	instead of	9·1
Zinc...............	65·4	,,	65·4
Cadmium.........	112·4	,,	112·4
Mercury	222·5	,,	200

If we include radium under this formula with the difference 4858·5, we obtain for the atomic weight 227·75.

If we take the differences between the first two lines of the triplet-series in Mg, Ca, and Sr, viz. 40·7, 102·6, and 394·0, we obtain the formula

$$\text{atomic weight} = \log^{-1}(0\cdot45705 + 0\cdot57223 \log x),$$

which gives for

Beryllium	8·43	instead of	9·1
Magnesium ...	23·89	,,	24·36
Calcium.........	40·58	,,	40·1
Strontium	87·56	,,	87·6

The lines of Mg, Ca, and Sr now employed by Runge and Precht for calculating the atomic weight of radium are *pairs* of lines not belonging to the triplet-series.

If we calculate a formula for the group containing copper and silver from the differences 248 and 920·8, we obtain

$$\text{atomic weight} = \log^{-1}(0\cdot83405 + 0\cdot4378 \log x).$$

Taking the differences for Li, Na, Au, and Hg as 0, 17·2, 3817, and 4633·2 respectively, we obtain the following atomic weights :—

Lithium	6·82	instead of	7·03
Sodium.........	21·52	,,	23·05
Copper	63·6	,,	63·6
Silver	107·93	,,	107·93
Gold............	190·7	,,	197·2
Mercury	206·2	,,	200

Lithium is included upon the supposition that the lines are doublets with an infinitesimal separation.

For the metals of the alkalies the formula

$$\text{atomic weight} = \log^{-1}(0{\cdot}65284 + 0{\cdot}53869 \log x)$$

gives for

Lithium	4·5	instead of	7·03
Sodium.........	20·82	,,	23·05
Potassium ...	39·62	,,	39·15
Rubidium......	85·02	,,	85·4
Cæsium	133·94	,,	133

It seems that magnesium fits in better with calcium and strontium than it does with zinc and cadmium, and that lithium and sodium will not go with potassium, rubidium, and cæsium, but fit in better with copper and silver.

XXX. *Notices respecting New Books.*

Radio-activity. By E. RUTHERFORD, *D.Sc., F.R.S., F.R.C.S.*
(Cambridge : at the University Press.)

IN the course of the eight years which have elapsed since Becquerel's discovery of the radio-activity of an uranium salt, quite a new aspect has been given to our ideas of atomic structure by investigations arising from that discovery and the development of methods of detecting and measuring minute electric currents. The familiar gold-leaf electroscope—the valuable qualities of which we see in a new light—has come forward pre-eminent among instruments of measurement as providing means of exploration into matters which were till recently thought to be probably forever beyond our ken.

Professor Rutherford's book on Radio-activity will be taken as supplementing the treatise of Professor J. J. Thomson on the general subject of the conduction of electricity through gases, and, it is hardly necessary to add, is a book of the highest interest and value. The book of course appears while yet the field is not nearly exhausted and while still a large band of enquirers are eagerly pushing forward our understanding of the many issues involved. It, of necessity, marks but a stage of enquiry, leaving many pressing questions unanswered. The reader, however, feels convinced of the solidity of foundation of the central idea pervading the work— the intimately atomic nature of radio-activity. The whole experimental evidence so far obtained for this great advance is before him, marshalled and criticised by the man who has played so brilliant a part in establishing it. Along with this record of patient experimental work, to read J. J. Thomson's ' Electricity

and Matter ' is to feel we have gained a new cosmos ; to expand in Richter-like flights among the galaxies of the " indivisible " material unit !

The general nature and occurrence of radio-activity and the subject of the ionization of gases occupies the first two chapters. J. J. Thomson's great advance in finding the value of e, and hence of m, finds a place here. The ionization of gases is clearly treated in the brief space given to it. The succeeding chapter on Methods of Measurements will not only be valuable to those wishing to work in this field, but is requisite for the understanding of the extra-ordinary progress of research of which the book is a record. Rate methods, now almost exclusively used, alone are described. Chapter IV. records the evidence which has been advanced for the material nature of the alpha and beta radiations. Kaufmann's experimental verification of the views of J. J. Thomson on the electrical origin of mass, using the formula of M. Abraham, is given here. On the subject of the spinthariscope we confess to some difficulty in seeing how it can be assumed on the evidence that the impact of *each* alpha ray is marked on the screen by a flash of light. The phenomenon, we take it, is similar to the light given out by sugar, &c., when crushed, and the irritation, mechanical or electrical, required to produce a cleavage might surely be as often due to the near coincidence of, say, ten collisions as the effect of one ; if, indeed, we may assume that the impact of one ray is ordinarily adequate. When we have reached section 87 of this important chapter we have developed the subject so far as to see in the alpha particle a freed constituent of the heavy atom of radium, having a mass about that of helium, which must be rotating within the radio-active atom with a velocity sufficiently enormous to account for the speed in virtue of which we recognize its existence ; while the beta particles pour into space with a speed of motion about ten times as great, and strangely near the velocity of light. Reading further, we find that the nature of the gamma rays is by no means well deter-mined, although the weight of evidence assigns to them the character of very hard X-rays. The chapter concludes with an account of secondary rays, a subject possibly of considerable medical interest at no distant date. These deviable rays, copiously produced by gamma radiation, may afford a means of application of radio-activity to deep-seated disease which will be yet under the control of the physician and may be remitted at his will. The presence of an inert heavy metal, such as gold or platinum, at the seat of injury being alone required. Surface trials of this secondary radiation are being made, as we write, in a Dublin hospital and not without promise of success.

Measurements of the rate of emission of energy occupy the ensuing chapter. The author then briefly refers to the many strange effects of the radiations : phosphorescent ; physiological, &c. The subject of the continuous production of radio-active

matter, which follows, is mainly a record of the author's own brilliant researches in conjunction with Mr Soddy. The manner in which the author pierces the complexities of radio-active evolution, and derives numerical results by deductive reasoning aided by the one sensitive method of research, is more than admirable. Radio-active emanations are then treated. Here, again, the elucidation of the subject is mainly the author's, and the discovery of these transitional bodies practically his own. Excited radio-activity, again the independent discovery of Professor Rutherford, is dealt with in the next chapter. Chapter X. brings us to the consummation of finding the author's suggestion that in helium would be found a product of the material transformation of radium, verified in the experiments of Ramsay and Soddy.

The book is throughout so full of suggestive facts that comment within bounds is difficult. That the existence of radium, as a constituent of the Earth, has swept away the argument which would limit the time required by the geologist, has been known since the correspondence in ' Nature ' of September and October last year. Professor Rutherford shows, however, in his book how completely the view that the Earth's age is no longer determinable from temperature gradient inwards, is justified. Geologists must now take for their time limits what interval denudation rates afford—whether measured by sedimentation or solvent denudation.

With this subject is associated the origin of radium. It seems at present to be still quite unknown. In the work under notice preference is given to uranium as the source. Good reasons are given, but since then Mr. Soddy has shown in a letter to ' Nature ' that this source of derivation is very doubtful. May it not turn out that the escaped particles of the radio-active substances in pitch-blende are contributary to the formation of radium by intra-atomic combination with substances of *lower* atomic weight ? Perhaps this process goes further than the genesis of radium. There is an extraordinary richness of elemental varieties in pitchblende. Radium happens to be a comparatively unstable atomic configuration, hence its distinction among the others. We are certainly obliged to seek for some source of supply of radium. The rapid rate of its destruction involves this. And, by the way, Professor Rutherford's estimate of the rate of breakdown (p. 332) is even exceeded by the recent work of Ramsay and Soddy.

We confess we do not care much for the term " Conservation of Radio-activity." We are accustomed to use the term " Conservation " in connexion with quantities such as energy, areas, matter. If we say radio-activity is subservient to the law of the conservation of energy and admit its intrinsic atomic nature, we do not see that anything is gained by the phrase introduced by the author and Mr. Soddy.

We would like to see the table on p. 326 presented also in diagrammatic form.

The last chapter of the book is on the distribution of radio-activity in air, earth, and water. It closes like an unfinished drama of absorbing interest. We know that none of us will live to see the finished work, but we close the book feeling that it is well to have seen so much in our day.   J. J.

May 20th.

Mathematical and Physical Papers. By the late Sir GEORGE GABRIEL STOKES, Bart., *Sc.D., LL.D., D.C.L., Past Pres. R.S., &c.* Vol. IV. Cambridge: at the University Press. 1904. Pp. viii + 378.

A MELANCHOLY interest attaches to the publication of the fourth volume of Sir George Gabriel Stokes's Papers, for its appearance reminds one of the concluding words contained in the preface to vol. iii., in which the author expressed the hope that, "should life and health last," he would be able to put the remainder of his papers together without delay. Unfortunately, "these conditions"—as Dr. Larmor, who edits the present volume, remarks in his preface—"were not destined to be fulfilled ; and the task of completing this memorial of his genius has fallen to other hands."

In vol. iii. was commenced the series of important memoirs which contain Sir G. G. Stokes's contributions to optical theory ; and this series is continued in the present volume, of which they form the bulk. The period covered is 1852–1876. There were, however, numerous other subjects which claimed the author's attention, and we find notes on the theory of the Electric Telegraph, on Airy's Harton Pit experiment, on Clark's investigation regarding the change of form assumed by wrought iron and other metals when heated and then cooled by partial immersion in water : and many others. An appendix contains some most interesting correspondence between Sir G. G. Stokes and Lord Kelvin on the nature and possibilities of spectrum analysis.

The present volume, which is adorned with a beautifully executed portrait taken from an oil painting made in 1874 by Mr. Lowes Dickinson, is not the concluding one of the series. And the remaining papers are promised in a fifth volume, which is to contain a biographical notice, and possibly also a selection from Sir G. G. Stokes's scientific correspondence.

Die Kathodenstrahlen. Von G. C. SCHMIDT, *a. o. Professor der Physik an der Universität Erlangen* ("Die Wissenschaft," Heft 2). Braunschweig: F. Vieweg und Sohn. 1904. Pp. vii + 120.

SINCE the appearance of the electron theory, the interest taken by the scientific public in the phenomena exhibited by the discharge of electricity through gases has been enormously quickened. There

is a steadily growing demand for simple, non-mathematical accounts of both experimental results and their theoretical interpretation in the light of the new theory, and the demand is gradually being met. We heartily welcome the notable addition to this type of literature in the volume under review. The author writes in an easy and simple but deeply interesting style, and without burdening the reader with an excess of detail succeeds in presenting fully and clearly the general outline of his subject. The book is copiously illustrated, and contains a useful bibliographical appendix for those wishing to go more deeply into the subject.

Karl Heumanns Anleitung zum Experimentieren bei Vorlesungen über Anorganische Chemie. Von Prof. Dr. O. Kühling. Mit 404 in den Text eingedruckten Abbildungen. Dritte Auflage. Braunschweig: F. Vieweg und Sohn. 1904. Pp. xxix+818.

The art of arranging successful and instructive lecture experiments is not quite so easy to acquire as might at first sight be supposed, and perhaps nothing is more wasteful of time or more trying to the patience of the experimenter than the repeated failures of his efforts to exhibit some effect which, while easily perceived by a single person, is difficult to produce on a scale which will allow of its being readily detected by a large audience. To those whose duty it is to arrange for experimental illustrations of lectures on inorganic chemistry, we can warmly recommend the late Karl Heumann's book, now edited and brought up to date by Prof. Kühling. In it they will find a most valuable guide : the thorough and systematic manner in which the subject is treated leaves little to be desired.

In the introduction, the author deals with the designs of a chemical lecture-theatre, and considers in great detail the numerous arrangements required to ensure success and despatch in the carrying out of experiments. Next follows a section on glass-blowing, the treatment of cork and rubber stoppers, the removal of glass stoppers which have stuck fast, the collection of gases, the handling of liquid air, &c. The remaining portion of the book, which forms about seven-eighths of the total, is divided into two great sections, relating to experiments on the non-metallic and the metallic elements. Perfectly explicit directions are given for carrying out each experiment, and in most cases the apparatus employed is illustrated and fully described. The causes of failure are also very carefully considered, and various remedies and precautions suggested. To the professor or lecturer, as well as to his lecture assistant, the book should prove of inestimable value.

XXXI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. vii. p. 416.]

March 9th, 1904.—J. E. Marr, Sc.D., F.R.S., President,
in the Chair.

THE following communications were read :—

1. ' On the probable Occurrence of an Eocene Outlier off the Cornish Coast.' By Clement Reid, Esq., F.R.S., F.L.S., F.G.S.

An extensive deposit of subangular Chalk-flints occurs near Marazion, opposite a deep and wide valley which connects St. Ives Bay and Mount's Bay. This valley, though containing at St. Erth Lower Pliocene beds, is shown to be of much earlier date, and is probably an Eocene river-valley. Eocene rivers seem to have radiated from Dartmoor westward as well as eastward. The flint-and-chert gravel corresponds closely with the Eocene gravel of Haldon, and is apparently derived from a deposit under the sea off St. Michael's Mount. Continuing the direction of the Eocene valley seaward, the isolated mass of phonolite of the Wolf Rock is met with. The evidence suggests that, underlying the western part of the English Channel, an Eocene basin may occur comparable in importance with that of Hampshire.

2. ' The Valley of the Teign.' By Alfred John Jukes-Browne, Esq., B.A., F.G.S.

The Teign Valley is one of the most remarkable in the British Islands, because it is not a transverse valley preserving a general direction in spite of opposing ridges, nor is it a longitudinal valley running parallel to a dominant ridge, nor is it a simple combination of one with the other, as often happens ; but it apparently consists of parts of two transverse valleys linked by a longitudinal one.

The Teign runs off Dartmoor through a gorge which takes an easterly direction, as if it were going to join the Exe ; it is then deflected southward into what, with respect to the Permian escarpment, is a longitudinal valley ; this ends in a low-lying plain, and from this plain it escapes eastward to the sea through a transverse valley, which has been cut across the ridge of Permian and Cretaceous rocks.

Several attempts have been made to explain the anomalies of the course taken by the Teign ; but none of them is satisfactory, because the writers have not sufficiently considered the probable conditions of the surface on which the river-valleys were originated, or the extent to which the older rocks around Dartmoor may have been covered by Cretaceous and Tertiary deposits.

The author considers these points, and concludes that in Oligocene time a thick mantle of soft Neozoic strata must have stretched across Devon and the adjacent parts of the English Channel; that this mantle consisted mainly of Selbornian Sands and of the later Eocene deposits, the latter overlapping the former and passing onto the surface of the Palæozoic rocks; further, that these Eocene deposits covered all the central parts of Devon, and were banked up against the northern, eastern, and southern sides of Dartmoor. He assumes, moreover, that the post-Eocene elevation of the region gave this surface a general easterly slope; and consequently that, although streams ran off Dartmoor in all directions, those which drained eastward had the longer courses and passed from the moorland area onto a plain, the drainage of which was directed eastward to the shore of the Oligocene sea.

The general direction of the Upper Teign where it flows over the granitic area is east-north-easterly; the direction of its gorge as far as Clifford Bridge is nearly due east, and if the conditions were as above described, the precursor of this river is not likely to have followed the course of the present river beyond Clifford Bridge. There is not likely to have been any ridge or obstacle that would have deflected it so far to the southward, nor anything to prevent it from continuing its easterly course towards, and probably across, the valley of the Exe.

The valley of the Lower Teign below Dunsford is not likely to have existed in Oligocene time, but was part of the eastward sloping plain; the local drainage, however, may have been carried by a little brook flowing southward or south-eastward to join the river which was then initiating the valley of the Teign estuary. The erosion of the present longitudinal valley out of the Palæozoic rocks must have been accomplished in much later times, and was probably due to the development of the Permian escarpment.

The valley through which the Teign now flows from Newton to Teignmouth traverses this escarpment; and its excavation can only be attributed to a stream that flowed eastward from higher ground than the summit of Little Haldon. Such a stream is the Lemon or Leman, which rises on the east side of Dartmoor at a level of about 1200 feet above the sea. The ancestor of this stream must have carved its channel out of the ancient plain of Eocene deposits; and it is suggested that the valley of the Teign estuary is a portion of this ancient valley, which has survived all subsequent changes, except that of being cut down to modern base-levels.

The change which led to the diversion of the Upper Teign into this more southern valley is attributed to the later earth-movements, which gave a southerly tilt to the whole region, and a still greater local tilt owing to the formation of the Bovey syncline. This tilt would increase the velocity and erosive power of the stream which was then carving out the valley west of the Haldon Hills, and as it gradually cut down to a lower base-level, the little affluents which formed its head-waters would cut back northward into the

watershed which separated them from the eastward course of the Upper Teign. It is supposed that the portion of the Teign valley, which lies between Dunsford and Clifford Bridge, was formed by one of these affluents, and that it was deepened till the separating ridge at its head was reduced to a col or pass leading from the one valley into the other. A flood or the damming-up of the river by a landslip might send down the waters of the Upper Teign, and once this was accomplished the capture and diversion of the Upper Teign would be permanent.

The theory of the capture of one river by another has been accepted as an explanation of similar difficulties in the case of other rivers, and its application to the course of the Teign furnishes an intelligible explanation of the facts. The author thinks that some other river-courses and geographical features in Devon can be explained on the same theory of an easterly incline modified by a subsequent southerly tilt.

March 23rd.—J. E. Marr, Sc.D., F.R.S., President, in the Chair.

The following communication was read:—

' On the Moine Gneisses of the East-Central Highlands and their Position in the Highland Sequence.' By George Barrow, Esq., F.G.S.

The paper is divided into two parts. The first deals with the parallel-banded grey gneisses or gneissose flagstones of the Perthshire and Aberdeenshire districts, which, in their field-characters as well as in their composition and structure, are identical with the Moine gneisses of the North-West Highlands. A description is given of these gneisses, as seen in and about the Garry in Perthshire, and this is followed by a brief account of the same rocks in the ground to the east and north-east, extending to the Forest of Invercauld, north of Braemar in Aberdeenshire. Special attention is drawn to the fact that towards the eastern end of the area large masses of highly-quartzose gneiss occur, which are really part of the Central-Highland quartzites in what the author conveniently describes as a ' Moine-phase,' and should not strictly be included in the typical banded grey gneisses at all.

In the second part, dealing with the mode of ending-off of these gneisses to the south-east, it is shown that they cease to be recognizable as Moine gneisses, owing to the fact that they thin away and also become more finely banded, while at the same time they become less crystalline or cease to be gneisses. To prove this, an account is given of a series of sections lying along a belt 40 miles in length, extending nearly from Blair Atholl to the east of Balmoral, in Aberdeenshire. The first and most important of these occurs about

Gilbert's Bridge (in Glen Tilt), where the parallel-banded Moine gneisses can be traced passing slowly into the honestones, in which parallel banding is equally well shown. This is a well-known horizon in the Central-Highland sequence, lying next the white edge of the Highland Quartzite, forming, in fact, its original flaggy margin. These parallel-banded rocks are in many cases succeeded directly by a very impure phase of the Main or Blair-Atholl Limestone ; but in places patches of other material intervene, of which the most important is a dark schist : this suggests a small hiatus at the margin of the Limestone, and a photograph was exhibited to show this hiatus. The conclusions drawn from this section are supported by the section seen below Gilbert's Bridge, and a somewhat similar one in the Banvie Burn, north of Blair Castle. As before, there is clearly a small hiatus at the base of the Limestone.

In order to ascertain the meaning and extent of this break in the sequence, an account is next given of the complete succession near Braemar, and it is then seen that at Gilbert's Bridge the Little Limestone and part, or at times the whole, of the Dark Schist is missing. The hiatus always tends to occur as an area is approached where the material forming the Moine gneiss thickens, and was originally of a rather coarser or more sandy nature.

Where, however, the section is complete, it is seen that the material of the Moine gneisses is the flaggy margin or top of the Central-Highland Quartzite ; it is succeeded by the Little Limestone, above which is the Dark Schist, and then the Main or Blair-Atholl Limestone.

Other sections along the line of change are described, showing the varying phases of the honestones, and in two instances their passage into Moine gneiss. There is a constantly-varying hiatus at the base of the Main Limestone, but in the whole 40 miles this never exceeds the omission of the entirety of the Black Schist and the Little Limestone (of no great original thickness). This break in the sequence is of small importance, and, as already stated, often disappears as the material from which the Moine gneisses were formed became thinner and finer, or more of the nature of a mud.

Evidence is then given to show that the honestones tended to become more sandy and to thicken south-eastward again, or in the opposite direction to that in which the Moine gneisses come on. From this the author concludes that the parallel-banded material was deposited in a series of fans ; in the larger fans we have the material of the Moine gneisses ; in the smaller that of the honestones. Both are simply the flaggy top of the sandstone now forming the Central-Highland Quartzite, and are in fact a passage-rock on its margin. Anything like an unconformity between the two is obviously impossible.

Fig. 1.
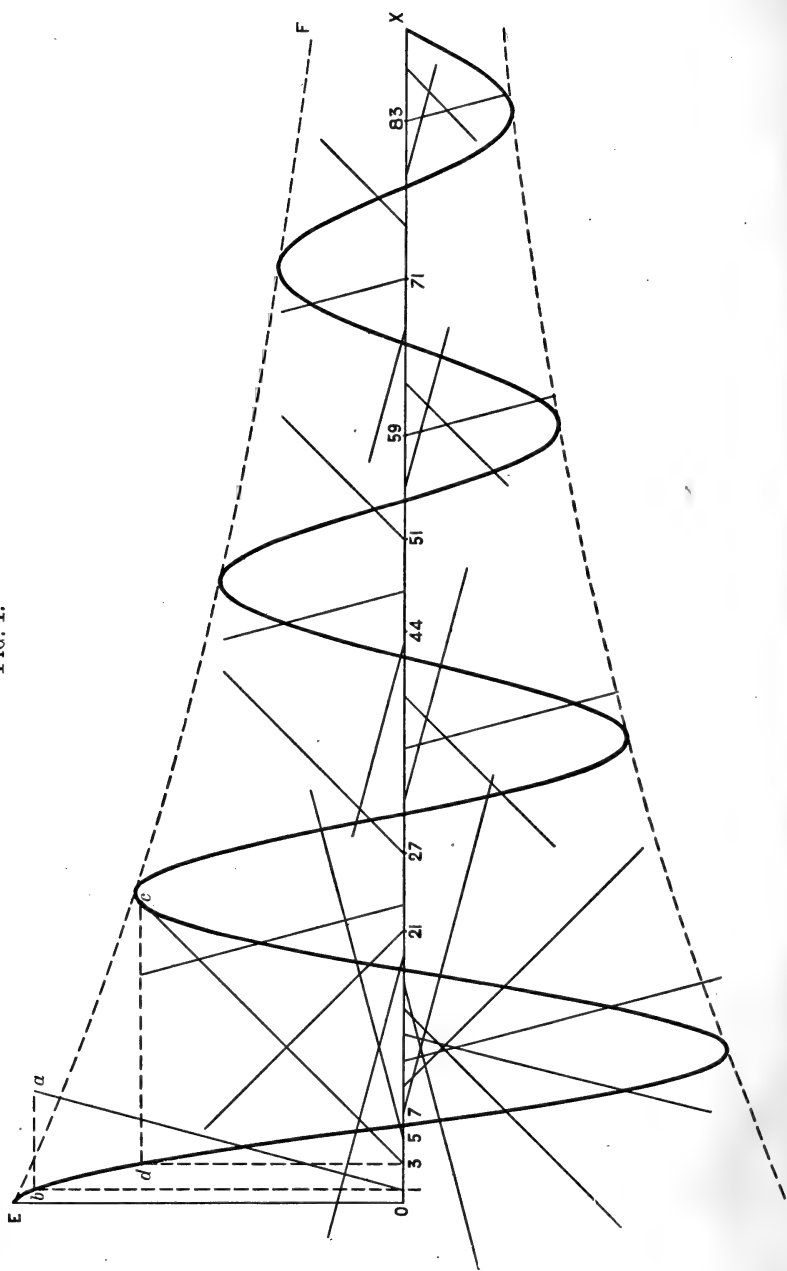
Fig. 2.
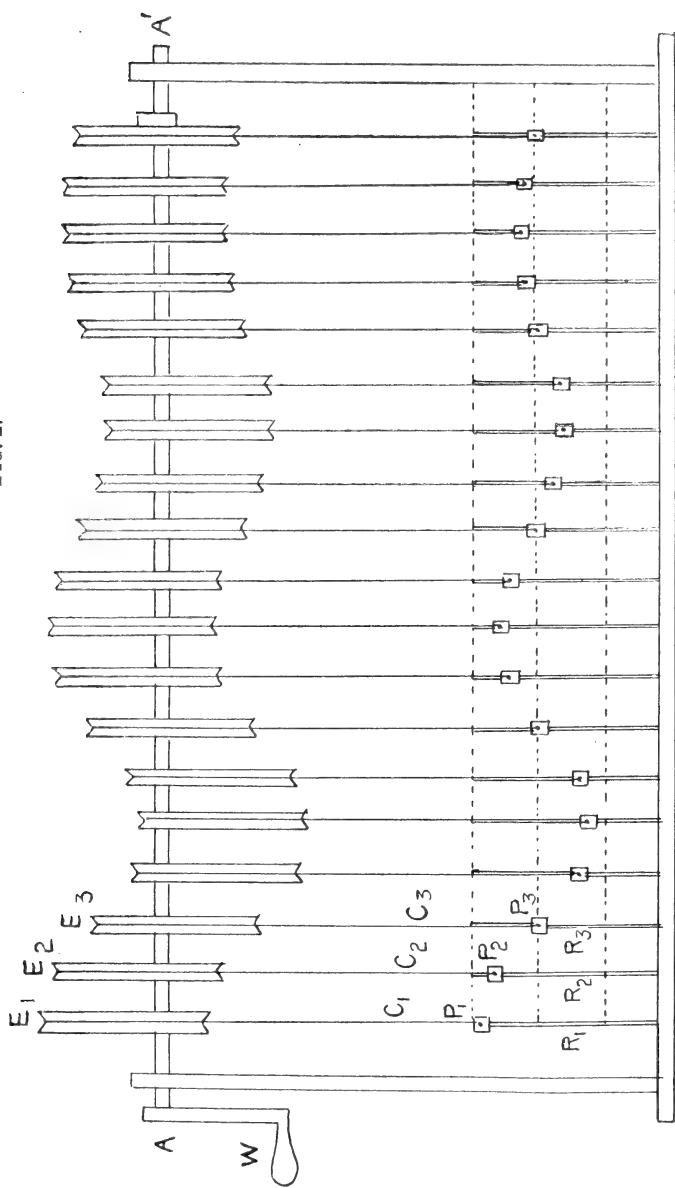
Elevation.
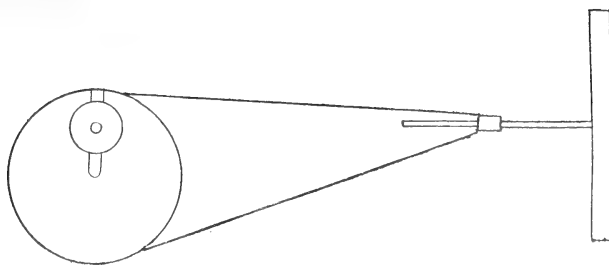
Section.
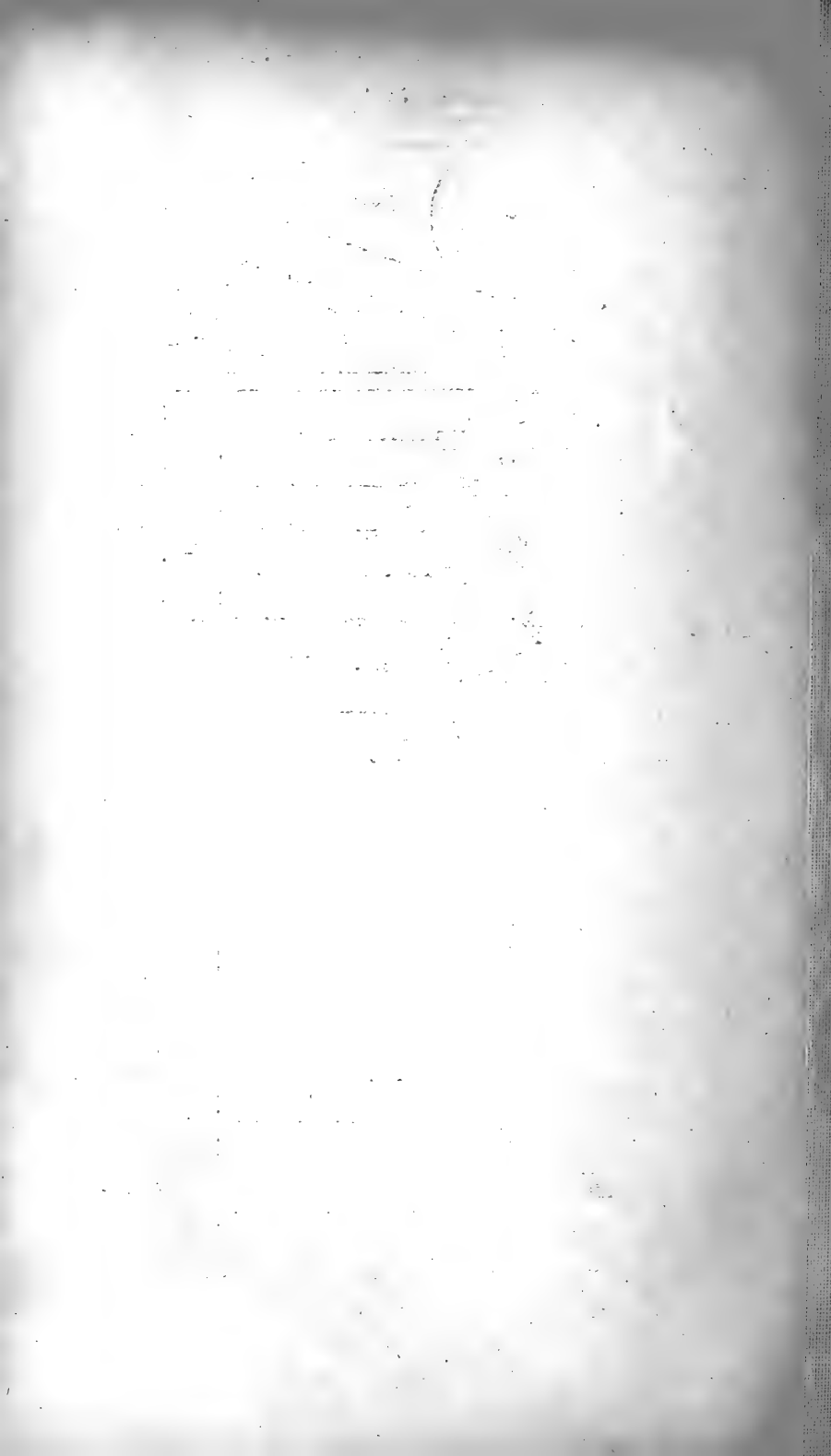
The gold foil used in the following six observations contained less than 0·1 per cent. of impurity. The photographs were taken by normally reflected light, with an objective of 0·65 N.A. The magnification is × 350.
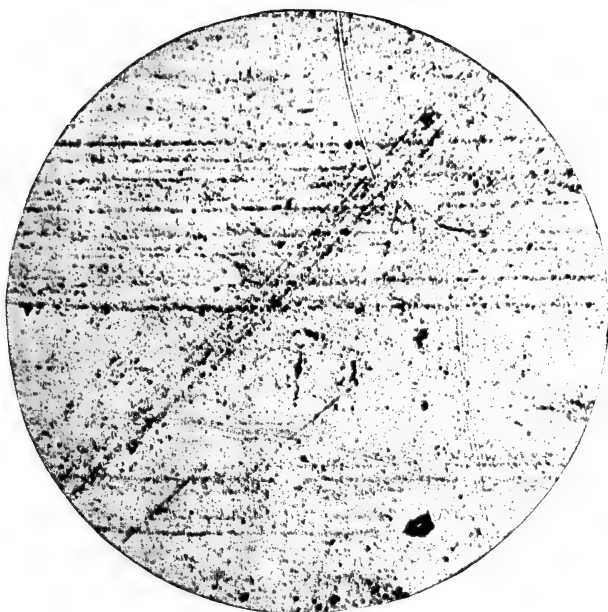
FIG. 3.—Gold foil annealed and polished by pressing on glass surface. The flow has been insufficient to cover the surface completely with a flowed layer, so that the granular structure is not obliterated.

FIG. 4.—Gold foil beaten out on polished steel anvil and lightly polished on rouged leather. The surface is completely covered with a smooth brightly-reflecting layer, free from granular or other structure.
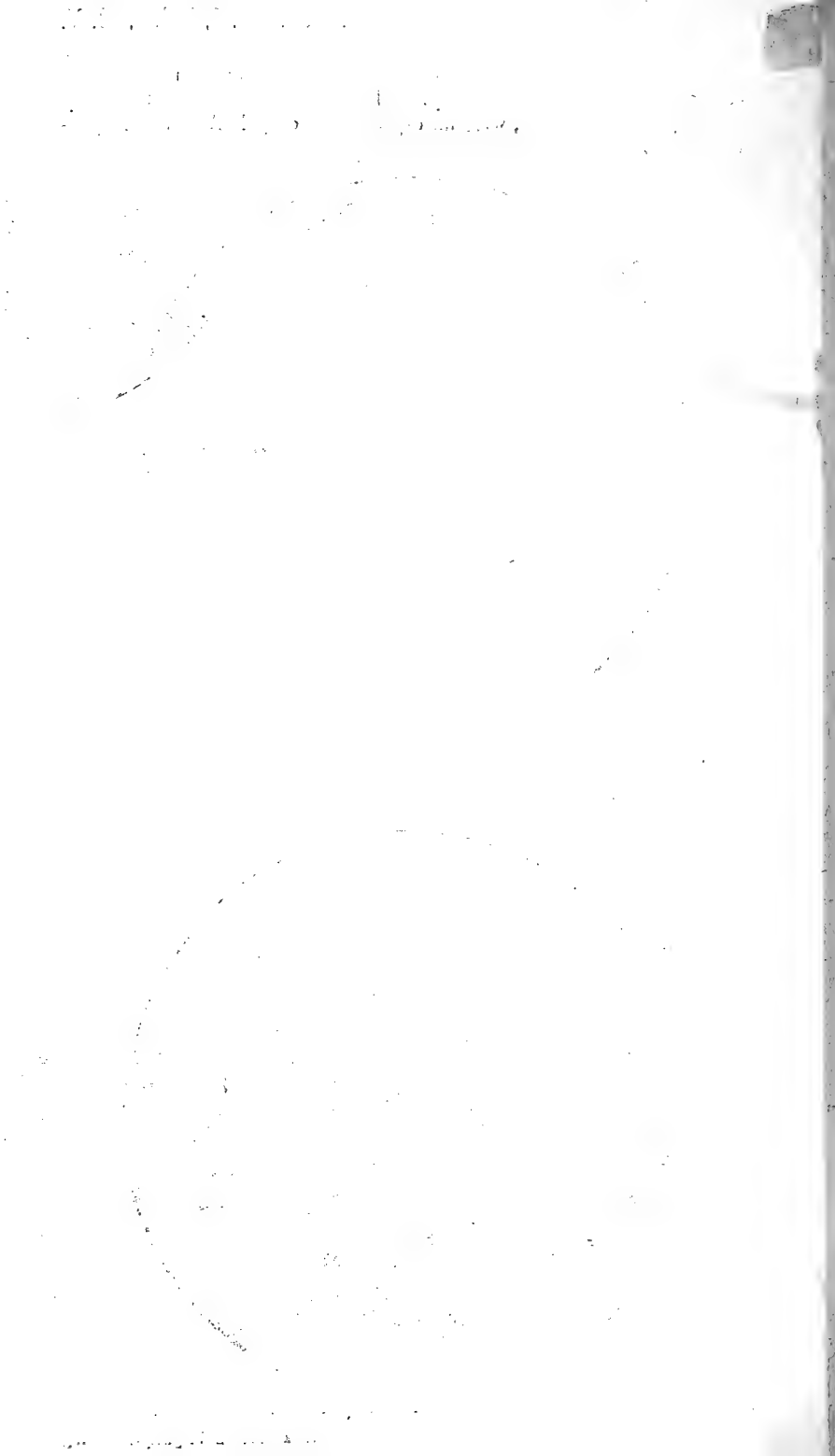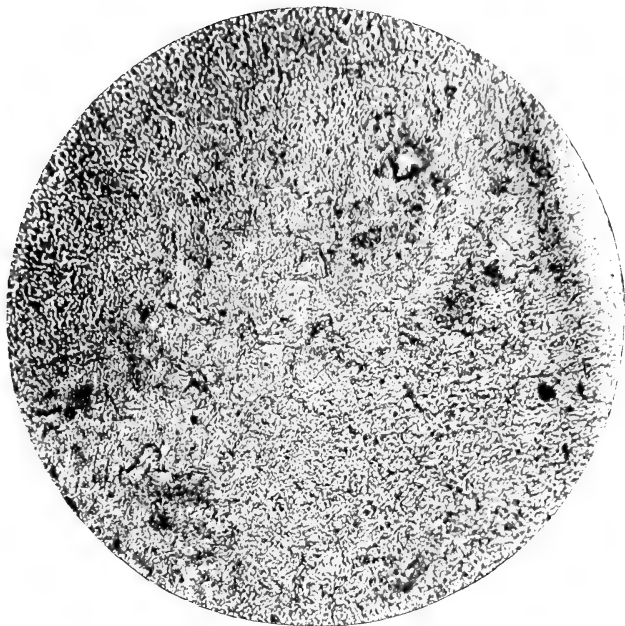
Fig. 5.—The same, lightly etched with chlorine water, disclosing granular texture immediately below the smoothly polished surface.
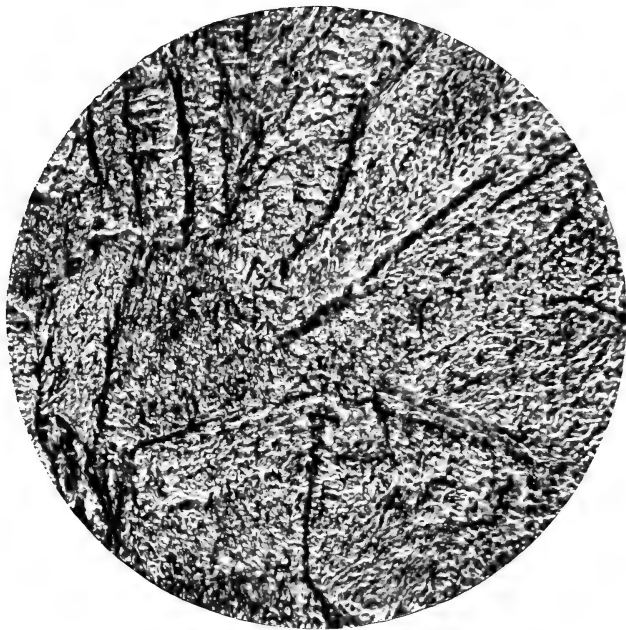
Fig. 6.—The same, close to one of its edges, further etched with chlorine water disclosing deformed grains and lamellæ immediately below the granular layer seen in Fig. 5. The breaking down of the lamellæ into granules is well seen.
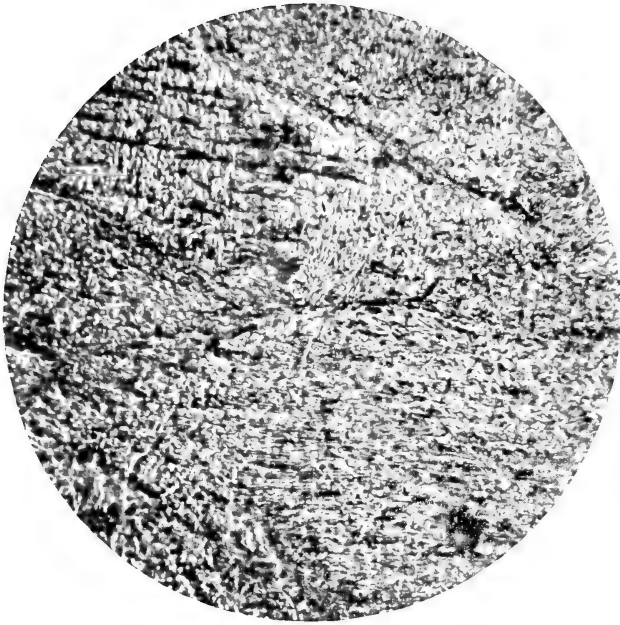
Fig. 7.—The same, at a point nearer the centre of the strip where the lamellæ are rather less distorted.
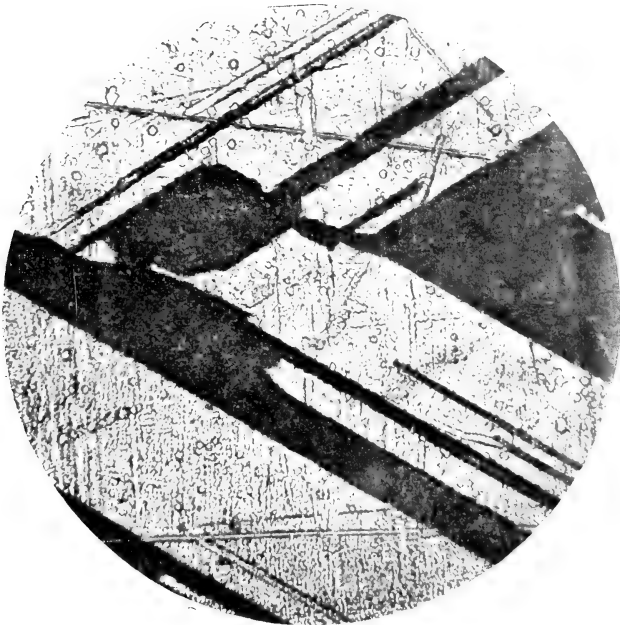
Fig. 8.—Gold foil annealed and at once etched; showing, for comparison with 6 and 7 the straight sharp outlines of undistorted lamellæ or grains.
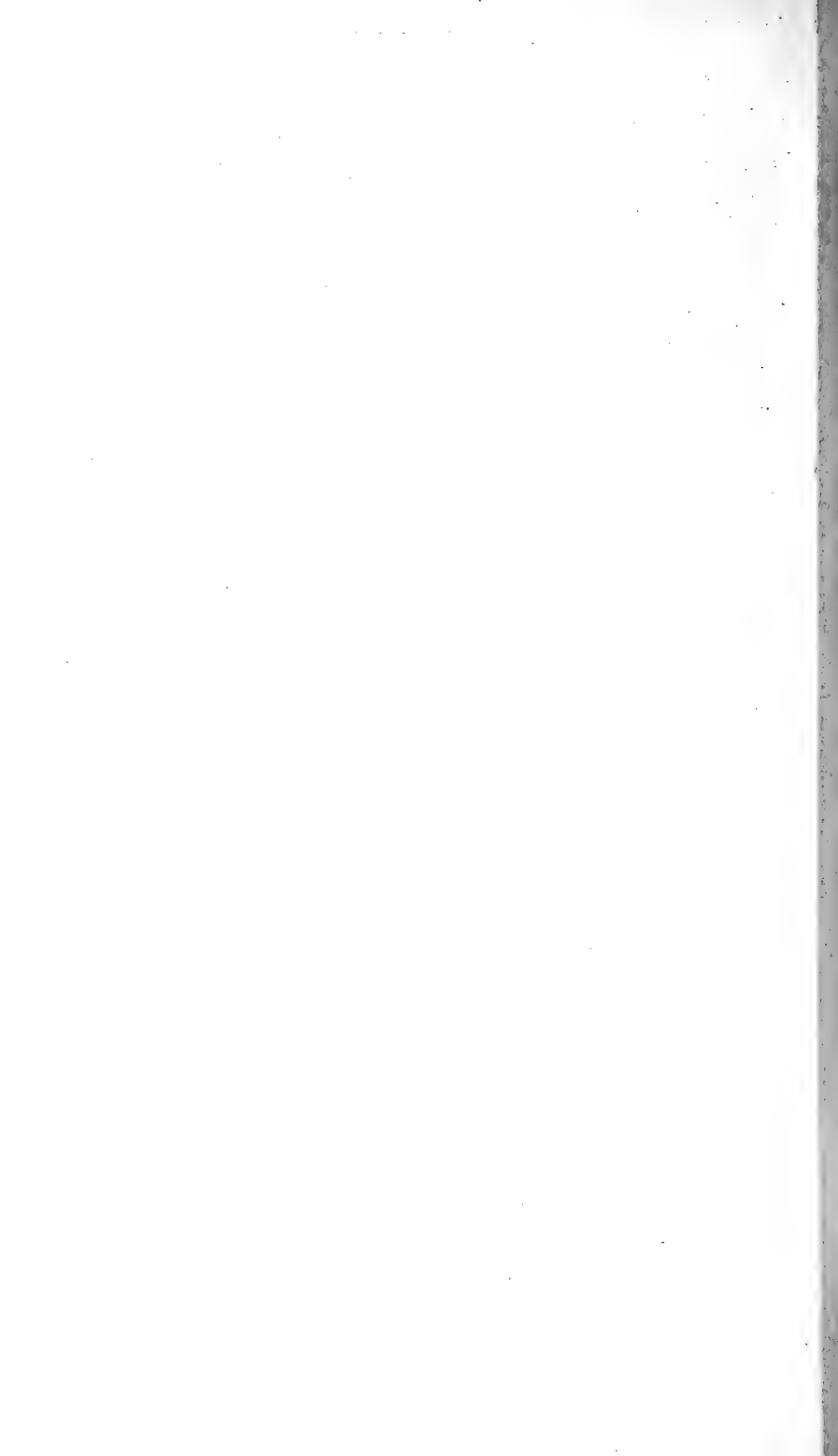
Fig. 9.—Calc-spar etched after rubbing. Transmitted light. × 215.

Fig. 10.—Antimony with emery furrows. Normal light. × 775.
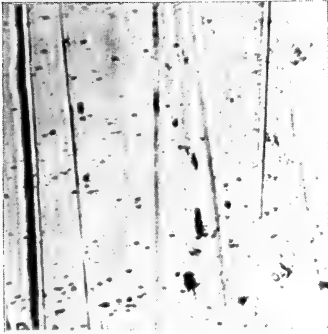
Fig. 11.—The same polished across the furrows and ridges, surface film spreading over these. Normal light. × 775.

Fig. 12.—Surface film partly removed by etching. Normal light. × 775.
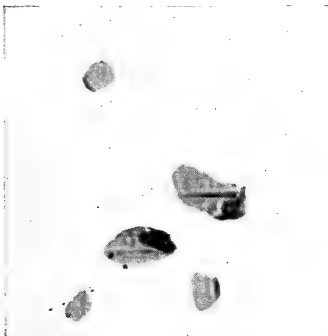
Fig. 13.—The same with pits covered by surface film. Normal light. × 775.
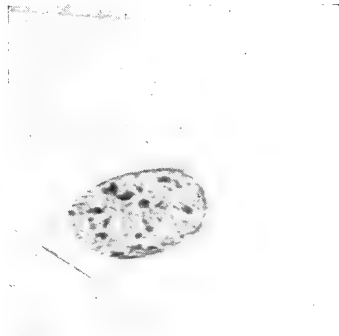
Fig. 14.—The same, large pit being covered by surface film. Normal light. × 775.
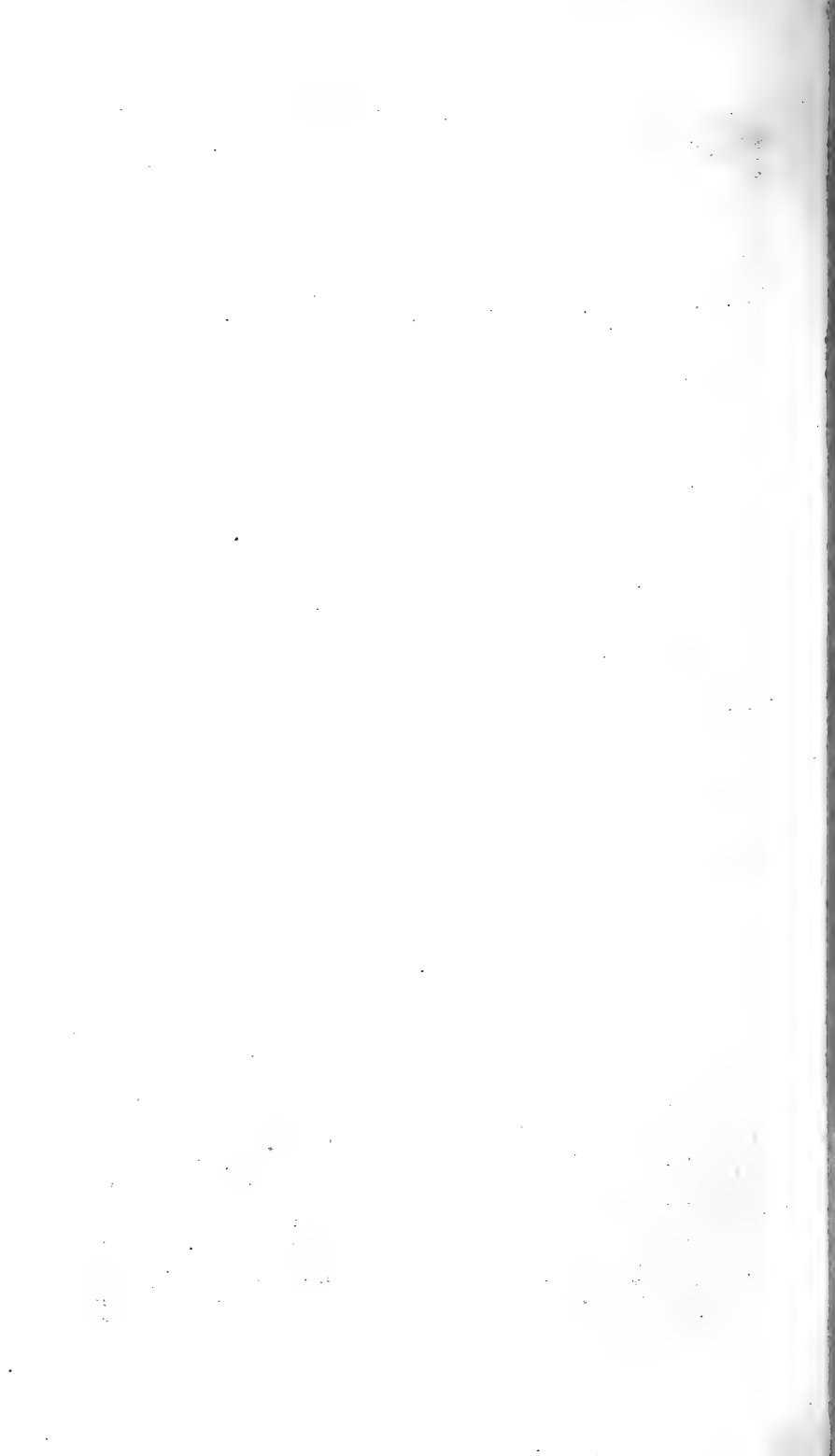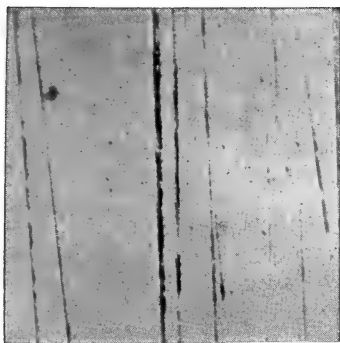
FIG. 15.—Speculum metal, polished across emery ridges and furrows. Normal light. × 775.
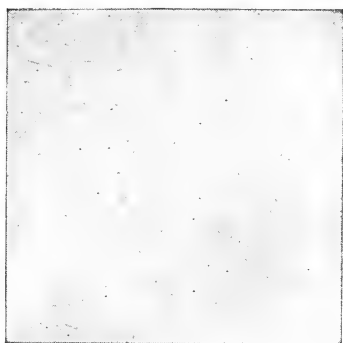
FIG. 16.—The same, surface film completely formed. Normal light. × 775.
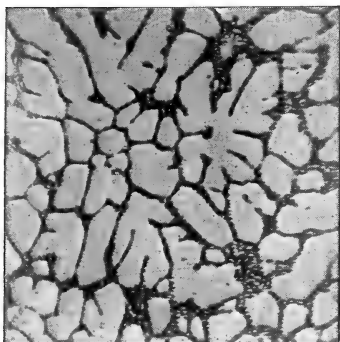
FIG. 17.—The same, surface film removed by etching. Normal light. × 775.
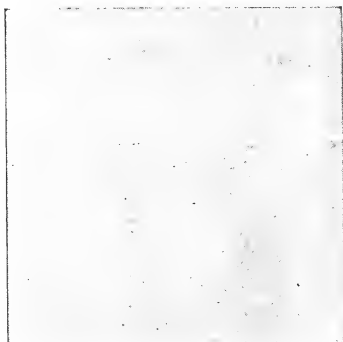
FIG. 18.—The same, surface film restored. Normal light. × 775.
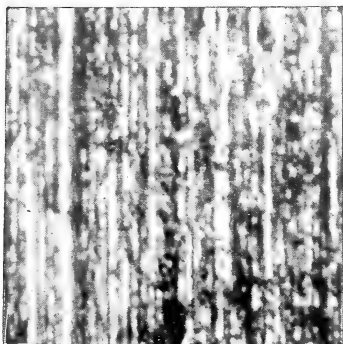
FIG. 19.—Lead, cut surface, flow lines breaking up into granules. Oblique light. × 580.
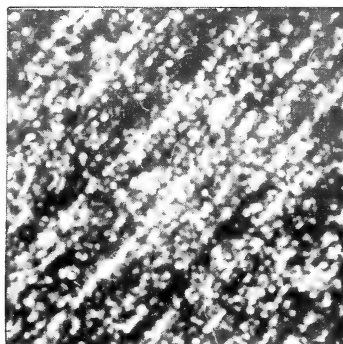
FIG. 20.—The same, lighted along the flow lines. Oblique light. × 580.
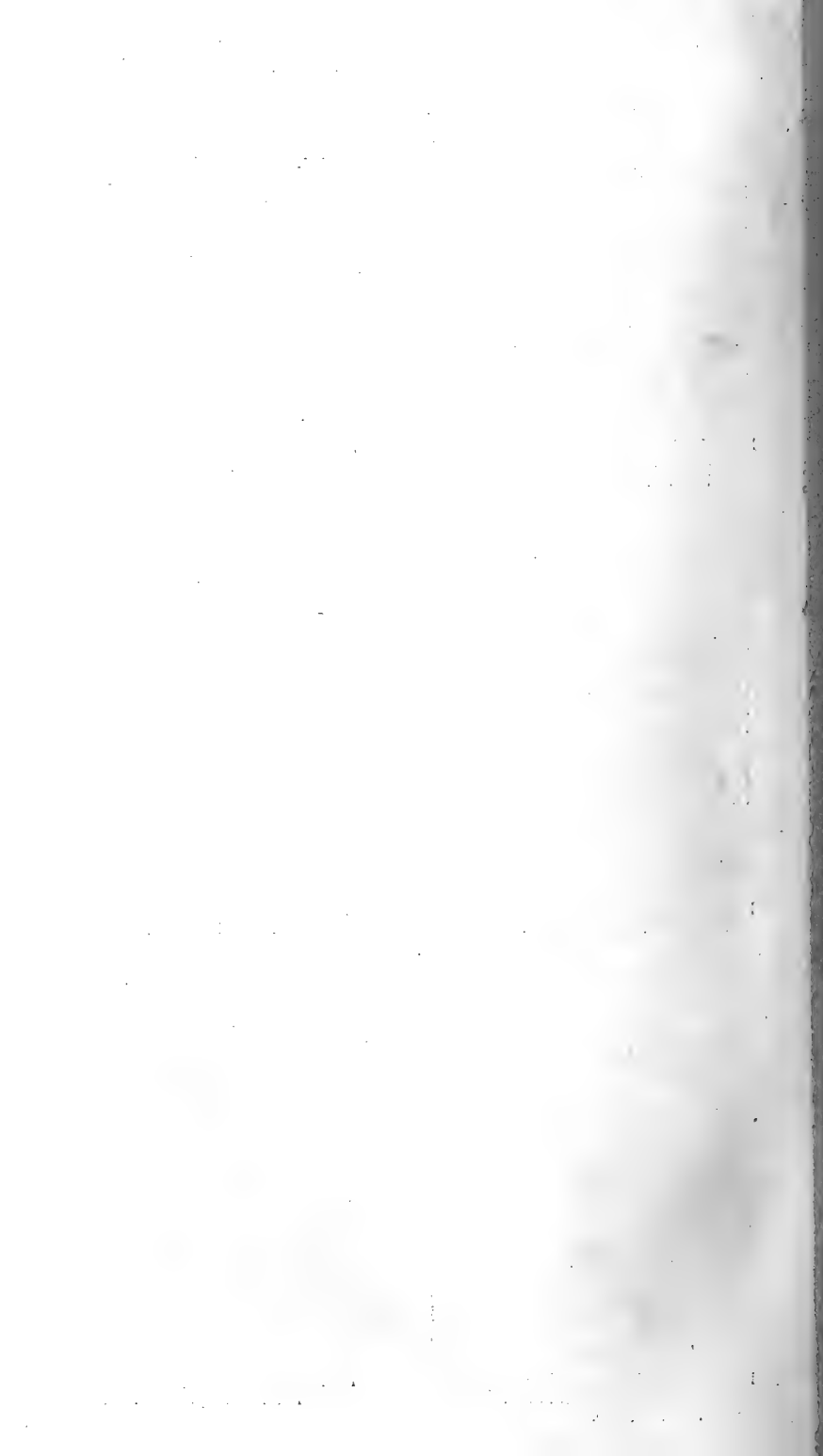
XXXII. *A Quantitative Determination of the Anomalous Dispersion of Sodium Vapour in the Visible and Ultra-violet Regions.* By R. W. Wood, *Professor of Experimental Physics, Johns Hopkins University*[*].

[Plates X.–XII.]

THE dispersion of a medium with but a single band of metallic absorption is represented by the formula

$$n^2 = 1 + \frac{m\lambda^2}{\lambda^2 - \lambda_m^2},$$

in which λ_m is the wave-length at the centre of the absorption-band, and m a constant. This formula, which is developed from electromagnetic considerations, is practically identical with the original Sellmeier formula. Up to the present time it has never been tested experimentally for the reason that no medium of suitable constitution is known which admits of accurate determinations of the variation of the refractive index with the wave-length. We possess such a medium in the vapour of metallic sodium, but the experimental difficulties which stand in our way are so great, that only qualitative observations have been made up to the present time.

The formula given above, being the simplest dispersion formula which we possess, ought by all means to be tested experimentally; and it was with this end in view that the work reported in the present paper was undertaken.

After a large amount of fruitless work, methods have at

length been developed which have made possible accurate determinations of the absolute refractive index of the metallic vapour at various densities, for all wave-lengths comprised between the extreme red and the remote ultra-violet. For wave-lengths immediately adjoining the D lines the refractive index has been found to have a value as high as 1·38, as great as that of some liquids, while the dispersion is so great even at the position of the D line of helium, that, could we form a prism of the vapour giving the same deviation as a 60° glass prism, we could by its aid separate a close Zeeman doublet by an amount as great as the distance between the red and blue of the spectrum formed by the glass prism. The results obtained have been applied to the formula, and most excellent agreement found.

The anomalous dispersion of the vapour was first observed by Kundt, and has since been studied by Becquerel, Julius, Ebert, and others. These investigators have for the most part limited their observations to the dispersion produced by flames of prismatic form, containing the vapour of sodium. Under these conditions the anomalous dispersion is only apparent in the immediate vicinity of the D lines, extending to a distance up and down the spectrum not much greater than the distance which separates the lines. Some four years ago it was shown by the writer * that by employing the vapour of the metal in glass tubes, it was possible to obtain far greater deviations, and to extend the observations on the dispersion from the extreme red to the violet. These results were made possible by the very remarkable physical properties of the vapour, which will be discussed more in detail presently. The prisms formed in heated glass tubes are in reality non-homogeneous cylinders of the vapour, the density being greatest along the heated floor of the tube and least along the top. The definition given by non-homogeneous masses of vapour is surprisingly good, and relative values of the refractive indices can be determined without difficulty; but as no means could be found of determining the angle of the equivalent prism, it was impossible to do more than guess at the absolute values. An attempt made several years ago to obtain data by interference methods gave no results, owing to the fact that the fringes were immediately obliterated by the convection-currents of heated hydrogen. By adopting the expedient of heating the metal in a high vacuum, it has been found possible to obtain deviations as high as 1500 fringe-widths with the D_3 light of helium, without in any way

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* Wood, Proc. Roy. Soc. (1901) & Phil. Mag. [6] vol. iii., "The Anomalous Dispersion of Sodium Vapour."

altering the appearance of the fringes. By combining the data obtained with the interferometer with the relative values obtained by prismatic deviation, it has been possible to obtain absolute values for a very wide range of wave-lengths. We will begin with a brief discussion of the properties of the medium.

Physical and Optical Properties of the Vapour of Metallic Sodium.

One of the chief difficulties which has stood in the way of measuring the optical constants of sodium vapour is the impossibility of confining the vapour in vessels bounded by flat surfaces of glass. As soon as the temperature is raised to such a point that vapour of any considerable density forms, the glass is attacked and rendered opaque by the reduction of silica. The vapour, however, has a most remarkable viscosity, which I am at the present time investigating, which makes it possible to form a mass of great density separated from the glass plates which close the ends of the tube by a high vacuum. If we place a number of pieces of clean sodium in a tube of hard Jena glass, the ends of which are closed with small pieces of thin plate-glass, and exhaust the tube on a mercurial pump, on heating the under side strongly with small Bunsen flames, the sodium vapour shows very little inclination to distil to the cold parts of the tube. It condenses to be sure on the upper side of the tube, but is given off so much more rapidly from the surface of the molten metal than it can diffuse to the upper portion, that the density gradient is very steep. Observations on the deviation produced by the non-homogeneous cylinder show that the equivalent prism has a form similar to that shown in fig. 1, the density gradient

Fig. 1.
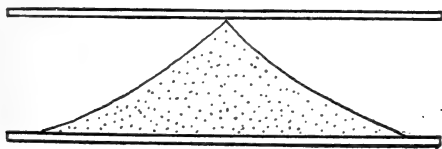
being steeper near the bottom of the tube. To secure good definition it is therefore necessary to place in front of the tube an opaque screen, perforated with a wide horizontal slit. The tube thus prepared, used in the manner to be presently described, shows the strong anomalous dispersion in the vicinity of the D lines with great distinctness. If the flames are made to play upon the upper surface of the tube,

X 2

after some of the vapour has condensed there, the spectrum straightens out, and the dispersion presently occurs in the opposite direction, showing that the base of the prism is always against the heated portion of the tube regardless of gravity.   In a tube 25 cms. long, 10 cms. of which are heated red-hot on all sides by means of an iron wire carrying a heavy current, a mass of sodium vapour may form of sufficient density to give a displacement of several thousand helium (D_3) fringes; notwithstanding this, practically no trace of the vapour can be detected beyond the heated portion.   This condition can be maintained for an hour or more, owing to the slowness of the diffusion.   If mercury is heated under similar conditions, it is impossible to obtain shifts of more than a fringe or two.   Potassium distils almost in a flash to the colder parts of the tube.   The other alkali metals have not as yet been investigated.   The vapour of sodium appears to possess a cohesion almost as great as that of a liquid.   Even in a vacuum-tube it appears to have a free surface, while in an atmosphere of hydrogen it can be dipped up on a wire, a vigorous shaking of which is insufficient to dislodge it or cause its dissipation. A number of very interesting experiments with the vapour have already been made, and others are in progress at the present time.   They will be reported in a subsequent paper, as for the present we are only concerned with the fact that a homogeneous cylinder of very dense vapour terminated with sharply defined ends can be formed in an exhausted tube.

Anomalous dispersion occurs at the absorption-lines which belong to the principal series, being very strong at the D lines, feeble at the first pair of ultra-violet lines ($\lambda = 3303$) and almost imperceptible at the second pair ($\lambda = 2852$), as is shown in fig. 2.   If the vapour is of considerable density,

Fig. 2.

the D lines run together into a single absorption-band, and the dispersion can be represented throughout the complete range of wave-lengths as resulting from electrons of a single period, for the ultra-violet bands only affect the refractive index in their immediate vicinity.   These bands make their appearance in succession as the density of the vapour increases, the second not appearing until the band at the D lines

has become three or four times as broad as the distance between the lines. The complicated channelled absorption spectrum appears to exert no influence on the refractivity of the vapour. I feel certain, however, that the channelled absorption spectrum affects the refractivity of the medium in a small degree, and intend to test the matter in the near future by a modification of the method of striæ. The channelled bands affect the magnetic rotatory dispersion in a powerful degree, and this subject is under investigation at the present time. The magnetic dispersion is many times greater than the ordinary dispersion, as has been found by examining the vacuum dispersion-tubes in a powerful magnetic field with polarized light, and the whole subject will be fully taken up in a subsequent paper.

The absolute values of the refractive index for different densities have been determined with the interferometer for monochromatic light of different wave-lengths. Relative values (dispersion) have been obtained in the same way, as well as by the method of crossed prisms.

Determination of the Dispersion with the Interferometer.

The absolute determinations being the more difficult, it was decided to begin with an investigation of the dispersion, in order to get as much experience as possible with the behaviour of the vapour under various conditions. This was a wise decision, as was subsequently found, for a number of very elusive sources of error were discovered, which were eliminated in the final work.

The general plan of the apparatus is shown in Plate X. The interferometer was the form of the Michelson instrument designed by Brace.

The base of the instrument consisted of a heavy tube of brass 12 cms. in diameter and a little over a metre long. The arrangement of the mirrors is shown in the diagram, all but one being of speculum metal. Light from an arc-lamp was focussed on the slit of the monochromatic illuminator. The beam issuing from the slit of this instrument comprised a range of wave-lengths about equal to the range between the D lines. Near the slit was mounted a helium tube, which furnished a light of constant wave-length, and the observations consisted in counting the fringe-shifts for the two kinds of light when a given amount of sodium vapour was introduced into the path of one of the interfering beams. A small mirror was mounted in front of the interferometer, which deflected a portion of the emergent light into a telescope, the remainder of the beam entering a second telescope. By a

little adjustment it was possible to get the fringes formed by helium light distinct in one telescope, and those formed by the light from the monochromatic illuminator distinct in the other. The helium tube standing a little to one side of the slit, very little overlapping of the two systems occurred.

The sodium was contained in a seamless steel tube, covered with asbestos paper and wrapped with a helix of iron wire through which a heavy current was passed. The ends of the tube were closed with plates of glass, which were of such a thickness that they served in place of the compensating plate which is usually placed parallel to the transparent mirror. The tube was exhausted by means of a mercurial pump, with which it was kept in connexion throughout the experiments in order to remove the occluded hydrogen which was given off as soon as the sodium was heated. This occluded gas gave a good deal of trouble, and the behaviour of the fringes seemed to indicate that a small amount of the gas became entangled in the sodium vapour, no amount of pumping appearing to remove it entirely. Its effect on the fringe-shift will be discussed presently.

The D_3 line of helium was so strong in the tube used that no colour-screens were necessary, the fringes being as distinct as those obtained with sodium light. Owing to its proximity to the absorption-band, very large fringe-displacements were obtained with a small amount of comparatively rare sodium vapour.

In the electrically-heated tubes the sodium vapour showed no tendency to form the non-homogeneous equivalent of a prism, and the fringes consequently remained straight, even after very great displacements.

Two observers were necessary ; one counting the helium fringes as they passed the cross-hair of the telescope, and the other noting the transit of the fringes formed by the light from the monochromatic illuminator.

If the light used was on the same side of the D lines as the helium line, the two sets of fringes were shifted in the same direction; if on the other side, the shifts were in opposite directions. In addition to comparing light from the mono-chromatic illuminator with the helium light, a very careful comparison was made with the green mercury line, since this light was to be used in the absolute determinations.

At first very discordant results were obtained, but the source of error was soon discovered to lie in the heating and conse-quent expansion of the base-bar of the interferometer by the radiation from the sodium tube. The instrument was then

covered with layers of cotton and asbestos board, and consistent results were at once obtained. There was still a source of trouble which for a long time remained undiscovered. On heating the tube it was found that the relative shifts changed as the temperature rose, becoming constant after a shift of about a dozen helium fringes. Repeated heating of the sodium to a high temperature, combined with pumping, lessened the discrepancies, but the trouble was never completely eliminated. It is now believed that a small amount of hydrogen is held captive by the sodium vapour, condensing with it as the tube cools. When the tube is again heated, this occluded hydrogen is evolved with the first traces of sodium vapour, and modifies its refractivity. If we are working with light on the same side of the D lines as the helium line, for which the refractive index of the vapour is less than unity, the evolution of this gas will oppose the displacement of the fringes, holding both sets back by practically the same amount. Suppose we are working with a wave-length for which the relative shift with respect to helium light is 1 : 10. If we form a sufficient quantity of sodium vapour to shift the helium fringes through 30 fringe-widths, and the other set 3 widths, and an amount of hydrogen sufficient to give a shift of one fringe-width has been set free, it is obvious that our observed ratio will be 2 : 29, or in round numbers 1 : 15. The errors observed were always at the beginning of the series when the tube was heated, and at the end when it was cooled, and were always of such a nature as to be explained perfectly by the hypothesis which I have advanced. On this account we always disregarded the relative values found when the sodium vapour was only present in very small quantities.

Many weeks of work were necessary before all sources of error were eliminated and the best methods of working discovered. As a sample of one of the troubles met and overcome, the following will serve. When working with light from the monochromatic illuminator of a wave-length very near that of the D lines, the fringes disappeared before shifts of more than one or two fringe-widths were observed. It had been our habit to set the mirrors of the instrument in such positions that the centre of the fringe system was seen in the telescopes. The disappearance was finally found to be due to the dispersion of the vapour, and by turning the screw of the instrument in such a way as to increase the length of the air path, they appeared again *. The most

* This action of the vapour on the fringes will be discussed in the paper following the present one (p. 324).

advantageous way to work was therefore to begin the observations with the maximum air path consistent with visibility of the fringes ; as the sodium vapour formed they increased in distinctness at first and then gradually faded away. In this way it was possible to get a shift twice as great as before ; if it was desirable to work with a denser vapour to eliminate the error due to evolved hydrogen, we had only to increase the length of the air path still further, and work further out in the system. Under these conditions the fringes are invisible at the beginning and end of the experiment, only coming into view when a considerable quantity of sodium vapour is present. The same thing is true to a less extent with helium light, the fringes disappearing after a shift of about 200 fringe-widths has taken place. By increasing the length of the air path, either by turning the screw of the instrument, or introducing one or more thick plates of plane-parallel glass, they again come into view, and the counting can be continued. The latter method was preferred, as the glass plate could be dropped into place in a moment without interrupting the count. This trick was only necessary in making the absolute determinations with helium light, as in the comparison work the vapour never attained such a density as to cause the helium fringes to disappear. When working close to the helium line with the monochromatic illuminator it was necessary to work with very narrow slits, otherwise the enormous dispersion of the vapour obliterated the fringes almost immediately, in spite of all precautions. It was impossible to approach closer to the absorption-band than the helium line, owing to this trouble, and ratios of $1 : 2$ are about as large as we feel much confidence in, though on one or two occasions we set the monochromatic illuminator on the D_3 line, and obtained equal shifts in the two systems. It was impossible to observe a shift of more than one or two fringes in this case before the system disappeared. The most accurate determinations are doubtless those where the ratio is between $1 : 3$ and $1 : 15$. In the blue-green region the ratio is in the neighbourhood of $1 : 40$, and the values are not as accurate, as an accidental displacement of half a fringe-width, which may easily occur as the result of a slight change in the temperature of the instrument or the air, exercises a much greater effect on the results. On this account the determinations made by the prismatic method are to be considered more reliable in this region of the spectrum.

Determination of the Dispersion in the immediate vicinity of the D Lines.

The extraordinarily high value of the dispersion close to the absorption-band makes it impossible to sift out from the spectrum, light sufficiently homogeneous to yield interference-fringes when even a very small amount of sodium vapour is introduced into the path of the light. It was found difficult to get satisfactory values with the light from the monochromatic illuminator much closer to D_3 than the distance separating D_3 from D_2, the fringes becoming blurred as soon as the smallest trace of sodium vapour was formed. A different method was accordingly adopted which yielded excellent results.

It is clear that if we employ light of two different wavelengths in illuminating the interferometer, the introduction of the metallic vapour will shift the two fringe systems by different amounts, the fringes appearing and disappearing as they get into and out of step in succession. If both systems are shifted in the same direction, which is the case when both wave-lengths are on the same side of the absorption-band, we can measure the dispersion by counting the number of fringes which pass the cross-hair of the telescope between two successive disappearances. If, for example, the number is found to be 15, we know that one set of waves is retarded or accelerated 15 wave-lengths, under the same conditions which cause a shift of 16 fringes for the other waves. All uncertainty as to the movement of the fringes during a period of minimum visibility was avoided in the method employed, as will appear presently.

To determine the dispersion close to the D lines we require highly homogeneous light of two different wave-lengths, which would yield a close double line at the point at which we wish to measure the dispersion.

This was accomplished by breaking up the radiation of a helium tube into a Zeeman triplet and cutting out the middle component by means of a Nicol prism placed with its plane of vibration perpendicular to the lines of magnetic force. A large Ruhmkorff magnet with an ammeter in circuit was employed, the key for making and breaking the circuit being placed close to the observing telescope of the interferometer. The distance between the components of the double line obtained when a current of 30 amperes was flowing through the magnet was found to be $1/23$ of the distance between the sodium lines. This was determined very easily by counting the number of turns of the interferometer-screw necessary to cause the fringes to pass through a given number

of phases of maximum visibility, the method being the one commonly employed in the laboratory in the determination of the relative wave-lengths of the sodium lines. The magnetic field was now thrown off, and the temperature of the heating coil gradually raised. As the sodium vapour formed, the fringes were counted as they drifted across the field. The current was thrown into the magnet at frequent intervals for a fraction of a second, and the fringes were seen to pass through successive phases of visibility and invisibility. The momentary disappearance caused by the magnetic field did not interfere with the counting, for the fringes moved slowly and with the regularity of clockwork. The exact moment at which the fringes disappeared could not of course be accurately determined, since they remained invisible during a period occupied by a shifting of several fringe-widths, but by observing six or seven successive disappearances values were obtained which did not differ by more than two or three per cent. The average of a number of readings showed that the sodium vapour shifted the two sets of fringes in the ratio of 10·5 to 11, that is fringes formed by waves of length 587487 are shifted 10·5 fringe-widths, while those formed by waves of length 587513 are shifted 11 fringe-widths. These numbers do not look very formidable until we translate them into prismatic deviations. A prism having the same dispersion giving a total deviation of only 11 degrees would separate the two components of the Zeeman triplet half a degree, or if we could construct a sodium-vapour prism giving the same deviation as a 60° glass prism, two lines in the spectrum twenty-three times as close together as the D lines would appear separated by a distance greater than the distance between the red and the bluish green of the spectrum formed by the glass prism.

The same method was applied to a determination of the dispersion between the yellow mercury lines, for the purpose of obtaining a check on the curve obtained by comparing the shifts of the helium fringes with fringes formed by the light from the monochromatic illuminator.

A prism was placed in front of the observing telescope to separate the yellow fringes from the more intense green ones. On setting them in motion by forming sodium vapour in the path of the light, they were found to disappear periodically in the same manner. It was impossible to get as accurate data as were obtained with the Zeeman lines, since there were moments of complete invisibility, but by counting steadily it was possible to tide over the few seconds when the field was a blank and get a fair estimate of the relative shifts.

A number of observations were made, the mean of which gave 5 : 6 as the ratio of the shifts of wave-lengths 5770 and 5790. These values fitted almost exactly on the dispersion-curve obtained with the monochromatic illuminator and the helium tube. Obviously a more accurate method would have been to provide a device by which the light of one of the yellow lines could have been cut off as desired. An attempt was made to do this with the monochromatic illuminator, but the loss of light was too great.

Some very curious results were obtained by illuminating the interferometer with sodium light. The fringes disappeared at the moment when the thermo-element indicated a temperature of 180°. It was at first thought that the disappearance was due to absorption of the light by the vapour in one of the two paths, but on covering up the other path it was found that the field of the instrument was still brightly illuminated. On turning the screw of the instrument the fringes appeared again, and a little further experimenting showed that the effect of the vapour was to alter the visibility curve, its periodicity becoming less as the density of the vapour increased. No satisfactory explanation of the altered appearance of the fringe system could be found, and an attempt was made to get rid of one of the D lines in the illuminating light, thus simplifying the conditions. Prisms and gratings were tried, but the loss of light was so great, that even with an oxyhydrogen sodium flame the fringes could be made out only with the greatest difficulty. The desired result was finally obtained by mounting a quartz crystal between two crossed nicols. A number of crystals were examined with polarized light perpendicular to the axis, and one selected which showed dark bands in the spectrum separated by a distance about double the distance between the D lines. The nicols were arranged so that the emergent light vibrated in such a plane as to be most copiously reflected by the interferometer mirror, *i. e.* the first nicol was mounted with its short diagonal horizontal, while the one next to the instrument was placed with its long diagonal horizontal. Between the nicols was mounted the quartz crystal with its axis making an angle of 45 degrees with the vertical. The light from an oxy-hydrogen sodium flame, after passage through the polarizing system, was brought to a focus on a cardboard screen by means of a lens, forming a series of bright bands separated by dark intervals. A narrow slit in the screen allowed light corresponding to D_1 or D_2 to enter the interferometer, according as it was set on one edge or the other of one of the bright bands. This arrangement worked admirably, and yielded an abundance of light. No

trace of the periodic invisibility was found in the fringe system, such as is always observed when ordinary sodium light is used.   As soon as the smallest trace of sodium vapour was formed in the tube, the fringes were found to have become quite invisible in certain parts of the system. The visibility curve changed rapidly as the density of the vapour increased, the period becoming smaller.   The general behaviour of the fringes did not differ materially from that observed when both sodium lines were used, proving that the presence of the two wave-lengths was not essential.   The appearance of fringes under the conditions of the experiment depends upon the fact that the emission-line is broader than the absorption-line.   The absorption is restricted to the centre of the line, the edge light only getting through the sodium tube and forming fringes.   This edge light is most powerfully dispersed by the vapour, and the two fringe systems which it forms are shifted in opposite directions.   It was hoped that a method might be worked out for determining the dispersion of the vapour in the immediate vicinity of the D lines, by studying the behaviour of sodium light when dispersed in the interferometer, and a great deal of time was lost in the endeavour to unravel the very complicated changes produced in the appearance of the fringe system.   If we were dealing with strictly homogeneous radiations, lying to the right and left respectively of the absorption-band, the two fringe systems would move in opposite directions at the same rate, and we should have simply rapid alternations of visibility and invisibility.   By the simultaneous use of a helium tube we could determine the number of disappearances of the sodium fringes corresponding to a shift of one or more helium fringes, which would give us at once a measure of the shift of the system formed by the sodium light.   This would give us a measure of the refractive index closer to the absorption-bands than we could get by any other method. An attempt was made to utilize the Zeeman phenomenon in this case, the sodium flame being placed in a magnetic field and the interferometer illuminated with one or the other of the D lines, by means of the polarizing system.   There was no difficulty in finding positions of the interferometer mirror for which the fringes were visible with the field on, and invisible when it was off ; but nothing resembling a rapid appearance and disappearance of the fringes was observed when the sodium vapour was formed.   This is probably due to a number of causes.   In the first place, the light from the flame is by no means strictly homogeneous, for a pale flame, containing only a little sodium and yielding narrow lines,

does not give light enough. In the second place, as is well known, the D lines do not break up into polarized triplets in the magnetic field, but into a quadruplet and sextuplet. At the time of trying the experiment this circumstance was not thought of, and the failure may have been due in part to the fact that only D_2 was utilized, which is obviously less suited to the purpose, for the reason that it gives us a double line on each side of the absorption-band when the central components are cut out by the nicol. I am of the opinion, however, that the finite breadth of the normal line is chiefly responsible for the failure. Some experiments were made with vacuum-tubes containing sodium vapour, which yield highly homogeneous light, but they were found troublesome to manage. It is quite possible that by the use of these tubes, results could be obtained in the case of D_1, and utilizing the light emitted in the direction of the lines of force (Zeeman doublet) we might get data for both lines. This experiment would have been tried were it not for the fact that almost as satisfactory results can be obtained by the method of prismatic deviation, with a much less expenditure of time.

As a discussion of the modifications of the appearance of the fringes formed by sodium light, by the introduction of the vapour of sodium into the path of one of the interfering beams of light, has no especial bearing upon the determination of the dispersion of the vapour, it will be postponed for the present. A fuller discussion will be given in a subsequent paper on " The Achromatization of Fringes formed by approximately homogeneous light by highly dispersing media."

Prismatic Determination of the Dispersion in the immediate vicinity of the D Lines.

As I have already shown, prisms of excellent defining power can be formed by heating metallic sodium in highly-exhausted tubes of hard glass. In my earlier experiments, in which the metal was heated in an atmosphere of hydrogen, the definition was not good enough to obtain a smooth curve close to and between the D lines by the method of crossed prisms. The photographs illustrating my first paper, in which the dispersion between the lines was shown, were taken by employing a prismatic flame of hydrogen burned at a platinum jet, and fed with the vapour of sodium. By using vacuum-tubes heated by very small flames far better results can be secured, and perfectly smooth sharp curves obtained. Two different methods were used in the determination of the deviation produced by the prism for wave-lengths close to the D lines.

A reticulated mesh was ruled on a small plate of glass with one of the Rowland dividing-engines. This plate was mounted in the eyepiece of the spectroscope on the slit of which the image of a horizontal slit was thrown, after dispersion by the sodium tube. The spectroscope was provided with a plane grating ruled with 20,000 lines to the inch, which gave a very brilliant 2nd order spectrum. There were four squares of the reticulated mesh between the D lines, and nine squares between D_2 and D_3.

As the temperature of the sodium tube was raised, the relative deviations in the different squares of the mesh were noted. The light immediately adjacent to the D lines was deviated entirely out of the field of the telescope before any measurable deviation at the helium line occurred. At a temperature at which the spectrum at the helium line was deviated the width of one square, the wave-lengths between the D lines, and for a distance of two squares to each side of them, were lost by absorption. By means of step by step measurements, taken at different temperatures, it was possible to get a very good idea of the dispersion curve within this very

Fig. 3.
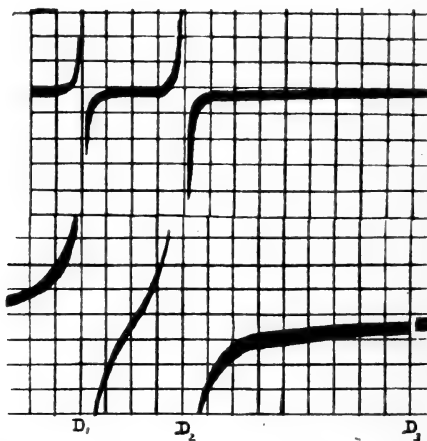
narrow range of the spectrum. The appearance of the spectrum on the reticulated background at two different temperatures is shown in fig. 3, the dispersion at D_2 being somewhat greater than at D_1. Observations were taken of the dispersion within a quarter of a square of D_2, that is within a distance of the absorption-line equal to 1/16 of the distance between the D lines. The deviation at this point was calculated to be about

80 times the deviation at the helium line. If we number the wave-lengths at the lines of the mesh between D_3 and D_2 1 to 9, the observations were as follows :—The light very close to D_2 (1/4 of a square distant) was seen to be distinctly pulled down through a distance of seven squares with a vapour-density sufficient to deviate the spectrum at the first line ($= 5888\cdot3$) through a distance of one square. The temperature was then raised a trifle. The light very close to D_2 now disappeared, partly as a result of absorption and partly through being deviated out of the field. The spectrum at the first line was deviated through three squares, that at the second line one square. The temperature was now raised until the spectrum at the 9th line was deviated through one square. The deviation at the 2nd was now four, that at the 3rd three squares, and that at the 5th two squares.

These results for the different wave-lengths between D_3 and D_2 are given in the following table :—

		Deviation.
D_3	5875.......................	1
	5881·6	2
	5885	3
	5886·6	5
	5888·3	12
	5889·6	84
D_2	5890	—

These values are of course only approximations, the errors becoming greater as we approach D_2. The same experiment was repeated, using a filar micrometer in the eyepiece in place of the reticulated mesh, and values obtained which agreed fairly well with those given above. This method, however, did not admit of such a near approach to D_2 as the other.

From these results it is possible to calculate the values of the refractive index within this range for the very dense vapour used in the absolute determinations. These will be called observed values, and they will be compared with the values calculated from the dispersion formula for the corresponding wave-lengths.

I have already spoken of the enormous dispersion at the helium line, a dispersion great enough to separate a Zeeman doublet by an amount equal to nearly the width of the entire visible spectrum produced by a glass prism of 60°, provided we were able to construct a prism of sodium vapour giving the same mean deviation, and get the light in question through it. Great as is this dispersion it is as nothing when compared

with that within say an Ångstrom unit of one of the D lines, as an inspection of the above table indicates.

The dispersion within the range between D_2 and D_3 is shown graphically in Curve 1, Pl. XI., ordinates and abscissæ being inside the small rectangle.

If this curve were completed up to $\lambda = 58896$, at which the largest recorded deviation occurs, the point in question would be at the 76th square below the unity line. To include this point it would have been necessary to take the ordinates of the curve on about 1/4 of the scale employed.

Visual Observations of the Dispersion in the Visible Spectrum by the Method of Crossed Prisms.

As a check on the results obtained with the interferometer, a series of measurements of the dispersion was made by the method of crossed prisms. The method was identical with that described in my previous paper, except that the metal was heated in a highly exhausted tube of hard Jena glass instead of in a tube filled with hydrogen. These tubes can be very easily prepared, and give no trouble at all. They are, however, apt to crack if reheated, after having been allowed to cool. It is not necessary to have the sodium in separate small lumps, as the action is not the formation of a number of prisms of sodium vapour, but a cylinder of vapour, very dense along the heated floor of the tube, and decreasing to very nearly zero along the top. If it is desired to prepare a tube for purposes of demonstration, a tube of thin steel is preferable, as it can be used over and over again. In this case it is necessary to lay a thick pad of wet asbestos-paper along the top of the tube, since the more uniform heating which results from the better conductivity of the steel is detrimental to the formation of the non-homogeneous cylinder of vapour, which only takes place when the temperature gradient between the top and bottom of the tube is very steep. The plate-glass ends are fastened on with sealing-wax, a small glass tube being sealed in in the manner described in my previous paper, through which the air is exhausted. (See fig. 4.)

Fig. 4.
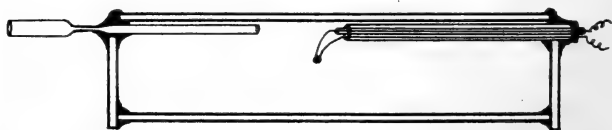
Glass tubes were used in making the measurements, as there was no occasion for cooling the tube and reheating it,

and the temperature gradient takes care of itself in this case. The sodium was first melted in a small iron crucible and poured into an improvised sodium press; a piece of apparatus so easily made and so useful that a brief description of it may not be out of place. A steel cylinder is bored with a hole two or three cms. in diameter to within a few millimetres of its end. A 3-mm. hole is then drilled through the end at the centre of the larger hole. A solid steel piston is turned so as to fit easily into the bore of the larger cylinder. The molten sodium is poured in, allowed to cool, and the piston inserted. The whole is then put into the jaws of a large vice, and the sodium wire squeezed out, directly into the dispersion-tube. In this way the formation of oxide is prevented, and the sodium is freed from the hydrocarbon in which it is kept. The glass plate is immediately cemented on, and the tube exhausted on a mercurial pump. An ordinary hand-pump would doubtless answer every purpose, as the vacuum does not need to be below a few millimetres. I see no reason why a well-made steel tube should not be used year after year in class demonstration, though I have not tried keeping one so long. With the tube once prepared, the apparatus for showing the anomalous dispersion of the vapour, which is one of the most beautiful optical experiments that I know of, can be set up in five minutes. The former method which I described was rather troublesome, as it involved the use of a hydrogen generator and drying-tubes, and it was often difficult to avoid the formation of smoke in the tube. The vacuum-tubes make the experiment as easy to perform as the simplest of the laboratory demonstrations. A combustion furnace is not necessary, two or three Bunsen burners turned down low answering every purpose. The amount of dispersion can be regulated by turning the flames up or down. A full description of the disposition of the apparatus will be found in my first paper. In brief, it consists of a horizontal slit on which the crater of an arc-lamp is focussed. If a Nernst lamp is available it may advantageously be substituted for the illuminated slit, the glower being mounted in a horizontal position. The light from the slit is made parallel with a lens, and after traversing the sodium tube is focussed on the slit of a small spectroscope by means of a second lens. It is almost as satisfactory to use a single lens passing the convergent beam through the dispersion-tube.

If it is desired to show the anomalous *spectrum*, a low-power eyepiece is substituted for the spectroscope. The appearance of this spectrum is shown by a coloured plate illustrating the earlier paper. In the present work it was desirable

to have a normal spectrum, and the spectroscope was replaced by a collimator and plane grating. The light from the grating entered a camera made of a long wooden box, provided with an achromatic telescope-lens of 2 metres focus.

The spectrum, which was received on a plate of ground glass, was about 25 cms. long and 1 mm. wide. Its position was accurately recorded on the ground glass by means of a narrow pencil mark ruled along its centre, the ground surface having been sponged over with oil to increase its transparency. The burners of the furnace were now lighted and turned down very low so that the tips of the flames fell a little short of the tube. As soon as the temperature of the tube became constant, the curved branches of the dispersed spectrum were outlined on the glass with the pencil. The burners were then turned up a little and the deviations of the oppositely curved branches increased, their new positions being recorded in the same manner. A number of records were made in this way, and the tube was then allowed to cool off. The spectrum straightened, and was found to occupy the same position on the plate as before, showing that no displacement of the apparatus had occurred. A cadmium spark and sodium flame were now placed before the slit of the grating spectroscope, and the position of the bright lines recorded on the plate. The deviations which different regions of the spectrum had experienced were then carefully measured. The object of making a number of records was to show whether the relative deviations depended on the temperature of the tube and density of the vapour. No indications of any such irregularity were found. The values found in this way agreed admirably with the results obtained with the interferometer. It moreover furnished data regarding the dispersion in the blue and violet region, which the interferometer failed to do with any accuracy for reasons which I have given. Photographs of the dispersed spectrum are reproduced on fig. 8, Plate XII. They are neither retouched nor enlarged, and were made with sunlight; the channelled absorption spectra in the red and green appear on the plate.

Numerical Results.

The results obtained by the three methods are given in the second column of the table in the section devoted to the refraction and dispersion of sodium vapour of great density, which follows. For the sake of uniformity they have all been reduced to the same scale. A shift of 100 helium (D_3)

fringes being taken as the standard. A wave-length, for which the relative shift with respect to helium light of the interferometer-fringes was found to be 1 : 4, is entered in the table as 25.

The shift ratio for the green line of mercury with respect to helium was found to be 1 : 25. The wave-length of this light is, therefore, represented in the table by the numeral 4, which means that a prism of sodium vapour which gives a deviation 4 for green Hg light gives a deviation 100 for helium light. The results obtained by the methods of crossed prisms (both visual and photographic) were reduced to the same scale, being compared with the value found for the green mercury line. This was necessary, since if a vapour of sufficient density is used to give a measurable deviation in the blue and violet region, it absorbs almost entirely that portion of the spectrum occupied by the helium line. The values obtained by means of the Zeeman doublet and the two yellow lines of mercury, by the method of coincidences, are not given in the table. As these values were purely relative, *i. e.* not compared with helium or any other standard wave-length, it was necessary to assign to one of the wave-lengths a shift taken from the curve determined by the other methods, and then calculate the shift of the other wave-length. For example, in the case of the yellow mercury lines, of wave-lengths 577 and 579, the ratio as obtained by the coincidence method was 5 : 6. The shift of 577 on the curve was found to be 14, *i. e.* 14 of these fringes to 100 of helium 5. The shift of wave-length 579 with respect to helium is given by the proportion $5 : 6 = 14 : x$, from which we get $x = 16 \cdot 8$, a value which fell exactly on the curve obtained with the interferometer.

The same thing was done in the case of the values obtained by breaking the helium line up into a Zeeman doublet, 11 fringes for $\lambda = 587487$, 11·5 fringes for $\lambda = 587513$. The values for the range of wave-lengths comprised between the helium line and D_2 were determined by the method of crossed prisms, the measurements being made with the reticulated mesh in the focus of the eyepiece. For light of wave-length midway between D_3 and D_2 the deviation is twice as great as for D_3, consequently we enter this in the table as 200.

The Ultra-violet Dispersion.

A qualitative study of the dispersion in the ultra-violet was first made by the method of crossed prisms.

A tube of hard Jena glass, 40 cms. long and 1·5 cms.

internal diameter, was fitted with end-plates of quartz, loaded with sodium and exhausted. The arrangement of the appa-
ratus is shown in fig. 5. As the method depends on forming a sharp image of a brightly illuminated horizontal slit upon the vertical slit of the spectrograph, quartz lenses could not be used owing to their chromatic aberration. Concave silvered mirrors are free from this defect, but have abnormally low reflecting power in the ultra-violet region. Very satis-factory photographs were obtained with them, however, in the preliminary work, though they were replaced by magna-lium reflectors later on. The light from the crater of an arc-lamp was focussed on the horizontal slit, and collimated with one of the concave mirrors. It then passed through the sodium tube, after which it was brought to a focus by a second mirror on the slit of a quartz spec-

Fig. 5.

trograph. The tube was heated by a row of small Bunsen burners, and as soon as a steady state was reached, which could be told by observing the curved branches of the spectrum with an eyepiece, the photographic plate was inserted and exposed. Owing to the low reflecting power of the silver the ultra-violet region was somewhat underexposed. The curvature of the spectrum in the immediate vicinity of the first two ultra-violet lines of the principal series was very pronounced, however. The plates obtained with the small quartz spectro-graph are too small to reproduce well, especially as the density is not very great in the most interesting region, and it has seemed better on this account to make drawings from them. The general appearance of the photographed spectrum is shown in fig. 2 (p. 296), which has been enlarged about two diameters. It is apparent that the effect of these lines on the refractivity of the medium is negligible except in their immediate vicinity.

Plates obtained in the manner described were useless for purposes of measurement, as no record was made of the position of the spectrum on the plate before its deviation by the sodium prism.

As the displacement is very slight in the remote ultra-violet the two records would overlap in this region, even if the spectrum was very narrow. To overcome this difficulty the following method was devised. A slide was fitted to the
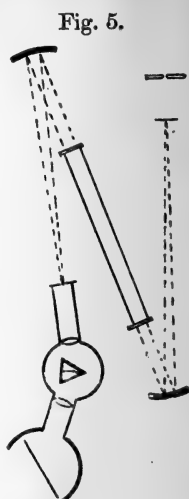
plate-holder, perforated with a number of vertical slits, each about 2 mms. wide, the widths of the opaque bars and slits being equal. This slide was left in the plate-holder during the exposures, and screened off the spectrum at regular intervals. In order to obtain greater deviations a large tube of Jena glass 80 cms. long was used. It was heated in a combustion furnace, the burners of which were turned down low, the best results being obtained when the tops of the flames only partially enveloped the tube.

As it was desired to extend the observations as far down into the ultra-violet as possible, the spark was substituted for the arc, and to avoid loss of light by reflexion one of the mirrors was dispensed with. The spark was furnished by a large induction-coil operated with an alternating current of 110 volts, a leyden-jar of large size having been joined in parallel with the terminals. Cadmium electrodes were used, the spark-gap being placed close to the horizontal slit, which was mounted at the centre of curvature of a concave reflector of magnalium of 1 metre radius. The convergent beam from the reflector passed through the sodium dispersion-tube and came to a focus on the slit of the spectrograph. The bottom of the tube having been raised to a full red heat in the combustion-furnace, the photographic plate was exposed with the slotted slide in position. The anomalously dispersed spectrum was thus recorded in small sections corresponding to the slits in the slide. The tube was then allowed to cool off, the slide drawn out a distance equal to the width of one of the slits,

Fig. 6.
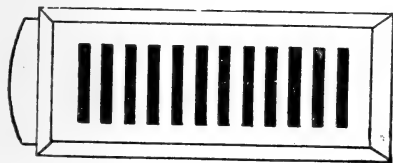
Fig. 7.
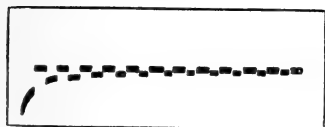
and a second exposure made which recorded the position of the undeviated spectrum. A drawing of the plate-holder, with its perforated slide and the deviated and undeviated spectra obtained with it, are shown in figs. 6 & 7.

Absolute Determinations of the Refractive Index.

The deviations of the light-rays by the non-homogeneous cylinders of sodium vapour formed in exhausted tubes gives us no measure of the absolute refractive index, for the reason that we have no way of determining the angle of the equivalent prism. Various methods were tried designed to give to a mass of vapour of uniform density a prismatic form. None of them was very satisfactory, and until the discovery was made that a fairly uniform cylinder of vapour could be formed in an exhausted tube, the absolute value of the index for any given wave-length could only be guessed at.

The results found for the dispersion with the interferometer showed that results correct to within probably 5 per cent. could in all probability be obtained by this method.

The density of sodium vapour at different temperatures has been investigated by Jewett (Phil. Mag. iv. p. 551, 1902) for a range comprised between 368° and 420°. It was accordingly determined to take the temperature of the vapour as a measure of its density.

A thermo-couple of iron and constantan wire was made by holding the ends of the wires together, both being connected to a storage-battery giving an E.M.F. of about 20 volts. The other wire of the battery was then touched momentarily to the tips of the two wires, the small arc which formed on separation fusing them together very neatly. I do not remember to have seen this method of making thermo-couples described, and have mentioned it as it was found very satisfactory. The fused bead which united the wires was then hammered out into a thin disk, and the junction mounted in the axis of the sodium tube, the wires being insulated with thin glass tubes which passed through a larger tube as shown in fig. 4.

The couple was calibrated in baths of molten lead, zinc, tin, and aluminium before and after the experiment. The determinations at the lower temperatures and densities were made with the light from the helium tube, those at high temperatures with the green light from a mercury tube. This was necessary since a mass of very dense sodium vapour not only absorbs helium light strongly, but owing to its high dispersion makes the fringes invisible, the different wave-lengths in the D line (which is of course of finite width) being shifted by different amounts. If the increase in the density with increasing temperature followed the same law as in the experiments of Jewett, the optically determined density-curve should agree with that found by the gravimetric method employed in his work. For example, he found that the

density at 412° was six times as great as at 385°, and we should consequently look for a fringe-shift at the higher temperature six times as great as at the lower. This, however, was by no means the case, the fringe-shift being only about twice as great. This is, on the whole, exactly what we should expect under the conditions of the experiment, for we must remember that our column of sodium vapour terminates in a vacuum, if the expression is allowable. If the sodium were heated in a tube provided with end plates which were not acted upon by the vapour, so that the whole could be brought to a constant temperature, it is probable that agreement with the results obtained by Jewett would be obtained. As it is, we have a cold condenser at each end of the column, and only obtain vapour of any considerable density through the circumstance that the rate of diffusion is comparatively slow.

I hope in the near future to determine the actual density under the conditions of the experiment.

The density at a given temperature was found to depend, to a large extent, upon the previous treatment of the tube. If the tube was heated for the first time to a temperature of say 350 degrees, a shift of say 10 helium fringes would be observed. If the tube was then raised to a red heat and allowed to cool, on reheating it to the same temperature (350°) a shift of perhaps 15 fringes would be found. It appeared as if the density at a given temperature was much greater if a sodium " dew " had been formed over the entire inner surface of the tube, by the condensation of the dense vapour given off at the high temperature ; in other words, the density at a given temperature was proportional to the surface of molten sodium which was exposed.

The determinations were made in glass tubes for the reason that in steel tubes the transition from the heated to the cool portion of the tube was not sufficiently sudden to ensure uniform density in the cylinder of vapour. The Jena-glass tubes could, however, be raised to a full red heat by means of the coil of iron wire without showing any trace of sodium vapour in the portions protruding from the helix.

In order to obtain consistent results, it was found necessary to heat the tube very gradually in order to allow the thermo-element time to take the temperature of the vapour. One observer watched the scale of the galvanometer, the other counted the fringes as the tube was heated or cooled. The helium fringes disappeared after a shift of something over a hundred fringe-widths had occurred, but by dropping a plate of plane parallel glass into the air-path of the interferometer

they could be made distinct again and the count continued. In this way it has been possible to record shifts as high as 400 fringe-widths. Beyond this point it is impossible to go with the helium light, the components of helium light becoming too much spread out by the dispersion of the vapour to be brought together again by going out any farther in the fringe system. To extend the observations to higher temperatures and greater vapour-density, the green radiation from a mercury-tube was used, with which fringe-shifts as high as 100 fringe-widths were observed, corresponding to a shift of 2500 helium fringes. In this way it has been possible to obtain measures of the optical density of the vapour under the specified conditions from a temperature of 280°, at which point the fringes commenced to move, to above 650°, at which temperature the dense vapour was luminous, the entire section of the tube appearing filled with a dull red glow. Plotting the fringe-shifts as abscissæ and the temperatures as ordinates, we obtain curves of form similar to the density-curve obtained by Jewett, though by no means coincident with it, for reasons which I have already given.

The observations were made both when heating and cooling. At first it was found that the shift obtained on cooling was greater than the one observed during the heating; in other words, the fringe system did not return to the starting-point. This was found to be due to hydrogen gas liberated from the sodium, which, from the circumstance that it shifts the fringes in the opposite direction from the shift produced by the sodium vapour, retards the motion of the system during its liberation, and by remaining unabsorbed in the tube prevents the fringes from returning to the starting-point. This trouble was eliminated by heating the tube to a high temperature and setting the mercurial pump in operation. The observations extended over several weeks, many tubes both of steel and glass being used. The length heated was varied and the different series of observations compared. Most of this work was only useful in determining and eliminating the sources of error.

The final series, which was considered the best, was made with a tube of Jena glass, the length of the heated portion being 8 cms. The retardation, or rather the acceleration, since we are working with light on the blue side of the absorption-band, is due to a column 16 cms. long, since it is twice traversed by the interfering beam. From the data given in this table the refractive index of the vapour for either helium light or the green mercury radiation can be calculated for any temperature by means of the

formula $n = 1 \pm \dfrac{m\lambda}{\epsilon}$, in which m is the fringe-shift and ϵ the length of the column (16 cms.), the $+$ or $-$ sign being used according to the direction of the shift.

Temperature.	Fringe-shift.		Temperature.	Fringe-shift.	
274	1		560	14	
282	2		584	19	
314	3		595	23	Mercury
334	4		608	32	$\lambda = 546$
348	5		628	38	
360	6		644	50	
376	8		650	55	
389	9	Helium			
396	11	$\lambda = 5875$			
408	14				
438	25	1			
452	35				
474	50	2	Mercury		
496	77	3	$\lambda = 546$		
508	98	4			
518		6			
540		10			

Curves are given for these optically determined densities in Plate XI., Curve 3.

The Refraction and Dispersion of Sodium Vapour of Great Density.

A knowledge of the absolute value of the refractive index of the vapour and its dispersion enables us to compile a table of the refractive indices for all wave-lengths, for vapours of varying density. This has been done for the very dense vapour obtained by heating a vacuum-tube containing the metal to a temperature of 644° C.

A column of the vapour at this temperature 8 cms. in length examined by transmitted light has a distinct blue colour, as a result of the channelled absorption-spectrum.

The values are given in the following table, and will be spoken of in future as "observed values," to distinguish them from values calculated from the dispersion formula. It must be remembered that sodium vapour as dense as that with which we are dealing in the present case has an absorption-band at the D lines broad enough to completely cut out everything down to and even below the helium line, at least for all thicknesses with which it is possible to work. On this account we are obliged to calculate the refractive indices within this region from observations made with a less dense vapour, a method which in the present case is probably allowable within certain limits. A thin enough sheet of the vapour

would probably transmit light within this region with a velocity indicated by the calculated indices. The question of selective reflexion at the surface and refusal to transmit the radiation will be considered presently.

In the following table the wave-lengths are given in the first column, the fringe displacements in comparison with helium light in the second column. These values, with the exception of those in the extreme red, blue-violet, and ultra-violet, were obtained with the interferometer.

In the third column are given the actual fringe-shifts which would be found for a layer of vapour 8 cms. thick (effective thickness 16 cms.), and in the fourth column the refractive indices. The indices calculated from the dispersion formula are given in the fifth column, for the sake of comparison with the observed values. More will be said of them in the next section.

λ.	Relative Fringe-shift.	Total Shift.	Refractive Index Obs.	Refractive Index Calcul.
7500	2	25	1·000117	
6310	4	55	1·000197	
6200	6	75	1·000291	1·000285
6137	7	88	1·000335	
6055	11	138	1·000523	1·00052
6013	14	175	1·000658	
5977	20	250	1·000934	
5960	25	313	1·001164	
5942	33	413	1·001532	
5916	60	750	1·002972	
5875	100	1250	0·995410	·9958
5867	67	834	·996929	·99692
5858	50	625	·997711	
5850	40	500	·998172	·99815
5843	33	413	·998492	
5827	25	313	·998862	
5807	20	250	·999093	
5750	11	138	·999505	
5700	9	113	·999599	
5650	7·4	92	·999650	
5460	4	50	·9998294	
5400	3·6	45	·9998481	
5300	2·9	36	·9998807	·999885
4500	1·4	17·5	·9999508	·999965
3610	1·09	13·6	·9999698	
3270	·9	11·4	·9999768	·999987
2260	·7	8·7	·9999877	·999995

For $\lambda = \infty$ the value of n is calculated to be 1·0000275, while for $\lambda = 4167$ it is ·999975, the deviation being the same in both cases, but in opposite directions. The complete dispersion-curve will be found on Plate XI. Curve 2, the

numbers representing the ordinates and abscissæ being outside of the large rectangle.

Refractive Indices in the vicinity of the D lines.

λ.	*n* Calc.	*n* Obs.
5875	0·9958	0·9954
5882	0·9890	0·9908
5885	0·9830	0·9860
58866	0·9750	0·9770
58884	0·9450	0·9443
58896	0·697	0·614
5991	1·0046	
5904	1·0092	
5901	1·0138	
58994	1·0184	
58976	1·0557	
5897	1·094	
58964	1·386	

Application of the Results to the Dispersion Formula.

The simplest form of the dispersion formula, developed from electromagnetic considerations for a medium with a single absorption-band, is

$$n = 1 + \frac{m\lambda^2}{\lambda^2 - \lambda_m^2},$$

in which m is a constant, λ the wave-length of the light employed, and λ_m the wave-length at the centre of the absorption-band.

The vapour of sodium has, of course, a pair of very close absorption-bands (the D lines) which are chiefly effective in modifying the refractivity of the medium. As I have said, the ultra-violet bands affect the dispersion in their immediate vicinity, but their effect can be neglected in comparison with the stronger band, except for wave-lengths very close to them.

The first question to consider is whether we are justified in considering the D lines as a single absorption-band in the case of the very dense vapour. If we consider the medium as having a single band, and assign to λ_m the value 5893 (a point midway between the D lines), we shall find that the observed and calculated values of the refractive index agree very closely, up to within a distance of the band about equal to the distance of the D_3 line of helium. If we attempt to go closer than this, we immediately find discrepancies, which become larger as we approach the D lines.

The constant m in the above formula can be determined from a single observation of the refractivity. It was determined from two values, namely, the refractivity for the green line of mercury ($\lambda = 546$) and that for $\lambda = 585$, the latter wavelength being quite close to the D lines. Since the refractivity has widely different values for these two wave-lengths, we should expect the difference between the two calculated values to be a maximum in this case, in the event of the dispersion being incorrectly represented by the formula. The values for m found in the two cases were $0 \cdot 000056$ and $0 \cdot 000054$, a surprisingly close agreement. The mean value $m = 0 \cdot 00055$ was taken, and the indices for a number of wave-lengths calculated. Some of these values are given in the table of refractive indices, and they will be found to agree very closely with the observed values. The values calculated between the helium line and the D lines are given in the second part of the table.

It will be noticed that the greatest discrepancies are found in the ultra-violet, the observed values indicating a greater deviation (negative since the index is less than unity) than we should expect theoretically. In other words, we should expect the deviated spectrum to be more nearly in coincidence with the undeviated than was found in the photographs taken through the slotted slide. I am inclined to attribute the discrepancy to the presence of hydrogen in the tube. The dispersion-tube was exhausted and sealed off from the pump before introduction into the combustion-furnace. A small amount of hydrogen is always liberated from the sodium as soon as it is heated, and without doubt the tube contained a small amount of this gas during the experiment. At the time it was not expected that this would give any trouble, but observations appear to indicate the contrary. A tube containing air or any ordinary gas acts like a prism with its thin edge down, when the floor of the tube is strongly heated. This is owing to the rarefaction of the gas along the heated floor. Now the sodium prism is turned in exactly the opposite way, the denser portions being close to the floor. But on the blue side of the sodium absorption-band the index of refraction is less than unity; consequently the oppositely oriented prisms (hydrogen and sodium vapour) deviate in the same direction, and the effect of the hydrogen prism will be greatest in the ultra-violet for obvious reasons. The result will be that slightly too great deviations will be found in this region, which is precisely what we require to explain the discrepancies between the observed and the calculated values.

Unfortunately sufficient time did not remain for the repetition of the experiment with a tube permanently in connexion with the pump, but the omission is not serious, for I feel certain that the explanation given above is the correct one.

An inspection of the formula shows us that, according as we are on the red or blue side of the absorption-band, the refractive index is given by adding to or subtracting from unity the value of the constant $m = 0.000055$ multiplied by the fraction $\dfrac{\lambda^2}{\lambda^2 - \lambda_m^2}$. In the case of all other substances showing anomalous dispersion, aniline dyes for example, to the dispersion of which the formula has been applied, the value of this fraction never exceeds 3 owing to the impossibility of applying it to wave-lengths very close to the centre of the band. For example, in the case of the dispersion of nitroso-dimethyl-aniline, with its strong absorption-band at $\lambda = 43$, we cannot obtain accurate data nearer than $\lambda = 50$. In this case $\dfrac{\lambda^2}{\lambda^2 - \lambda_m^2} = 3.9$.

In the case of sodium vapour the value of the fraction may be several hundred or even thousand. In the case of $\lambda = 5882$ the fraction is 367, and yet the observed and calculated values agree closely. For $\lambda = 58884$ the fraction is 1940, and for $\lambda = 5889.6$ we have a value as high as 3944. The product of these very large numbers and the small fraction 0.000054 give, however, values of the index which are in close agreement with the observed values.

Discrepancies occur in the immediate vicinity of the D lines which can be explained in the following way. To get values in any way consistent with the observed values, it was necessary to assign to λ_m the value of the D_2 line, the mean value 5893 being too far removed from the wave-lengths in question to give the requisite steepness to the curve. The calculated values, therefore, apply to a medium with a single band at D_2, and with a constant $m = 0.00055$. This gives us a pretty good approximation to the observed curve, but the latter is due to the combined effects of the bands D_1 and D_2, the presence of the D_1 band tending to make the observed curve flatter than the calculated. A more correct approximation could be obtained by assigning to λ_m a value intermediate between D_2 and 5893. The proper method of procedure would of course be to make use of two members in the dispersion formula, one for D_1 and the other for D_2. Thus

$$n^2 = 1 + \frac{m\lambda^2}{\lambda^2 - \lambda_m^2} \times \frac{m'\lambda^2}{\lambda^2 - \lambda_{m'}^2}.$$

If m and m' were each assigned the same value obtained by dividing our original value by 2, in all probability a very close approximation would be obtained in the region in question. This has not been done for two reasons. In the first place, it does not appear as if much would be learned by the procedure; and in the second place, m and m' are not equal, as is shown by the stronger dispersion near D_2, and until the relative values have been determined we are not in a position to write the two-member formula accurately. It is doubtful whether anything new would come out of such a determination, and it was on that account not attempted.

Another matter of considerable interest is the question of the indices represented by the square root of a negative quantity in the immediate vicinity of the absorption-band on the blue side. Lord Kelvin interprets this as indicating that no light of such wave-lengths enters the medium; in other words, it is metallically reflected. It is in this way that he has explained the apparent greater broadening of the D lines on the more refrangible side in some of Becquerel's photographs. In the case with which we are dealing, the second term of our original formula does not become less than unity until we reach wave-length 58898, which we get by equating

$$\frac{m\lambda^2}{\lambda^2 - \lambda_m^2}$$ to unity and solving for λ.

This shows us that, even with a vapour so dense that both D lines run together and broaden out into a wide band, we do not get values of the index which are imaginary until we are within 0·2 of an Ångström unit of the D line, or, in other words, until we are within a distance of D_2 equal to 1/30 of the distance between D_1 and D_2. In the case of the comparatively rare vapour employed by Becquerel, we should have to approach much closer than this to get the imaginary values. This makes it appear certain that the greater broadening on the more refrangible side, if it exists, must be assigned to some other cause than imaginary values of the refractive index.

The medium is exceptionally interesting in that its dispersion can be represented throughout the entire range of wave-lengths without taking the extinction coefficient into account, as is always necessary in the case of solids and liquids when in the vicinity of the absorption-band.

The Question of the Selective Reflexion of Sodium Vapour.

The vapour of sodium should exhibit strong selective reflexion in the vicinity of the D lines, just as the aniline dyes

have metallic reflecting-power for light of certain wave-lengths. For obvious reasons the vapour must be very dense if the phenomenon is to be detected experimentally. It must, moreover, terminate abruptly in a flat surface. This can only be accomplished by confining it in closed vessels made of some transparent material, since in a vacuum-tube we have a gradual transition from dense to rare vapour at the free surface. I feel justified in speaking of the free surface of a gas in a vacuum-tube in this particular case. A more precise definition of the apparent surface may be an isothermal surface, on one side of which we have sodium vapour and on the other sodium fog. From such a surface we should hardly expect to get any trace of reflexion, which requires that the transition from dense to rare be sudden. If we confine the vapour in glass bulbs, we at once encounter many difficulties. The glass is at once attacked and discoloured, and we have the reflexion from the glass surfaces. An attempt was, however, made to observe the phenomenon in the following way. A small amount of the metal was sealed up in a hard glass bulb, highly exhausted. The bulb was mounted in a small air-bath of sheet-iron which could be rapidly raised to a red heat.

The reflexion of the filament of a Nernst lamp by the inner and outer surfaces of the bulb was observed through a large direct-vision prism. The two minute points of light were seen drawn out into spectra, and it was hoped that any trace of selective reflexion in the vicinity of the D lines would manifest itself by a change in the relative intensities of the two spectra at the point in question.

The bulb was heated very rapidly, but no conclusive observation made. The glass discoloured so rapidly that the densest vapour could not be studied. In one case it was thought that a slight brightening in the yellow occurred, and the subject will be further investigated. Possibly by employing light polarized in such a plane as to be refused reflexion by glass surfaces, we may be able to get the desired results.

Bulbs of fused quartz become discoloured in the same way, but I have been told that fused alumina remains transparent. If it be found possible to blow a small bulb of alumina on the end of a quartz tube, the question of the selective reflexion can be easily settled.

The phenomenon is doubtless connected intimately with the fluorescence of the vapour. In studying this phenomenon with very dense vapours, I was forced to the conclusion that the illumination of the vapour with light of the wave-length

of the D lines did not provoke the fluorescence.   Wiedemann and Schmidt observed a bright band in the fluorescence spectrum in the case of vapour confined in glass bulbs which appeared to coincide with the D lines.   The same appearance I afterwards observed independently, but on continuing the study of the subject was forced to refer the existence of this band in the yellow to sodium in the flame which heated the bulb.   This I have since found was a mistake, for on repeating the experiments with exhausted glass bulbs, I have succeeded in stirring up a faint fluorescence with approximately monochromatic light from the illuminator already referred to, of wave-length equal to that of the D lines. The failure to observe it in the case of the experiments made last year by Mr. Moore and myself was due to the fact that this yellow light was removed from the incident beam by the sodium vapour before the light-rays met at the focus.   This work is still in progress, and the fluorescent spectra given by the vapour when illuminated with monochromatic light of various wave-lengths have been photographed.

XXXIII. *The Achromatization of Approximately Monochromatic Interference Fringes by a Highly Dispersive Medium, and the consequent Increase in the Allowable Path-difference. By* R. W. Wood, *Professor of Experimental Physics, Johns Hopkins University* *.

THE results recorded in the present paper were, for the most part, obtained during the progress of an investigation of the dispersion of sodium vapour.   As I have mentioned in the previous paper, the path-difference under

* Communicated by the Author.

which it is possible to obtain interference-fringes with helium (D$_3$) light can be more than doubled by the introduction of a small amount of sodium vapour into the path of one of the interfering beams. This development of fringes far out in the system by the dispersive action of the vapour is accompanied by their complete disappearance at the centre of the system, where the difference of path is zero.

In order to understand this action of the vapour we must first consider briefly the conditions under which fringes may be visible.

Suppose that we have a system of circular fringes formed with white light, and consider a point just outside of the visible ring system, where the illumination appears uniform. Our fringe system is built up of an infinite number of coloured systems which are in coincidence at the centre, but which get more and more out of step as we advance out into the system, owing to the fact that the " scale " on which the Newton rings are formed decreases with decreasing wavelength. Let us now consider in what manner fringes may be made to appear at a point where the overlapping is so great as to destroy all trace of the fringes; in other words, how may achromatization be more or less completely secured.

It appears to me that there are only two conceivable ways in which the result can be obtained. If we could, by the introduction of a dispersing medium, increase the diameters of the blue rings without greatly affecting the diameters of the red ones, it is obvious that we should greatly increase the number of visible fringes without, however, altering their distinctness at the centre of the system.

A slight inclination of either of the back mirrors of the interferometer increases or diminishes the scale on which the fringes are formed, and since a similar change in the direction of the reflected rays can be effected by the introduction of an acute prism, it is easy to see that, owing to the dispersion of the latter, the change in the scale will be different for the different wave-lengths, more or less perfect achromatization resulting.

The introduction of a medium into the path of one of the interfering beams causes a shift of the fringe system as a whole, and if the medium is dispersing the shifts will be different for the different colours. The red, green, and blue fringes, which are out of step at a given point, may thus be brought into coincidence by the inequality of their respective displacements. In this case, however, since the systems are shifted as a whole, the fringes will be thrown out-of-step at the centre of the system, consequently we have obtained an

increase in the distinctness far out in the system, at the expense of distinctness at the centre. This is precisely what happens in the case which we are considering.

It has been found in every case that the introduction of sodium vapour into one path of the interferometer increases the distinctness of the fringes in a portion of the system which is brought into the field of the instrument by increasing the length of the other path.

We will now consider the case of the helium fringes, which under ordinary circumstances disappear when the path difference is between 1·5 and 2 cms., there being no recurrence of visibility by further increment of path difference as in the case of sodium light. We must therefore regard the helium (D_3) line as a single line of finite breadth or a close group of lines. In fig. 1 let BC represent

Fig. 1.
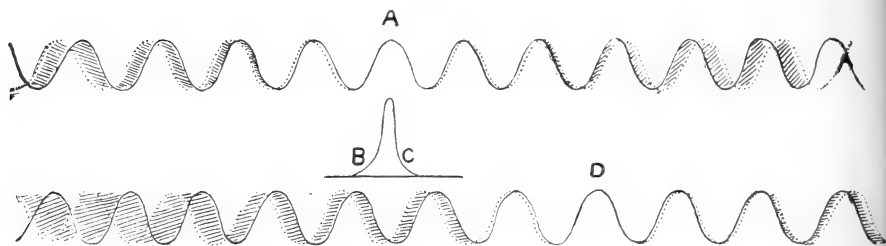
the intensity curve of the helium light, C being the edge of shorter wave-length. Immediately above we have a schematic representation of the fringe system, with its centre at A. Light from the side B of the D_3 line will produce the fringes indicated by the dotted line, which are farther apart than the fringes formed by the light of shorter wave-length coming from the side C of the line. There will, in addition, be an infinite number of other systems formed by light of wave-lengths intermediate between B and C which I have indicated by light shading.

Now suppose sodium vapour to be introduced into one path of the instrument, and the whole system shifted slightly to the left in consequence. Owing to the enormous dispersive power of the vapour, the dotted system (longer $\lambda's$) will be shifted more than the other, since the D_3 line lies on the blue side of the sodium absorption-band, and the change in the velocity of the light is greatest for the longest waves, namely, those on the B side of the line. The result of this dispersive action is that the fringes are brought into step at a point D, to the right of the centre, thrown out-of-step at

the centre, and still more out-of-step to the left of the centre. If we had but the two systems indicated by the solid and dotted lines, it is obvious that the systems would come into step again to the left of the centre, a condition which would occur if D_3 consisted of two infinitely narrow lines very close together. In the actual case the presence of waves of length intermediate between those of B and C make such a recurrence of visibility to the left of the centre impossible, and we have distinct fringes to one side only of the original centre of the system. On increasing the density of the sodium vapour, the point D of maximum visibility moves further along to the right, and to keep the fringes in the field it is necessary to turn the screw of the instrument in such a direction as to cause the system to move in *the same direction as the* shift due to the sodium vapour.

Now the sodium vapour accelerates the helium light, since its refractive index is less than unity for light of shorter wave-length than that of D_2, consequently the reduced path is less. To shift the fringes in the same direction as that resulting from the shortening of the path through the sodium vapour, we must *lengthen* the other or air-path, which is precisely what was found to be the case as I have already said.

If the D_3 line lay on the other side of the D lines, the shift would be in the opposite direction, *i. e.* to the right, and we might at first sight expect the point of maximum visibility to shift to the left of the centre. We must, however, remember that in this case the change of velocity is greatest for the *shortest* waves on the side C of the line; consequently the system indicated by the solid line will suffer the greatest displacement, and we shall have coincidence at D, to the right of the centre, exactly as before. To test this point experimentally, the interferometer was illuminated with light from the monochromatic illuminator, a narrow band on the green side of the D lines being utilized. The formation of sodium vapour in one of the paths gave rise to the same changes as were produced in the case of the helium light, it being necessary to increase the air-path to prevent the fringes from disappearing. If the fringes were made very narrow, so as to occupy only a small portion of the field, the wandering of the system to one side could be easily watched, as the sodium vapour was formed. It must be understood that only a very small actual *displacement* occurs, the wandering of the system being merely a change in position of the region over which fringes can be seen. On repeating the experiment with a band of approximately monochromatic light on

Z 2

the red side of the D lines, a similar drift of the position of maximum visibility was observed, and the direction of the drift was the same as before.   In the case of helium light I have been able to increase the path difference to five or six centimetres, or to nearly treble it.

The achromatizing action of the sodium vapour is most beautifully shown if we illuminate the interferometer with white light.

Under ordinary conditions only two or three black and white fringes are seen, bordered on each side by perhaps a dozen rainbow-coloured bands, which fade rapidly into a uniform illumination.   If sodium vapour is formed in one of the interferometer paths, the coloured fringes rapidly achromatize, and increase in number, breaking up, however, into groups as shown in fig. 2.   As the density of the vapour

Fig. 2.
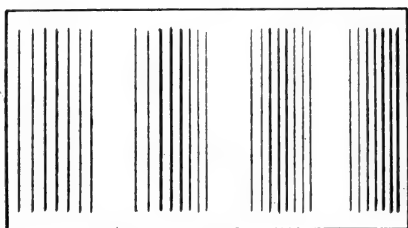
increases the number of groups increases, each group, however, containing fewer fringes.   The position of the centre of the grouped system drifts in the same direction as the point of maximum visibility in the previous experiments.

The explanation of the altered appearance of the fringes in this case is not as simple as in those previously considered. We are dealing with two wide ranges of wave-lengths on opposite sides of the absorption-band.   The fringe shifts of the two spectral regions will be in opposite directions, while the drifts of the points of maximum visibility will be in the same direction.   It appeared as if this might increase the width of the region over which fringes could be observed, for the red-orange fringes are shifted in one direction and the yellow-green in the opposite.   Each set would be more or less perfectly achromatized, and in the region in which they overlapped we should expect a periodic visibility, owing to the difference in the widths of the fringes of the two systems.

To test the point it seemed best to work with a narrow

range of the spectrum symmetrical about the D lines. This was obtained by opening the slit of the monochromatic illuminator, bisecting it with a wire, and adjusting the prisms so that the region of the D lines was screened off by the wire. By means of a small screen either of the two narrow portions of the spectrum bordering the D lines could be screened off.

The effect of the sodium vapour on the fringes formed when the interferometer was illuminated by either one or both of the two portions of the spectrum could then be studied at leisure.

It was found that when a considerable amount of the vapour was present the apparent centre of the greenish-yellow fringe system was widely separated from the centre of the orange-yellow system.

When both sorts of light were used at once there was a periodic visibility in the region in which the two systems overlapped, the appearance in the three cases being shown in fig. 3.

Fig. 3.
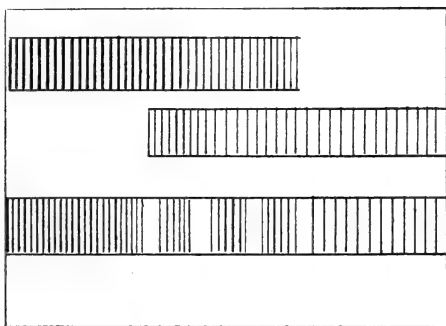
The case is a little more complicated when white light, or the entire spectrum, is used, but it does not differ materially from the special case just considered.

Practically the same thing occurs when the interferometer is illuminated with sodium light, except that in this case the density of the sodium vapour in the optical path must be very much smaller. A periodic visibility results even when the light of one of the D lines is removed by the polarizing system described in the previous paper. The case is of course similar to the last-mentioned, for the width of the D line illuminating the instrument is greater than the width of the absorption-band of the rare vapour. We thus have a condition identical with that which we had when

the emitting slit of the monochromatic illuminator was bisected with a wire which cut out the D lines from the narrow band of the spectrum which was utilized.

Note by Lord RAYLEIGH.

Having had an opportunity of seeing the above paper in proof, I append with Prof. Wood's permission a few remarks.

The remarkable shift of the bands of helium light when a layer of sodium vapour is interposed in the path of one of the interfering pencils, is of the same nature as the displacement of the white centre found by Airy and Stokes to follow the insertion of a thin plate of glass. If D denote the thickness of the plate and μ its refractive index, $(\mu - 1)$ D is the retardation due to the insertion of the plate, and if R be the relative retardation due to other causes, the whole relative retardation is

$$R + (\mu - 1)\, D, \quad \ldots \quad \ldots \quad (1)$$

in which R and D are supposed to be independent of the wave-length λ, while μ does depend upon it. The order of the band (n) is given by

$$n = \frac{R + (\mu - 1)\, D}{\lambda}. \quad \ldots \quad \ldots \quad (2)$$

For the achromatic band in the case of white light, or for the place of greatest distinctness when the bands are formed with light approximately homogeneous, n must be stationary as λ varies, *i. e.*,

$$\frac{dn}{d\lambda} = 0. \quad \ldots \quad \ldots \quad (3)$$

For a small range of wave-length we may write

$$\lambda = \lambda_0 + \delta\lambda,$$

so that

$$n = \frac{R + \left(\mu_0 + \dfrac{d\mu}{d\lambda_0}\delta\lambda - 1\right) D}{\lambda_0 + \delta\lambda}$$

$$= \frac{R + (\mu_0 - 1)\, D}{\lambda_0} + \frac{\delta\lambda}{\lambda_0}\left(D\frac{d\mu}{d\lambda_0} - \frac{R + (\mu_0 - 1)D}{\lambda_0}\right) \quad . \quad (4)$$

The achromatic band occurs, not when the whole relative retardation (1) vanishes, but when

$$R + (\mu_0 - 1)\, D = D\lambda_0\frac{d\mu}{d\lambda_0}. \quad \ldots \quad \ldots \quad (5)$$

If D be great enough, there is no limit to the shift that may be caused by the introduction of the dispersive plate.

As Schuster has especially emphasized, the question here is really one of the *group-velocity*. Approximately homogeneous light consists of a train of waves in which the amplitude and wave-length slowly vary. A *local* peculiarity of amplitude or wave-length travels in a dispersive medium with the *group* and not with the *wave*-velocity ; and the relative retardation with which we are concerned is the relative retardation of the groups. From this point of view it is obvious that, what is to be made to vanish is not (1) in which μ is the ratio of wave-velocities V_0/V, but that derived from it by replacing μ by U_0/U, or by V_0/U, where U is the group-velocity in the dispersive medium. In vacuum the distinction between U_0 and V_0 disappears, but in the dispersive medium

$$U = \frac{d(kV)}{dk}, \quad \cdots \quad \cdots \quad (6)*$$

k being the reciprocal of the wave-length in the *medium*. If we denote as usual the wave-length *in vacuo* by λ,

$$k = \frac{2\pi\mu}{\lambda} = \frac{2\pi V_0}{\lambda V}. \quad \cdots \quad \cdots \quad (7)$$

Accordingly

$$\frac{V_0}{U} = \frac{V_0 dk}{d(kV)} = \frac{d(\mu/\lambda)}{d(1/\lambda)} = \mu - \lambda \frac{d\mu}{d\lambda}. \quad \cdots \quad (8)$$

Substituting this for μ in (1), we see that the position of the most distinct band is given by

$$R + \left(\mu - 1 - \lambda \frac{d\mu}{d\lambda}\right) D = 0, \quad \cdots \quad \cdots \quad (9)$$

in agreement with (5).

XXXIV. *On Momentum in the Electric Field. By* J. J. Thomson, *M.A., F.R.S., Cavendish Professor of Physics, Cambridge* †.

I HAVE for some years made considerable use of the principle proved in ' Recent Researches,' p. 9, that momentum as well as energy is distributed throughout the electromagnetic field, and that changes in the momenta of magnets, of circuits conveying electrical currents, and other material systems in that field are accompanied by equal and opposite changes in the momentum of the field itself. Thus

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* ' Theory of Sound,' § 191, 1877.
† Communicated by the Author.

the system consisting of the field and the material systems in it, form a self-contained system the momentum of which remains constant in magnitude and direction. If we take the view that the effects in the electromagnetic field are to be explained on dynamical principles by the agency of a medium filling the field, the conception of momentum in this medium is as essential and as fundamental as that of energy. As the consideration of the momentum in the field throws a good deal of light on electromagnetic phenomena, and leads in a simple way to some of the most important laws of electromagnetic action; I have thought that a discussion of the application of the method to some simple cases might be useful, and also help students to get a better grasp of the principle and the methods of its application. A few such cases have already been given in my ' Electricity and Matter.' Some of these are repeated here for the sake of completeness.

The basis of the method is the result (Phil. Mag. xxxi. p. 149, 1891 ; ' Recent Researches,' p. 9) that in the electromagnetic field, there is at any point, an amount of momentum equal per unit volume to $\mu \mathrm{HD} \sin \theta$, where H is the magnetic force at the point, D the electric polarization (Maxwell's Electric Displacement), μ the magnetic permeability of the medium, θ the angle between the directions of H and D : the direction of the momentum is at right angles to H and D, drawn according to the scheme on fig. 1.

Fig. 1.
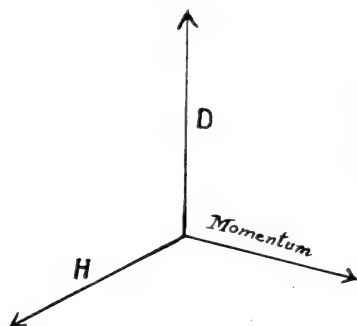
Thus P, Q, R the components of the momentum parallel to the axes x, y, z are given by the equations

$$P = \mu(\beta h - \gamma g),$$
$$Q = \mu(\gamma f - \alpha h),$$
$$R = \mu(\alpha g - \beta f).$$

Case of a Pole and Charged Point.—If we have a pole of strength m at A and a charge e at B, the preceding rule shows that the momentum at P is at right angles to the plane APB and is per unit volume equal to

$$\frac{1}{4\pi} \frac{e}{\mathrm{BP}^2} \frac{m}{\mathrm{AP}^2} \sin \mathrm{APB}.$$

This distribution of momentum can easily be shown to be equivalent to a moment of momentum em around AB ; the direction of rotation being opposite to that for a right-handed screw when the direction of translation is from A to B.

The resultant momentum in any direction vanishes ; as this is true for all positions of A and B it follows that however the point and pole may move about in the field the momentum in any direction gained by the pole must be equal and opposite to that gained by the point ; *i. e.*, the forces on the pole and point must be equal and opposite.

To find these forces, let us suppose that in a time δt the point B comes to B′, A remaining at rest ; then the moment of momentum in the field has changed from em along AB to em along AB′ ; *i. e.*, the moment of momentum in the field has changed by $em \sin \phi$ at right angles to AB in the plane ABA′. The material system consisting of the pole and point must have gained the moment of momentum lost by the field ; the moment of momentum $em \sin \phi$ in the assigned direction is equivalent to momentum $\dfrac{em \sin \phi}{\mathrm{AB}}$ at A at right angles to the plane BAA′ and an equal and opposite momentum at B. Thus we may suppose A to have gained and B lost this momentum in time δt, the momentum gained by A per unit time, *i. e.* the force acting upon A, is $\dfrac{em \sin \phi}{\mathrm{AB}} \Big/ \delta t$. Now $\sin \phi = \dfrac{\mathrm{BB}' \sin \theta}{\mathrm{AB}}$, where θ is the angle between BB′ the direction of motion of B and AB ; thus the force on A is

$$\frac{em \sin \theta}{\mathrm{AB}^2} \cdot \frac{\mathrm{BB}'}{\delta t} = \frac{em \, v \sin \theta}{\mathrm{AB}^2},$$

where v is the velocity of B.

Thus a moving charge is acted upon by a force at right angles to the direction of motion, at right angles also to the magnetic force, and equal to H$ev \sin \theta$: hence the principle of momentum gives us in a very simple way the force on a moving charged point.

Exactly the same result will apply if the pole moves instead

of the point ; a moving pole in an electric field will be acted on by a force $m\mathrm{F}v \sin \theta$, where F is the electric force, v the velocity, and θ the angle between F and v ; the moving pole will exert a force upon the fixed charge.

If pole and point move with the same velocity, AB will move parallel to itself ; there will be no change in the moment of momentum and no forces on the pole or point.

If we had any number of poles of strengths $m_1, m_2, \ldots m_n$ at the points $A_1, A_2, \ldots A_n$, and a system of charges $e_1, e_2, e_3, \ldots e_p$ at $B_1, B_2, \ldots B_p$, the distribution of momentum in the field has for its moment of momentum the resultant of moments of momenta $\Sigma m_r e_s$ acting along $A_r B_s$.

The following results are often useful in calculating the moment of momentum due to distributions of electricity over the surfaces of spheres.

The moment of momentum, due to a pole at A and a charge at B, contained within a sphere of radius r with its centre at either A or B is $\frac{1}{3} me \cdot \frac{r^2}{AB^2}$, if r is less than AB ; while the moment of momentum outside a sphere of radius r, when r is greater than AB, is $\frac{2}{3} me \frac{AB}{r}$

Let us suppose that the pole is shielded from the electric force due to the charge by being placed at the centre of a metal sphere of radius a ; then inside this sphere there is no electric force. If we calculate the moment of momentum in this case, taking into account the charge induced on the sphere round the pole, we shall find that it is equal to

$$me\left(1 - \frac{a^2}{AB^2}\right),$$

AB being supposed greater than a ; the axis of the moment is along AB. In this case the moment of momentum involves the distance between the pole and the point ; so that if we make the point move towards the pole, the moment of momentum in the field will change, the moment of momentum lost or gained by the field is gained or lost by the sphere, so that the sphere moves as if acted on by a couple $\frac{d\Gamma}{dt}$, where Γ is the moment of momentum of the field. Using the value of Γ just found, we find that the couple on the sphere is

$$\frac{2me\mathrm{V}a^2}{AB^3},$$

where V is the velocity of the point along AB. The existence

of this couple would, on the more usual way of regarding the phenomenon, be explained by noticing that when the charged point moves towards or away from the sphere the distribution of electricity over the surface of the sphere changes. There is thus a movement of electricity over the surface of the sphere constituting a system of electric currents, and the mechanical forces arising from the action on these currents of the magnetic field due to the pole at the centre gives rise to the couple whose moment we have just calculated.

If we consider the term em in the expression for Γ we get the same forces on the pole and the point as if the shielding sphere round the pole were absent, while the term $\dfrac{ema^2}{\mathrm{AB}^2}$ may easily be seen to give rise to a couple acting on the system consisting of the pole and sphere whose components parallel to the axes of x, y, z are respectively

$$ema^2 \left\{ u\,\frac{d^2}{dx^2}\frac{1}{r} + v\frac{d^2}{dx\,dy}\frac{1}{r} + w\,\frac{d^2}{dx\,dz}\frac{1}{r} \right\},$$

$$ema^2 \left\{ u\,\frac{d^2}{dx\,dy}\frac{1}{r} + v\,\frac{d^2}{dy^2}\frac{1}{r} + w\,\frac{d^2}{dy\,dz}\frac{1}{r} \right\},$$

$$ema^2 \left\{ u\,\frac{d^2}{dx\,dz}\frac{1}{r} + v\,\frac{d^2}{dy\,dz}\frac{1}{r} + w\,\frac{d^2}{dz^2}\frac{1}{r} \right\},$$

where x, y, z are the coordinates of B, $r = \mathrm{AB}$, u, v, w are the components of the velocity of B. We see from this expression that the axis of the couple on the sphere is parallel to the magnetic force at A due to a doublet at B magnetized parallel to the direction of motion of B. If the moment of this doublet is equal to the velocity of B, then if H is the force at A due to this doublet, the couple on the sphere has its axis parallel to H and its moment equal to $\mathrm{H}ema^2$.

Let us suppose that instead of a charged point we have a uniformly electrified spherical surface. Let e be the charge on the surface, a the radius of the sphere, B its centre. Then if the pole is at B, the moment of momentum about AB of the field is

$$em\left(1 - \frac{1}{3}\frac{a^2}{\mathrm{AB}^2}\right).$$

Here, as in the last case, we have, in addition to the forces which would act on a charge e placed at the centre, and on

the pole at **A**, a couple acting on the charged sphere whose components along the axes of x, y, z are respectively

$$\frac{1}{3} ema^2 \left\{ u \frac{d^2}{dx^2} \frac{1}{r} + v \frac{d^2}{dx\,dy} \frac{1}{r} + w \frac{d^2}{dx\,dz} \frac{1}{r} \right\},$$

$$\frac{1}{3} ema^2 \left\{ u \frac{d^2}{dx\,dy} \frac{1}{r} + v \frac{d^2}{dy^2} \frac{1}{r} + w \frac{d^2}{dy\,dz} \frac{1}{r} \right\},$$

$$\frac{1}{3} ema^2 \left\{ u \frac{d^2}{dx\,dz} \frac{1}{r} + v \frac{d^2}{dy\,dz} \frac{1}{r} + w \frac{d^2}{dz^2} \frac{1}{r} \right\},$$

where u, v, w are the components of the velocity of the centre of the sphere relative to the pole.

The moment of momentum remains unaltered when the charged sphere is rotated about a diameter: thus rotation of the sphere leaves the momentum in the electromagnetic field unaltered. Hence the momentum of the material systems in the field must remain unchanged, or the forces between the point and the rotating sphere must be equal and opposite.

Momentum due to Moving Charges.—If a charge e is moving with uniform velocity w parallel to the axis of z, then, if α, β, γ are the components of the magnetic force, f, g, h the components of the polarization in the dielectric, then, when the field has become steady, if we neglect squares and higher powers of w/V, where V is the velocity of light, we have

$$f = -\frac{e}{4\pi} \frac{d}{dx} \frac{1}{r}, \quad g = -\frac{e}{4\pi} \frac{d}{dy} \frac{1}{r} \quad h = -\frac{e}{4\pi} \frac{d}{dz} \frac{1}{r},$$

$$\alpha = 4\pi wg, \qquad \beta = -4\pi wf, \qquad \gamma = 0 ;$$

therefore

$$\alpha = -ew \frac{d}{dy} \frac{1}{r}, \quad \beta = ew \frac{d}{dx} \frac{1}{e}, \qquad \gamma = 0.$$

Here x, y, z are the coordinates of the point at which the magnetic force is (α, β, γ) and the dielectric polarization f, g, h. Hence by the rule the components p, q, r of the momentum per unit volume at **P** are given by the equations

$$p = \beta h - \gamma g = -\frac{e^2 w}{4\pi} \frac{xz}{r^6},$$

$$q = \gamma f - \alpha h = \frac{e^2 w}{4\pi} \frac{yz}{r^6},$$

$$r = \alpha g - \beta f = \frac{e^2 w}{4\pi} \frac{(x^2 + y^2)}{r^6}.$$

Hence the momentum contained in the space between the surface of the moving charged sphere (whose radius we represent by a) and a concentric sphere of radius R is equivalent to a momentum through the centre of the sphere parallel to z, the direction of motion of the sphere, and equal to $\frac{2}{3}e^2w\left(\frac{1}{a}-\frac{1}{R}\right)$. The whole momentum outside the sphere is $\frac{2}{3}e^2\frac{w}{a}$.

If the moving electrified body were ellipsoidal instead of spherical, then if the velocities parallel to the axes of the ellipsoid were u, v, w, the momenta parallel to the axes of x, y, z would be Au, Bv, Cw respectively, where A, B, C are not equal. Thus in this case the momentum would not be in the direction of the velocity unless the velocity were parallel to a principal axis of the ellipsoid : except in this case there would be a couple acting on the ellipsoid. For let O be the centre of the ellipsoid at any time, after a time δt let the centre come to O′; let I be the momentum at t acting in the direction OL ; then at $t+\delta t$ the momentum is I in the direction O′L′, O′L′ being parallel to OL. Thus the momentum in the field at the time $t+\delta t$ is not the same as it was at the time t, the difference is equivalent to the moment of momentum due to $-$I at O and I at O′. The moment of momentum lost by the field must be transferred to the moving body ; hence this will be acted upon by a couple in the plane LOO′. The components of the moment of this couple parallel to the axes of x, y, z are respectively

$$(B-C)vw, \quad (C-A)uw, \quad (A-B)uv.$$

Charged Sphere rotating about an Axis.—If a uniformly charged sphere of radius a is rotating with angular velocity $\boldsymbol{\omega}$ about an axis through its centre parallel to z, we can easily prove that the components of magnetic force at a point distant $r\,(r>a)$ from the centre of the sphere are given by the equations

$$\alpha = \frac{e\omega a^2}{3}\frac{d^2}{dx\,dz}\frac{1}{r},$$

$$\beta = \frac{e\omega a^2}{3}\frac{d^2}{dy\,dz}\frac{1}{r},$$

$$\gamma = \frac{e\omega a^2}{3}\frac{d^2}{dz^2}\frac{1}{r} ;$$

e being the charge on the sphere.

Since the components f, g, h of the electric polarization are given by the equations

$$f = -\frac{e}{4\pi}\frac{d}{dx}\frac{1}{r}, \quad g = -\frac{e}{4\pi}\frac{d}{dy}\frac{1}{r}, \quad h = -\frac{e}{4\pi}\frac{d}{dz}\frac{1}{r},$$

we see that p, q, r the components of the density of the momentum are given by

$$p = \frac{a^2 e^2 \omega y}{3 \times 4\pi r^6}, \quad q = -\frac{a^2 e^2 \omega x}{3 \times 4\pi r^6}, \quad r = 0;$$

thus the momentum at any point is parallel to the direction in which that point would move if the point were rigidly connected with the sphere.

The moment of momentum in the field about the axis of either x or y vanishes; about the axis of z it is equal to

$$\frac{2}{9}\frac{a^2 e^2 \omega}{a}.$$

If the shell had a mass M, the moment of momentum of the shell would be $\frac{2}{3}a^2\omega M$. Thus when the charge is rotating, the effective mass is $\frac{1}{3}\frac{e^2}{a}$. While when the motion is translatory the effective mass is $\frac{2}{3}e^2/a$, *i.e.* twice that for rotatory motion. In the case of an ellipsoid charged with electricity, the moment of momentum in the field corresponding to rotations ω_x, ω_y, ω_z about the axes of x, y, z, would be of the form $A'\omega_x$, $B'\omega_y$, $C'\omega_z$ where A', B', C' are different. Thus, unless the axis of rotation coincided with one of the principal axes, the axis of the moment of momentum would not coincide with the axis of rotation and the motion could not be steady.

Momentum in the Field before the Steady State is reached.—Hitherto we have considered the momentum when the field due to the moving charged particle has reached a steady state; we shall now consider the distribution of momentum before the steady state is reached. Let us suppose that a charged sphere is suddenly set in motion by an impulse parallel to the axis of z, then at a time t after the impulse has ceased to act, the distribution of magnetic force is as follows. Let O be the point from which the centre of the charged sphere starts. With centre O and radii Vt, $Vt + \delta$, where δ is small, describe two spherical surfaces; then outside the outer of these surfaces there is no magnetic force,

and therefore no momentum; in the region between the spheres the magnetic force at a point P has its components

Fig. 2.
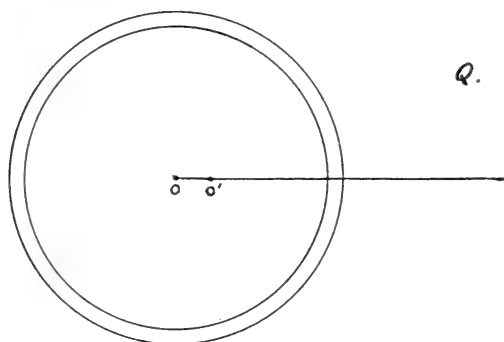
α, β, γ given by the equations

$$\alpha = \frac{ewy}{\delta \,.\, \mathrm{OP^2}}, \quad \beta = -\frac{ewx}{\delta \,.\, \mathrm{OP^2}}, \quad \gamma = 0 \,;$$

the components f, g, h of the dielectric polarization, which are required for calculating the momentum, may for this purpose, as we are neglecting terms involving w^2, be taken as unaltered by the motion and given by the equations

$$f = \frac{e}{4\pi} \frac{x}{\mathrm{OP^3}}, \quad y = \frac{e}{4\pi} \frac{y}{\mathrm{OP^3}}, \quad z = \frac{e}{4\pi} \frac{z}{\mathrm{OP^3}}.$$

Hence, calculating the momentum in the shell by the ordinary rule, we find that it is parallel to z and equal to

$$\frac{2}{3} \frac{e^2 w}{\mathrm{OB}},$$

where OB is the radius of the inner surface of the shell. Inside the inner shell the steady field has been established, so that if O′ is the position of O at the time t, the magnetic force at P′ will have for components

$$\alpha = ew \frac{y}{\mathrm{O'P'^3}}, \quad \beta = -ew \frac{x}{\mathrm{O'P'^3}}, \quad \gamma = 0.$$

As we are neglecting squares of w and $\mathrm{OO'} = wt$, we may in calculating the momentum suppose that O′ coincides with O: hence by the result given on p. 337, the momentum inside the sphere is parallel to z and equal to

$$\frac{2}{3} e^2 w \left\{ \frac{1}{a} - \frac{1}{\mathrm{OB}} \right\},$$

where a is the radius of the charged sphere. Thus the total momentum, viz., the sum of that inside the inner surface of the shell and the shell itself, is equal to

$$\frac{2}{3} e^2 w \left\{ \frac{1}{a} - \frac{1}{OB} \right\} + \frac{2}{3} e^2 w \frac{1}{OB} = \frac{2}{3} \frac{e^2 w}{a};$$

thus it is constant and equal to that in the field when the steady state is reached and the pulse has passed off to an infinite distance; the pulse in passing over any region leaving part of its momentum behind it.

When a particle which has been in uniform motion is suddenly stopped, then the state of things at a time t after the stoppage is as follows:—Let O be the centre of the sphere now at rest; describe two spheres centre O, radii Vt, $Vt + \delta$; then inside the inner sphere there is no magnetic force; in the space between the spheres the magnetic force is given by the equations

$$\alpha = -\frac{ewy}{\delta . OP^2}, \quad \beta = \frac{ewx}{\delta . OP^2}, \quad \gamma = 0,$$

while outside the outer sphere the magnetic force is the same as that before the stoppage of the particle and is given by

$$\alpha = \frac{ewy}{OP^3}, \quad \beta = -\frac{ewx}{OP^3}, \quad \gamma = 0,$$

We can easily see that the momentum in the shell is in this case equal and opposite to that in the space surrounding it, so that the total momentum in the field vanishes as soon as the charged point is reduced to rest.

Let us now consider the case of a charged particle suddenly set in motion in an external magnetic field. Suppose that this field is uniform and parallel to the axis of x, let it be due to two parallel plates of positive and negative magnetism, the plates being infinite and parallel to yz. Let us find the momentum due to the external magnetic field before the pulse started by the sudden movement of the sphere has reached the layers of magnetism. To calculate this momentum, we have to know the distribution of the dielectric polarization after the sphere has been started. Now in the pulse there is an additional polarization whose components f', g', h' are

$$f' = \frac{ew}{4\pi V\delta} \frac{xz}{r^3}, \quad g' = \frac{ew}{4\pi V\delta} \frac{yz}{r^3}, \quad h' = \frac{-ew}{4\pi V\delta} \frac{(x^2 + y^2)}{r^3},$$

where V is the velocity of light. The components of momentum due to a field H parallel to x and this polarization

are respectively :—

$$0, \quad \frac{\mathrm{H}ew}{4\pi\mathrm{V}\delta}\frac{(x^2+y^2)}{r^3}, \quad \frac{\mathrm{H}ew}{4\pi\mathrm{V}\delta}\frac{yz}{r^3}.$$

Integrating through the shell the components parallel to x and z vanish, and that parallel to y becomes

$$\frac{2}{3}\frac{\mathrm{H}ew}{\mathrm{V}}.\,\mathrm{OA};$$

where OA is the inner radius of the sphere, and is equal to $\mathrm{V}t$. Hence the momentum in the shell parallel to y is equal to

$$\frac{2}{3}\,\mathrm{H}ew\,.\,t.$$

The electrostatic field inside the shell is at the time t that arising from a point O′ which is not at the centre of the shell, but such that $\mathrm{OO}' = wt$. Calculating the momentum due to this field and the external magnetic force H, we find that is $\frac{\mathrm{H}}{3}\,\mathrm{H}e\mathrm{OO}' = \frac{1}{3}\,\mathrm{H}ewt$; hence the total momentum on the shell and inside it is

$$\mathrm{H}ewt.$$

Thus on the hypothesis that the pulse formed in the magnetic field is the same as that formed when the external magnetic field is absent, we conclude that the momentum in the field at right angles to the direction of the magnetic force, and also to the direction of motion of the charged particle, is increasing at the rate $\mathrm{H}ew$. By the principle of the conservation of momentum, the material system, *i. e.* the charged body and the magnet, must be losing momentum at that rate, *i. e.* they must be acted on by a force parallel to y equal to $-\mathrm{H}ew$. Now the charged particle is moving in a magnetic field, and so is acted on by a force parallel to y equal to $-\mathrm{H}ew$; while, since the wave of magnetic force has not yet reached the magnets, the latter do not experience any mechanical force. Thus the whole force acting on the system is such as to give to the system the momentum lost by the field. Thus the view that the wave of magnetic and electric force produced by the sudden starting of the charged point is not affected by the magnetic field is consistent with the principle of the Conservation of Momentum. We notice that the momentum arising from the external field vanishes when $t = 0$. Thus the momentum at that time is not affected

by the magnetic field, so that a given impulse will produce the same velocity in the particle whatever the magnitude of the field. The magnetic field affects the subsequent motion of the particle, but not its initial velocity or the pulses produced when it is started.

As another example of the principle of the Conservation of Momentum, let us consider the case where the electrified particle is suddenly started with a velocity w parallel to the axis of z in a uniform electric field parallel to x, the field being supposed to be due to uniformly electrified plates parallel to yz. If we calculate by the rule given above the part of the momentum arising from the electric field, we shall find that while the momentum in any direction vanishes the moment of momentum is finite.

If X is the electric force, K and μ the specific inductive capacity and magnetic permeability of the dielectric, then the moment of momentum about the axis of y in the region inside the pulse is equal to

$$\frac{\mu \mathrm{KX} ew}{6} \ (\mathrm{OB}^2 - a^2),$$

where OB is the inner radius of the pulse; it is equal to $\mathrm{V}t$, where V is the velocity of light and t the interval which has elapsed since the particle was started.

The moment of momentum in the pulse itself is equal to

$$\frac{\mu \mathrm{K} \cdot \mathrm{X} ew}{3} \ \mathrm{OB}^2 \ ;$$

hence, the total moment of momentum about y is equal to

$$\frac{\mu \mathrm{K} \cdot \mathrm{X} ew}{2} \ \mathrm{OB}^2 - \tfrac{1}{6} \mu \mathrm{K} \cdot \mathrm{X} ewa^2$$

$$= \tfrac{1}{2} \mu \mathrm{K} \cdot \mathrm{X} ew \mathrm{V}^2 t^2 - \tfrac{1}{6} \mu \mathrm{K} \cdot \mathrm{X} ewa^2.$$

The rate of increase of this, since $\mu \mathrm{K} = \dfrac{1}{\mathrm{V}^2}$, is $\mathrm{X} ewt$. Thus to keep the moment of momentum of the whole system constant, the material parts of the system, *i. e.* the charged point and the plates, must be acted on by a couple about the axis of y whose moment is $-\mathrm{X} ewt$. We can see that this is the case, for suppose O is the position of the charged body before it is set in motion, O′ the position after a time t, then, when the particle was at O the force on the plates was equal and opposite to that on the particle, it therefore passed through O and was equal to Xe. When, however, the particle is set in

motion the force on the electrified plates will not alter until the pulse reaches them, and so will continue to pass through O. The force on the point will, however, pass through O'. Thus we get a couple whose moment is XeOO' or Xewt, *i. e.* the couple required to keep the moment of momentum of the system constant.

Momentum when a charged point at rest is placed in the field due to a moving charged particle.—If the external electrostatic field in the last example had not been uniform but that due, say, to a charged particle at P, then we should have found that the momentum as well as the moment of momentum in the field would change as the pulse spreads out. In this case the force acting on the moving particle when it is at O' is $ee'/$O'P^2, and acts along O'P, while the force acting on P is $ee'/$OP^2 along OP, O being the initial position of O'. In this case, since OP is not in general equal to O'P the forces on O' and P are not equal in magnitude, so that there is a resultant force acting on the system.

If e is the charge on the moving point O, u, v, w the components of its velocity, then α, β, γ the components of the magnetic force produced by it at a point P whose coordinates are x, y, z, are given by the equations

$$\alpha = e\left(v\frac{d}{dz}\frac{1}{r} - w\frac{d}{dy}\frac{1}{r}\right)$$
$$\beta = e\left(w\frac{d}{dx}\frac{1}{r} - u\frac{d}{dz}\frac{1}{r}\right)$$
$$\gamma = e\left(u\frac{d}{dy}\frac{1}{r} - v\frac{d}{dx}\frac{1}{r}\right),$$

where r is the distance of P from O. If Q is the position of a charge e' at rest, then f, g, h, the components of the electric polarization at P due to this charge, are given by the equations

$$f = -\frac{e'}{4\pi}\frac{d}{dx}\frac{1}{r'}, \quad g = -\frac{e'}{4\pi}\frac{d}{dy}\frac{1}{r'}, \quad h = -\frac{e'}{4\pi}\frac{d}{dz}\frac{1}{r'},$$

r' being equal to PQ.

U, the x component of the momentum in the field, is given by the equation

$$U = \mu\iiint(\gamma g - \beta h)dx\,dy\,dz$$
$$= \frac{\mu ee'}{4\pi}\iiint\left\{\left(w\frac{d}{dx}\frac{1}{r} - u\frac{d}{dz}\frac{1}{r}\right)\frac{d}{dz}\frac{1}{r'} - \left(u\frac{d}{dy}\frac{1}{r}\right.\right.$$
$$\left.\left. - v\frac{d}{dx}\frac{1}{r}\right)\frac{d}{dy}\frac{1}{r'}\right\}dx\,dy\,dz.$$

2 A 2

We can easily show that

$$\iiint \frac{d}{dx}\frac{1}{r}\cdot\frac{d}{dz}\frac{1}{r'}\,dx\,dy\,dz = 2\pi\frac{d^2\mathrm{R}}{dx_2 dz_2}$$

$$\iiint \frac{d}{dz}\frac{1}{r}\frac{d}{dz}\frac{1}{r'}\,dx\,dy\,dz = 2\pi\frac{d^2\mathrm{R}}{dz_2^2},$$

where R is the distance OQ between the fixed and moving points, and x_2, y_2, z_2 are the coordinates of Q. Using these values for the integrals we find

$$\mathrm{U} = -\frac{\mu ee'}{2}\left\{ w\frac{d^2\mathrm{R}}{dx_2\,dz_2} + v\frac{d^2\mathrm{R}}{dx_2\,dy_2} - u\left(\frac{d^2\mathrm{R}}{dy_2^2} + \frac{d^2\mathrm{R}}{dz_2^2}\right)\right\}$$

$$= -\frac{\mu ee'}{2}\left(w\frac{d^2\mathrm{R}}{dx_2\,dz_2} + v\frac{d^2\mathrm{R}}{dx_2\,dy_2} + u\frac{d^2\mathrm{R}}{dx_2^2}\right) + \mu ee'\frac{u}{\mathrm{R}}$$

$$= -\frac{\mu ee'}{2}\frac{d}{dx_2}\left(u\frac{d\mathrm{R}}{dx_2} + v\frac{d\mathrm{R}}{dy_2} + w\frac{d\mathrm{R}}{dz_2}\right) + \mu ee'\frac{u}{\mathrm{R}}\cdot$$

Similarly,

$$\mathrm{V} = -\frac{\mu ee'}{2}\frac{d}{dy_2}\left(u\frac{d\mathrm{R}}{dx_2} + v\frac{d\mathrm{R}}{dy_2} + w\frac{d\mathrm{R}}{dz_2}\right) + \mu ee'\frac{v}{\mathrm{R}}$$

$$\mathrm{W} = -\frac{\mu ee'}{2}\frac{d}{dz_2}\left(u\frac{d\mathrm{R}}{dx_2} + v\frac{d\mathrm{R}}{dy_2} + w\frac{d\mathrm{R}}{dz_2}\right) + \mu ee'\frac{w}{\mathrm{R}}\cdot$$

We notice that

$$\frac{d\mathrm{U}}{dx_2} + \frac{d\mathrm{V}}{dy_2} + \frac{d\mathrm{W}}{dz_2} = 0.$$

We shall now show that the line of the resultant momentum passes through Q the fixed point. Take Q as origin, let x, y, z be the coordinates of any point in the field. Then the moment of momentum of the field about the axis of z is equal to

$$\mu\iiint \{(\gamma g - \beta h)y - (\alpha h - \gamma f)x\}dx\,dy\,dz$$

$$= \mu\iiint \gamma(xf + yg + zh)dx\,dy\,dz$$

$$- \mu\iiint h(x\alpha + y\beta + z\gamma)\,dx\,dy\,dz;$$

each of the integrals is equal to

$$\frac{1}{2}\frac{ee'}{\mathrm{R}}(vx_1 - uy_1),$$

where R is the distance OQ between the fixed and moving

points and $x_1\, y_1\, z_1$ the coordinates of O when Q is taken as origin. Thus the moment of momentum about the axis of z vanishes. Similarly we may show that it vanishes about the axes of x and y, and thus the resultant momentum passes through Q the fixed point.

This is only a particular case of the much more general proposition that the resultant of the momentum due to a stationary electrified point and a magnetic field passes through the fixed point when the magnetic field is continuous and derivable from a vector potential. For, taking the fixed point as origin, Γ the moment of momentum about the axis of z is given by the equation

$$\Gamma = \mu \iiint \{(\gamma g_1 - \beta h_1)y - (\alpha h_1 - \gamma f_1)x\}\, dx\, dy\, dz,$$

where f_1, g_1, h_1 are the components of the electric polarization due to the charge, and are respectively equal to

$$-\frac{e}{4\pi}\left\{\frac{d}{dx},\ \frac{d}{dy},\ \frac{d}{dz}\right\}\frac{1}{r}.$$

Substituting these values we find

$$\Gamma = \frac{\mu e}{4\pi}\iiint\left[\left(\beta\frac{d}{dz}\frac{1}{r} - \gamma\frac{d}{dy}\frac{1}{r}\right)y - \left(\gamma\frac{d}{dx}\frac{1}{r}\right.\right.$$
$$\left.\left. -\alpha\frac{d}{dz}\frac{1}{r}\right)x\right]dx\, dy\, dz.$$

Integrating by parts, we find if α, β, γ are continuous, and if at great distances from the origin they vary more rapidly than $1/r^2$,

$$\Gamma = \frac{\mu e}{4\pi}\iiint\frac{1}{r}\left\{\frac{d}{dy}(y\gamma) + \frac{d}{dx}(x\gamma) - \frac{d}{dz}(\beta y + \alpha x)\right\}dx\, dy\, dz$$
$$= \frac{\mu e}{4\pi}\iiint\frac{1}{r}\left\{y\left(\frac{d\gamma}{dy} - \frac{d\beta}{dz}\right) + 2\gamma - x\left(\frac{d\alpha}{dz} - \frac{d\gamma}{dx}\right)\right\}dx\, dy\, dz.$$

Putting

$$\alpha = \frac{d\mathrm{H}}{dy} - \frac{d\mathrm{G}}{dz},\quad \beta = \frac{d\mathrm{F}}{dz} - \frac{d\mathrm{H}}{dx},\quad \gamma = \frac{d\mathrm{G}}{dx} - \frac{d\mathrm{F}}{dy},$$

we have

$$\Gamma = \frac{\mu e}{4\pi}\iiint\frac{1}{r}\left\{x\nabla^2\mathrm{G} + 2\frac{d\mathrm{G}}{dx} - y\nabla^2\mathrm{F} - 2\frac{d\mathrm{F}}{dy}\right.$$
$$\left. -\left(x\frac{d\mathrm{J}}{dy} - y\frac{d\mathrm{J}}{dx}\right)\right\}dx\, dy\, dz,$$

where

$$\mathrm{J} = \frac{d\mathrm{F}}{dx} + \frac{d\mathrm{G}}{dy} + \frac{d\mathrm{H}}{dz}.$$

The terms in J cancel on integration, and we have

$$\Gamma = \frac{\mu e}{4\pi} \iiint \frac{1}{r} \nabla^2 (x\mathrm{G} - y\mathrm{F}) dx\, dy\, dz$$

$$= \mu e \text{ value of } (x\mathrm{G} - y\mathrm{F}) \text{ at the origin}$$

$$= 0.$$

Hence the moment of the momentum about any axis through the electrified point vanishes, so that the resultant momentum passes through the point.

The magnetic force due to a moving particle is the same as that due to an element of current at the particle. The direction of the current being parallel to the direction of motion of the particle, and the intensity ι of the current being determined by the relation $\iota ds = e$ (velocity of particle), ds is the length of the element of current. Hence we may apply the preceding results to find the momentum in the field due to the mutual action of any system of currents and a fixed electrified point; and hence it follows that the resultant momentum passes through the electrified point.

We have previously seen that when the magnetic field is due to a system of magnetic poles in a non-magnetic medium, the momentum in the field due to the action of the magnetic field on a fixed electrified point, has zero resultant but finite moment of momentum, and hence can be represented by a momentum passing through the point and an equal and opposite momentum through some point of the system producing the magnetic force. In the case when the magnetic field is due to currents the second momentum, *i. e.* that passing through the system producing the magnetic field, is absent. This result seems at first sight not in accordance with the principle that we can replace a magnet, as in Ampère's hypothesis, by a system of electric currents; for in this case there would be a finite resultant momentum, while the moment of momentum about any line passing through the fixed electrified point would vanish. The explanation of the discrepancy is that in calculating the momentum due to the magnetic poles, we have supposed that the poles were surrounded by a medium which was non-magnetic, *i. e.* that the magnetic induction was equal to the magnetic force throughout the field. If the magnetic induction is not equal to the magnetic force, the results of our investigation would have to be modified, for the momentum is proportional to the magnetic induction and not to the magnetic force. When we allow for this difference, we see that in the case of the magnets, as well as in that of the currents, the

momentum in the field has a finite resultant passing through
the fixed electrified point. For consider a magnetized element
AB (fig. 3) and an electrified point at P, then if m is the

Fig. 3.
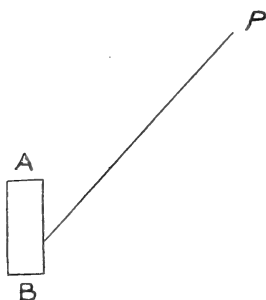
strength of the pole at A, $-m$ at B, the momentum in the
field calculated on the assumption that the magnetic force is
equal to the magnetic induction at every point in the field,
is made up of a moment of momentum em along AP and a
moment of momentum $-em$ along BP. The resultant of
these is a moment of momentum $\dfrac{em\text{AB}\sin\theta}{\text{OP}}$ at right angles
to OP, O being the middle point of AB and θ the angle
AOP. This moment of momentum is equivalent to a
momentum $\dfrac{em\text{AB}\sin\theta}{\text{OP}^2}$ through P at right angles to
the plane of the paper downwards and a momentum
$\dfrac{em\text{AB}\sin\theta}{\text{OP}^2}$ through O at right angles to the plane of the
paper upwards. In the magnet, however, the magnetic
induction is not equal to the magnetic force, so to get the
momentum we must add to the preceding momentum a
momentum equal to (difference between magnetic induction
and magnetic force in the magnet) × (electric polarization at
right angles to AB) × volume of magnet. The direction
of this momentum is at right angles to the plane of the paper
downwards. Now the difference between magnetic induction
and magnetic force $=4\pi\text{I}$, where I is the intensity of
magnetization, the polarization at right angles to AB is
$\dfrac{e}{4\pi}\dfrac{\sin\theta}{\text{OP}^2}$; hence the additional momentum is $\dfrac{\text{I}e\sin\theta}{\text{OP}^2}$ (volume
of magnet), but I × (volume of magnet)$=m\,.\,\text{AB}$. Hence we
see that this momentum just balances that acting through

the magnet, and we are left with the momentum through the fixed point, *i. e.* we get the same result as if we replace the magnet by its equivalent currents.

The following illustration will show the difference in the physical effects produced by the differences in the distribution of momentum just considered. Suppose AB is a magnetized piece of soft iron, **P** the charged point, then we have seen that the momentum in the field has for resultant a momentum I through P perpendicular to the plane containing AB and P. Suppose that AB is demagnetized by tapping, there will then be no momentum in the field; hence by the principle of the conservation of momentum the momentum in the field before the tapping must be transferred to the material system in the field. Thus the charged point will acquire momentum I, but the momentum of AB will remain unchanged. If, however, the distribution of momentum in the field were equivalent to I at P and −I at the magnet, then, when the magnet was tapped I would gain momentum as before ; but in this case the magnet would gain an equal momentum in the opposite direction. Thus in this case the system would behave as if the point and magnet were acted on by equal and opposite impulses, while in the former case, which is that given by the rule we have used for finding the momentum, the charged particle would alone be acted upon by an impulse, the magnet remaining undisturbed.

Connexion between the Momentum and the Vector Potential.— From the investigation on page 344, it follows that U, V, W, the components of the momentum due to magnetic force produced by an element of current ιds and the electric field due to a charge e' at P, are given by the equations

$$U = \mu e' \frac{\iota}{R} \frac{dx}{ds}\, ds - \tfrac{1}{2}\mu e' \iota \left\{ \frac{dx}{ds}\frac{d^2 R}{dx^2} + \frac{dy}{ds}\frac{d^2 R}{dy^2 dx} + \frac{dz}{ds}\frac{d^2 R}{dx dz} \right\} ds$$

$$= \mu e' \frac{\iota}{R}\frac{dx}{ds}\, ds - \tfrac{1}{2}\mu e' \iota \left\{ \frac{d}{ds}\frac{dR}{dx} \right\} ds,$$

with similar expressions for V and W ; here R is the distance between the current element and the charged point. Now if we find the momentum at P due to a closed circuit from this expression by integration, we see that the second term vanishes, and $\overline{U}$ the momentum parallel to x acting through P is equal to

$$\mu e' \int \frac{\iota}{R}\frac{dx}{ds}ds ; \quad \text{but} \quad \mu \int \frac{\iota}{R}\frac{dx}{ds}ds$$

is equal to the x component at P of the vector potential due to the current. Hence we see that the vector potential at any point is the momentum due to the magnetic force produced by the system giving the vector potential and the electric field due to unit charge at the point. We thus get a physical interpretation for the vector potential, and see that instead of being merely an analytical device, it represents a most important physical property of the system. We shall show later on that the principle of the conservation of momentum leads at once to the ordinary equations for the induction of currents.

Momentum due to Two Moving Particles.—Let P and Q be the moving particles, e and e' their charges, R the distance between them, u_1, v_1, w_1 ; u_2, v_2, w_2 the components of their velocities, then from the results proved on page 344 we see that the momentum in the field is equivalent to the following distribution.

A momentum passing through **P** parallel to the axis of x equal to

$$\frac{2}{3}\frac{e^2\mu}{a}\,u_1 + \frac{\mu ee'}{R}\,u_2 - \tfrac{1}{2}\mu e'e\left(u_2\,\frac{d^2R}{dx_2{}^2} + v_2\,\frac{d^2R}{dx_2 dy_2} + w_2\,\frac{d^2R}{dx_2 dz_2}\right),$$

and a momentum through Q parallel to x equal to

$$\frac{2}{3}\frac{\mu e'^2}{b}\,u_2 + \frac{\mu ee'}{R}\,u_1 - \tfrac{1}{2}\mu ee'\left(u_1\,\frac{d^2R}{dx_1{}^2} + v_1\,\frac{d^2R}{dx_1 dy_1} + w_1\,\frac{d^2R}{dx_1 dz_1}\right);$$

a and b are the radii of the spheres whose centres are P and Q over which the charges are spread, and $x_1, y_1, z_1, x_2, y_2, z_2$ are the coordinates of P and Q respectively. We have of course

$$\frac{d^2R}{dx_2{}^2} = \frac{d^2R}{dx_1{}^2} \qquad \text{and} \qquad \frac{d^2R}{dx_2 dy_2} = \frac{d^2R}{dx_1 dy_1}.$$

There are, of course, momenta through P and Q parallel to the axes of y and z respectively given by symmetrical expressions. These expressions enable us to find the velocities acquired when a charged particle P at rest in the presence of another, Q, is struck by an impulse. Let us suppose that the particle P initially at rest is struck by an impulse I_x parallel to x, then the momentum in the charged particles and in the field must be equivalent to this impulse. Hence, if $u_1, v_1, w_1, u_2, v_2, w_2$ are the velocities acquired by P and Q

respectively, m and m' the mechanical masses of these particles, we have

$$P_x = m u_1 + \frac{2}{3}\frac{\mu e^2}{a} u_1 + R u_2 - (\alpha u_2 + \beta v_2 + \gamma w_2),$$

$$0 = \left(m' + \frac{2}{3}\frac{\mu e'^2}{b}\right) u_2 + R u_1 - (\alpha u_1 + \beta v_1 + \gamma w_1),$$

$$0 = \left(m + \frac{2}{3}\frac{\mu e^2}{a}\right) v_1 + R v_2 - (\beta u_2 + \delta v_2 + \epsilon w_2),$$

$$0 = \left(m' + \frac{2}{3}\frac{\mu e'^2}{b}\right) v_2 + R v_1 - (\beta u_1 + \delta v_1 + \epsilon w_1),$$

$$0 = \left(m + \frac{2}{3}\frac{\mu e^2}{a}\right) w_1 + R w_2 - (\gamma u_2 + \epsilon v_2 + \eta w_2),$$

$$0 = \left(m' + \frac{2}{3}\frac{\mu e'^2}{b}\right) w_2 + R w_1 - (\gamma u_1 + \epsilon u_1 + \eta w_1);$$

where R $(\alpha, \beta, \gamma, \delta, \epsilon, \eta)$ are written for $\mu ee'/R$,

$$\tfrac{1}{2}\mu ee'\left(\frac{d^2 R}{dx_2^2}\quad \frac{d^2 R}{dx_2 dy_2}\quad \frac{d^2 R}{dx_2 dz_2}\quad \frac{d^2 R}{dy_2^2}\quad \frac{d^2 R}{dy_2 dz_2}\quad \frac{d^2 R}{dz_2^2}\right)$$

respectively.

Writing l, m, n for the direction cosines of the line joining the electrified points and q for $\mu^2 e^2 e'^2/R^2 M.M'$, where

$$M = m + \frac{2}{3}\frac{\mu e^2}{a}, \qquad M' = m' + \frac{2}{3}\frac{\mu e^2}{b},$$

we find

$$u_1 = \frac{P}{M}\left(1 + \frac{l^2 q}{1-q}\right),$$

$$v_1 = \frac{P}{M}\frac{lmq}{1-q},$$

$$w_1 = \frac{P}{M}\frac{lnq}{1-q};$$

$$u_2 = -\frac{\mu ee'}{2RM'}\frac{P}{M}\left(1 + \frac{l^2(1+q)}{1-q}\right),$$

$$v_2 = -\frac{\mu ee'}{2RM'}\frac{P}{M}\frac{lm(1+q)}{1-q},$$

$$w_2 = -\frac{\mu ee'}{2RM'}\frac{P}{M}\frac{ln(1+q)}{1-q}.$$

We see that u_2, v_2, w_2 are large compared with v_1, w_1, so

that the deflexion of the velocity of the particle struck from the line of impulse is not so marked as the motion imparted to the charged particle in its neighbourhood. The deflexion of the first particle may be supposed to occur as follows: when the particle is first acted on by the impulse it starts parallel to the impulse with a velocity P/M; a pulse of strong electric force spreads out from the particle, after a time this reaches the second particle and starts it impulsively; the sudden starting of the second particle produces pulses of electric force which travel on and strike the first particle *deflecting it from* the original direction.

The momenta of the particles may be expressed in the following way. $U_1, V_1, W_1, U_2, V_2, W_2$ being the components of the momenta of the first and second particles: $u_1, v_1, w_1, u_2, v_2, w_2$ the components of their velocities:—

$$U_1 = Mu_1 + \frac{ee'\mu}{2R} u_2 + \frac{ee'\mu}{2R} l(lu_2 + mv_2 + nw_2),$$

$$V_1 = Mv_1 + \frac{ee'\mu}{2R} v_2 + \frac{ee'\mu}{2R} m(lu_2 + mv_2 + nw_2),$$

$$W_1 = Mw_1 + \frac{ee'\mu}{2R} w_2 + \frac{ee'\mu}{2R} n(lu_2 + mv_2 + nw_2),$$

$$U_2 = M'u_2 + \frac{ee'}{2R} \mu u_1 + \frac{ee'}{2R} \mu l(lu_1 + mv_1 + nw_1),$$

$$V_2 = M'v_2 + \frac{ee'}{2R} \mu v_1 + \frac{ee'}{2R} \mu m(lu_1 + mv_1 + nw_1),$$

$$W_2 = M'w_2 + \frac{ee'}{2R} \mu w_1 + \frac{ee'}{2R} \mu n(lu_1 + mv_1 + nw_1).$$

Take the case when the particles are moving with the same velocity; then, unless the momentum of the system is in the direction of the velocity, the line of action of the resultant momentum will change: thus the moment of momentum in the field about axes of x, y, z will be changing at the rate

$$(W_1 + W_2)v - (V_1 + V_2)w ; \quad (U_1 + U_2)w - (W_1 + W_2)u ;$$
$$(V_1 + V_2)u - (U_1 + U_2)v.$$

Since the momentum of the system consisting of the field and particle remains constant, the particle must be acted on by couples about the axes of x, y, z, equal and opposite to the rate of increase of the moment of momentum in the field. Hence, substituting for the values of U, V, W, we find that

when the particles are moving with the same velocities they are acted on by couples whose moments round x, y, z are

$$\frac{ee'\mu}{R}(lu+mv+nw)(mw-nv),$$

$$\frac{ee'\mu}{R}(lu+mv+nw)(nu-lw),$$

$$\frac{ee'\mu}{R}(lu+mv+nw)(lv-mu).$$

These couples vanish if

$$\frac{u}{l} = \frac{v}{m} = \frac{w}{n},$$

i. e., if the particles are moving along the line joining them ; and when

$$lu+mv+nw=0,$$

when the direction of motion is at right angles to the line, in all other cases there is a couple tending to twist the particles so that the line joining them gets parallel to the direction of motion if the charges are of the same sign, or perpendicular to the direction of motion if the charges are of opposite signs.

Kinetic Energy of a System consisting of two moving particles.—If T is the Kinetic Energy,

$$T = \tfrac{1}{2}(u_1 U_1 + v_1 V_1 + w_1 W_1 + u_2 U_2 + v_2 V_2 + w_2 W_2)$$

$$= \tfrac{1}{2}M(u_1{}^2 + v_1{}^2 + w_1{}^2) + \tfrac{1}{2}M'(u_2{}^2 + v_2{}^2 + w_2{}^2) + \frac{\mu ee'}{R}(u_1 u_2 + v_1 v_2 + w_1 w_2)$$

$$- \frac{\mu ee'}{2}\left(u_1\frac{d}{dx_1} + v_1\frac{d}{dy_1} + w_1\frac{d}{dz_1}\right)\left(u_2\frac{d}{dx_1} + v_2\frac{d}{dy_1} + w_2\frac{d}{dz_1}\right)R.$$

By Lagrange's equations we have

$$\frac{d}{dt}\frac{dT}{du_1} - \frac{dT}{dx_1} = \text{force parallel to } x_1 \text{ acting on the first particle.}$$

Hence the effect of the motion may be represented by a force parallel to x_1 equal to

$$\frac{dT}{dx_1} - \frac{d}{dt}\frac{dT}{du_1}.$$

When the velocities of the particles are constant, this may be written as

$$\mu ee'\left\{ v_1\left(v_2\frac{d}{dx_1}\frac{1}{R} - u_2\frac{d}{dy_1}\frac{1}{R}\right) - w_1\left(u_2\frac{d}{dz_1}\frac{1}{R} - w_2\frac{d}{dx_1}\frac{1}{R}\right)\right\}$$

$$-\tfrac{1}{2}\mu ee'\frac{d}{dx_1}\left(u_2\frac{d}{dx_1} + v_2\frac{d}{dy_1} + w_2\frac{d}{dz_1}\right)^2 R$$

$$+\mu ee' u_2\left(u_2\frac{d}{dx_1} + v_2\frac{d}{dy_1} + w_2\frac{d}{dz_1}\right)\frac{1}{R}.$$

The expressions in the first line represent the usual expressions for the force on the first particle due to its motion in the magnetic field produced by the motion of the second. The terms in the second and third lines reduce to

$$\tfrac{1}{2}\mu ee'\frac{x_1-x_2}{R}\frac{1}{R^2}(u_2{}^2+v_2{}^2+w_2{}^2)(1-3\cos^2\theta),$$

where θ is the angle the direction of motion of the second particle makes with the line joining the two particles. Thus these terms represent a force acting along the line joining the particles and equal to

$$\frac{\mu ee'}{2R^2}(u_2{}^2+v_2{}^2+w_2{}^2)(1-3\cos^2\theta).$$

This radial force may be compounded with the force $\dfrac{ee'}{R^2}$ due to the electrostatic attraction, and we take it into account by supposing that the attraction of the second particle on the first is not

$$\frac{ee'}{R^2}, \text{ but } \frac{ee'}{R^2}\left(1 + \frac{(u_2{}^2+v_2{}^2+w_2{}^2)}{2V^2}(1-3\cos^2\theta)\right),$$

where V is the velocity of light.

The difference between the electric forces due to a charge at rest and in motion is proportional to v^2/V^2, where v is the velocity of the particle and V that of light; and thus for any individual particle moving slowly compared with light is exceedingly small. Yet, on the view that the structure of the atoms is electrical, the number of charged particles is so enormous, and the velocity of the negative particles so much greater than the positive, it might seem that if any order were to exist in the motion of the negative particles as in the case of a magnet, finite differences of electric force in different directions might be produced: thus, for example, the magnet might behave as if it had a charge of negative

electricity at its ends and positive at its sides. A little closer consideration will show, however, that such an effect is not probable, for suppose the negative particles are describing closed circular orbits, then the average effect will be the same as if the charge were uniformly distributed round the orbit; thus the average effect will be that due to a uniformly electrified ring rotating about an axis through its centre at right angles to its plane, and in this case the electric force due to the motion obviously vanishes.

Electromotive Force due to Induction.—Take the case of a particle fixed by the coordinates x_1, y_1, z_1, in the presence of any number of charged particles moving about in any manner. Let us fix the position of these particles by coordinates ξ_r, η_r, ζ_r. Then we see from the preceding example that T, the kinetic energy, can be written as follows :

$$\mathrm{T} = \frac{\mathrm{M}}{2}(u_1^2 + v_1^2 + w_1^2) + (u_1\mathrm{F} + v_1\mathrm{G} + w_1\mathrm{H}) + \mathrm{T}_0,$$

where F, G, H are linear functions of the velocities $\dot{\xi}_r$, $\dot{\eta}_r$, $\dot{\zeta}_r$ and do not involve u_1, v_1, w_1; T_0 is a quadratic function of the velocities $\dot{\xi}_r$, $\dot{\eta}_r$, $\dot{\zeta}_r$. The momentum of the first particle parallel to x is

$$\mathrm{M}u_1 + e\mathrm{F}.$$

We have seen that F is the vector potential due to the remaining particles at the first particle.

By Lagrange's equation we have

$$\frac{d}{dt}(\mathrm{M}u_1 + e\mathrm{F}) - e\frac{d}{dx_1}(\mathrm{F}u_1 + \mathrm{G}v_1 + \mathrm{H}w_1) - \frac{d\mathrm{T}_0}{dx_1} = \text{force tending to increase } x_1.$$

Now

$$\frac{d}{dt}(e\mathrm{F}) = e\left(\frac{\partial\mathrm{F}}{dt} + u_1\frac{d\mathrm{F}}{dx_1} + v_1\frac{d\mathrm{F}}{dy_1} + w_1\frac{d\mathrm{F}}{dz_1}\right)$$

and

$$e\frac{d}{dx_1}(u_1\mathrm{F} + v_1\mathrm{G} + w_1\mathrm{H}) = e\left(u_1\frac{d\mathrm{F}}{dx_1} + v_1\frac{d\mathrm{G}}{dx_1} + w_1\frac{d\mathrm{H}}{dx_1}\right);$$

hence

$$\frac{d}{dt}\mathrm{M}\dot{u} = -e\frac{\partial\mathrm{F}}{dt} + ev_1\left(\frac{d\mathrm{G}}{dx_1} - \frac{d\mathrm{F}}{dy_1}\right) - ew_1\left(\frac{d\mathrm{F}}{dz_1} - \frac{d\mathrm{H}}{dx_1}\right)$$

$$+ \frac{d\mathrm{T}_0}{dx_1} + \text{external force tending to increase } x_1.$$

Thus, in consequence of the variation in the external field

and motion of the point itself, there is a force acting on *unit* electric charge, *i. e.* an electric force equal to

$$- \frac{\partial F}{\partial t} + v_1\left(\frac{d G}{dx_1} - \frac{d F}{dy_1}\right) - w_1\left(\frac{d F}{dz_1} - \frac{d H}{dx_1}\right) + \frac{1}{e}\frac{d T_0}{dx_1};$$

these are the usual expressions for electromotive forces due to the induction of current, the term $\frac{1}{e}\frac{d T_0}{dx_1}$ being included in the term derived from the scalar potential.

On the Momentum in Electrical Waves.
Pressure of Radiation.

Let us consider a plane electrical wave in which, as we shall prove later, the electric force and the magnetic force are at right angles to each other, and both in the wave-front. Let H be the magnetic force at a point in the wave-front, then X, the electric force, is in a progressive wave equal to VH, where V is the velocity of light, X and H being measured in electromagnetic units; the electric polarization parallel to X is therefore $H/4\pi V$, so that the momentum which is at right angles both to F and H is along the normal to the wave-front, *i. e.* in the direction of propagation of the wave, and is equal per unit volume to $\frac{H^2}{4\pi V}$. Now $H^2/4\pi$ is the energy (electrostatic + electromagnetic) per unit volume, so that the momentum per unit volume is equal to (energy per unit volume)/(velocity of propagation of the wave). If this wave were to fall on a body by which it is absorbed without reflexion, this momentum would be communicated to the absorbing body, the amount of momentum communicated to the body per unit time would be per unit area of wave-front equal to V times the momentum per unit volume; it would therefore equal the energy per unit volume in the wave. This communication of momentum to the body being proportional to the area of the surface, might be produced by the application of a suitable pressure to the surface, the pressure being equal to the momentum communicated to the body per unit area per unit time. Thus a body absorbing electrical waves behaves as if a pressure acted on its surface in the direction of propagation of the wave equal to the energy per unit volume in the wave. Light waves are a particular case of electrical waves, hence when light falls on an absorbing substance it exerts a pressure on the surface equal to the energy per unit volume in the light wave. The existence of

this pressure of radiation as it is called, was shown by Maxwell to be a consequence of the Electromagnetic Theory of Light ; it has been detected and measured in some beautiful experiments made by Lebedew [*] and by Nichols and Hull [†].   The magnitude of this pressure in the case of light waves is very small.

If the light, instead of being absorbed, were wholly reflected, then, when light is incident normally on the surface, the incident light communicates to it per unit time an amount of momentum in the direction of the light equal per unit of surface to E, where E is the energy of the incident light per unit volume ; the reflected light carries away per unit time an amount of momentum in the opposite direction equal to E. Hence the effect on the reflector is the same as if it received 2E units of momentum per second in the direction of the incident light; *i. e.,* as if the reflecting surface were acted on by a pressure equal to 2E : hence for normal incidence the pressure on a perfectly reflecting surface is twice that on a perfectly absorbing one.   If the absorbing body were moving towards the wave with the velocity u, the amount of momentum absorbed in unit time, and therefore the pressure of the radiation, would be increased in the proportion of $V + u$ to V. Radiation will give rise to pressure when it is refracted, for the momentum in the medium being in the direction of propagation, when the latter is changed the momentum in the medium is changed also, and therefore by the principle of the Conservation of Momentum the refracting substance must suffer a change in momentum.   Thus, suppose a beam of light is travelling along a curved path in a refracting medium ; then, if ds is an element of the path, $d\theta$ the angle between the direction of the beam at the beginning and end of the element, the direction of momentum is changed by $\delta\theta$ in passing along ds.   Thus, if M is the momentum per unit volume in the beam, an amount of momentum $VM\delta\theta$ at right angles to the path of the light must be communicated to the medium per unit time : thus the force per unit volume at right angles to the light will be $VM\dfrac{\delta\theta}{\delta s}$ or E/ρ, where ρ is the radius of curvature of the beam.

* Rapports présentés au Congrès International de Physique (2) p. 133.
† Proc. American Academy of Arts and Sciences, xxxviii. p. 559.

XXXV. *The Relative Amount of Ionization produced in Gases by Röntgen Rays of Different Types.* *By* R. K. McClung, *M.A., Trinity College, Cambridge;* 1851 *Exhibition Science Scholar* *.

Introduction.

THE question of the relative amount of ionization produced in different gases by the action of Röntgen rays is one which has been investigated by several experimenters. Being a fundamental question in connexion with ionization in gases, it was investigated quite early in the history of Röntgen rays. Perrin† and Rutherford‡ about the same time, but quite independently, tested the ionization in several gases and found the relative values for these gases. Later J. J. Thomson§ made a very comprehensive set of experiments which included a large number of gases. The values obtained by these different observers for the various gases are in most cases in fair agreement with one another, but in a few instances there is considerable disagreement. This disagreement is especially noticeable in the case of hydrogen. It has been suggested by Prof. J. J. Thomson, in speaking of this point in his recent book on the ' Conduction of Electricity through Gases,' that this discrepancy might be explained by the fact that the different experimenters used rays of different degrees of hardness, and that probably the relative amount of ionization in different gases depends upon the type of rays used. The experiments described in this paper were undertaken, therefore, to see if some explanation could be found for this discrepancy in previous results, especially in the case of hydrogen.

For the purpose of these experiments it is very necessary, in order to obtain perfectly definite results, that, in comparing the ionization in two gases, rays of exactly the same sort and the same intensity should be used to ionize each gas. A balance method was therefore adopted by which the two gases under investigation in any given instance were ionized simultaneously by the rays. Any change in the ionizing power of the rays would thus affect both gases at the same time. Another reason for adopting this method

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was that it is a much more sensitive and exact one than that of comparing separate and direct measurements made on the two gases at different times.

Description of Apparatus and Method.

The apparatus which was used is shown diagrammatically in fig. 1. The Röntgen-ray bulb was enclosed as usual in a

Fig. 1.
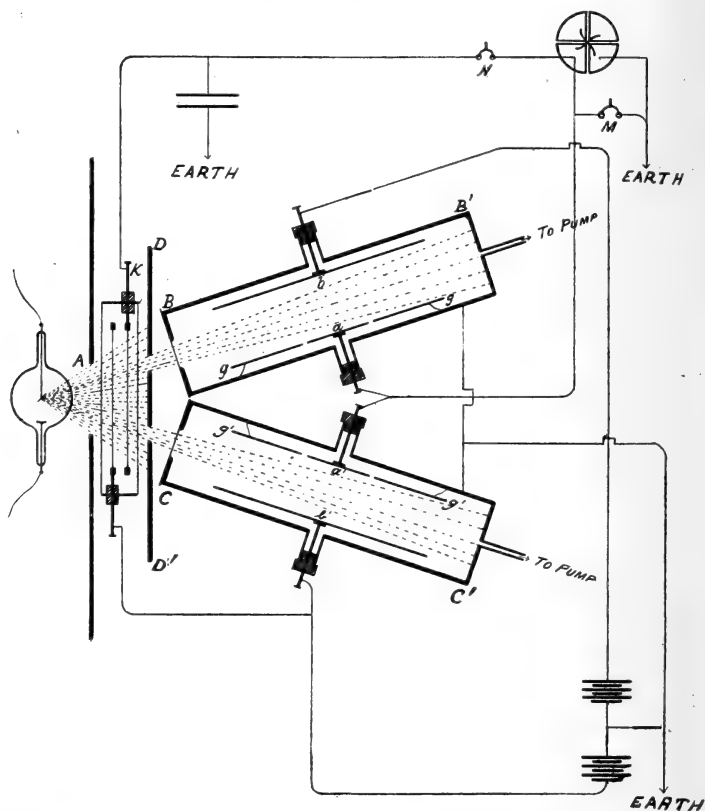
lead-covered box and the rays emerged through a rectangular opening A. BB′ and CC′ were two brass cylinders which contained the gases which were ionized. These cylinders were about 27 cms. in length and 8 cms. in diameter. The ends at B and C were made of thick aluminium plates recessed at the centre, as shown in the diagram, so that the rays might only have to pass through a small thickness of

aluminium. The cylinders were made gas-tight so that they could be exhausted. The electrodes a and a' consisted of thin aluminium plates 7 cms. by 4 cms., and they were surrounded by the guard-rings g and g' made of the same metal and connected to earth. The electrodes b and b' also consisted of aluminium plates about 23 cms. by 6 cms. All these electrodes were supported by stout brass rods which passed out through ebonite plugs. In the actual experiments the ionization measured was that which was produced in the volume of gas contained between the plates a and b and that between a' and b'.

Just close to the ends B and C of the cylinders was placed a thick lead screen DD′ in which two well-defined rectangular holes were cut, so that a definite beam of rays might pass into each cylinder.

As the rays produce secondary radiation at the surface of metals when falling upon or emerging from them, the ends B′ and C′ of the cylinders were covered on the inside with cardboard, while the inside surface of the ends B and C were covered with white paper in order to reduce this secondary radiation as much as possible. Besides being thus very considerably reduced, these secondary rays would have to traverse about 10 cms. of gas before they would be able to ionize any of the gas on which the measurements were being made. Any effect from the secondary rays from the surface of the metals would thus be reduced to a very small amount, if not practically eliminated altogether. The openings in the lead screen DD′ were also made of the proper dimensions, so that the rays should pass between the aluminium electrodes without impinging upon them.

Now if the gas in each of the cylinders is ionized and the electrodes b and b' are raised to a high potential while the electrodes a and a' are insulated, then a and a' will become charged. If the potential of b is opposite in sign to that of b', then a and a' will receive charges of opposite sign. If a and a' are connected together and the charges received by them are equal but of opposite sign, they will of course nullify each other, but if not equal there will be a residual effect. In the experiments, the one electrode b was connected to one pole of a battery of accumulators, and the other one b' was connected to the other pole, while the middle of the battery was connected to earth, as shown in the diagram. The two electrodes a and a' were connected together and to one pair of quadrants of an electrometer, the other pair of quadrants being to earth.

The method of comparing the amount of ionization in two

gases was therefore as follows:—Both of the cylinders were filled with air at atmospheric pressure after it had been carefully dried, by being passed through a long glass tube containing pumice-stone soaked in strong sulphuric acid. The cylinders were then carefully adjusted in position until the ionization in them was such as to produce equal charges upon a and a', and no deflexion of the electrometer-needle, due to these charges, took place when the quadrants were insulated.   When this balance was obtained, one of the cylinders was exhausted and filled with the gas which was to be compared with air.   It was exhausted and refilled several times so as to get rid of all the air in the cylinder.   Now, since the ionization in this gas would in general be different from that in air, the charges received by a and a' would not now be equal, and the balance would be destroyed.   Instead of measuring the alteration of the balance by the deflexion of the electrometer-needle, the pressure either of the air in the one cylinder, or the gas in the other one, was altered until the amount of ionization in each of the two cylinders was the same and the balance was restored once more.

It has been shown by Perrin[*] and also by Rutherford and McClung[†] that the total amount of ionization in a gas is proportional to the pressure of the gas.   Suppose that the ionization in a gas X at a pressure P is being compared with the ionization in a gas A at the same pressure.   The two cylinders are filled with the gas A at the pressure P, and the ionization in one balances that in the other.   One vessel is then filled with the gas X at the pressure P, and suppose first that the ionization in it is less than in A.   Then the vessel containing the gas A will have to be partially exhausted in order to restore the balance.   Suppose that when the balance is restored the pressure of the gas A is P'.   Let I_A be the ionization in A at the pressure P, and let I_X be the ionization in the gas X at the same pressure P.   Then if the ionization in the one cylinder filled with the gas A at the reduced pressure P' balances that in the other cylinder filled with the gas X at a pressure P, we have the ratio $\dfrac{I_X}{I_A} = \dfrac{P'}{P}$.   Similarly, if the ionization in X is greater than that in A, then it would be necessary to partially exhaust the vessel containing X in order to obtain a balance.   If P'' were the pressure of the gas X when a balance was obtained,

* Perrin, *Annales de Physique et de Chimie,* xi. p. 496 (1897).
† Rutherford and McClung, Phil. Trans. cxcvi. (1901).

then $\dfrac{I_X}{I_A} = \dfrac{P}{P''}$. In the actual experiments the gases were compared, in every instance, with air. The ionization of the given gas at atmospheric pressure was compared in each case with air at the same pressure.

This was found to be a very delicate method of comparison. The electrometer used was one of the Dolezalek pattern, which gave a deflexion of about 2000 scale-divisions for a difference of potential of 1 volt between the quadrants when the needle was charged up to a potential of 120 volts. It was possible by this method and apparatus to detect an extremely slight change in the ionization in either of the cylinders. When a balance was once obtained, an extremely small alteration in either of the cylinders would produce a deflexion of the electrometer-needle in either one direction or the other. When any gas was being compared with air, the pressure in the partially exhausted cylinder had to be adjusted very accurately, as a very slight alteration in the pressure would destroy the balance. This delicacy of the method made it possible to detect very slight changes in the ionization which, with a less delicate apparatus, might very easily escape notice.

Comparison of the Ionization in Hydrogen and Air.

In the results obtained by previous experimenters hydrogen was the gas which showed the greatest discrepancy. In the present series of experiments hydrogen was, therefore, the first gas experimented upon. It was in every case of course compared with air. Several comparisons were made, using as the source of the Röntgen rays a bulb with an automatic vacuum-regulator attached. By means of this regulator the pressure inside the bulb was kept very constant, and the quality of the rays given out therefore remained very uniform from time to time. This was one of the regular type of automatically regulating bulbs made by the firm of H. W. Cox; and since it proved so satisfactory as a source of rays it may perhaps not be out of place to give a brief description of it here. A diagram of the bulb and the attached regulator is shown in fig. 2. The bulb itself and its electrodes are of much the same form as in the ordinary simple form of focus-tube, but attached to the bulb is a side tube R communicating with the vacuum-regulator S. This is just a small discharge-tube, and the anode is connected to the anode of the main bulb. The cathode is connected to a wire which terminates at a spark-gap P. The terminal on the other

side of this air space is connected to the cathode of the main
bulb.   When the pressure in the bulb becomes too low, due
to the discharge passing for a time, then a spark will pass

Fig. 2.
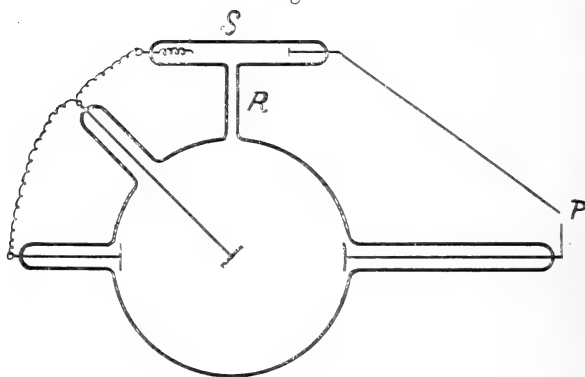
across the air-gap P and a discharge pass through the side
tube S.   The frequency of the passage of this discharge can
be regulated by adjusting the length of the · spark-gap P.
When the discharge passes through S a little gas or vapour
is liberated from the electrode, and the pressure is thus very
slightly increased.   This arrangement works quite auto-
matically and maintains a very constant pressure in the bulb,
and causes it to work very steadily compared with one which
has no such regulator.
    In order to test the constancy of the rays during a series
of observations a standard apparatus was placed between the
source of the rays and the cylinders B and C, as shown in
the diagram.   This standard apparatus was similar in con-
struction to the one used by the author in the experiments
on the effect of temperature on ionization, and is described
on page 91 of the Philosophical Magazine for February 1904.
The constancy of the ionization was tested in the following
manner:—When the cylinders had been carefully adjusted
so that a good balance was obtained, the rays were started
and allowed to run for about fifteen or twenty seconds, so
that the ionization might become steady.   During this time
the electrodes a, a', and K, as well as the quadrants of the
electrometer, were all connected to earth.   At the end of
this time, all these electrodes were insulated simultaneously

by opening the keys **M** and **N**, which worked by means of strings from a distance, and the electrode **K** was allowed to charge up for a given length of time. There was also a capacity in parallel with **K**. Since **K** was quite insulated from the electrometer it was, therefore, first observed whether the balance between a and a' remained steady, and then at the end of a given time the rays were shut off and the key **N** was closed and the charge in the electrode **K** was measured. The balance and the intensity of the ionization were thus tested simultaneously.

The two cylinders BB' and CC' were filled with air at atmospheric pressure and a balance established, and the strength of the ionization tested in the standard apparatus. One of the cylinders was then exhausted and filled with hydrogen at atmospheric pressure. The other cylinder containing the air was then partially exhausted until the ionization in it just balanced the ionization in the hydrogen. This balance and the intensity of the ionization were again tested.

Several experiments were made using the automatic regulating bulb, and extremely constant results were obtained. Although these results, giving the ratio of the ionization in hydrogen to that in air, agreed so extremely well among themselves, yet this ratio differed very considerably from any of the numbers obtained by the previous observers. This fact, therefore, suggested the possibility that if a different X-ray bulb were used the ratio might be altered. Several other bulbs were consequently tried and quite different results obtained with them. None of these bulbs were automatic regulating ones, but were of the ordinary form of focus-tubes. There was no means of keeping the pressure in them quite constant, and therefore the rays given out were liable to vary. Not only did the different bulbs give different results, but in some instances the results obtained at different times from the same bulb varied among themselves. The results obtained from the various bulbs on different occasions are given in Table I. All the determinations and the dates on which the experiments were made are inserted to show how constant the results remained in the case of the bulb which was regulated automatically, although the experiments extended over a considerable time, while in the case of bulb No. 2, which was not so regulated, the results varied from time to time. Bulb No. 1 was the automatic regulating one, while the others were all nonautomatic.

TABLE I.

Relative Ionization in Air and Hydrogen, using different bulbs.

Bulb.	Date of Determination.	Pressure.		$\dfrac{P'}{P}$	
		P.	P'.		
No. 1.	Nov. 23	766·2	89·1	·116	Automatic bulb.
,,	,, 25	765·4	89·0	·116	,, ,,
,,	,, 27	744·7	84·0	·112	,, ,,
,,	Dec. 4	747·1	90·7	·121	,, ,,
,,	,, 5	741·2	90·0	·121	,, ,,
,,	,, 9	741·2	89·5	·120	,, ,,
,,	Jan. 20	777·2	90·3	·116	,, ,,
,,	Mar. 15	762·9	80·8	·105	,, ,,
No. 2.	Dec. 1	752·9	63·0	·084	Non-automatic bulb.
,,	,, 2	764·8	61·0	·079	,, ,,
,,	Jan. 16	761·2	51·8	·068	,, ,,
,,	,, 18	769·1	48·0	·062	,, ,,
,,	,, 19	775·2	47·0	·060	,, ,,
No. 3.	Dec. 11	748·7	42·5	·056	,, ,,
,,	,, 14	756·1	39·6	·052	,, ,,
No. 4.	Jan. 14	738·1	43·8	·059	,, ,,

Different bulbs of course give out different types of rays, and even the rays given out by the same bulb under different conditions vary from time to time. It has long been known, of course, that rays of different degrees of hardness produce different amounts of ionization in any given gas. It appears, however, from these experiments that when the rays vary the ionization does not change in the same ratio for these two gases. The change in type of the rays seems to affect one gas to a greater extent than the other. The relative amount of ionization produced in these two gases is a function of the type of rays used to produce the ionization. This explains, therefore, how it is that different experimenters may get quite different values for the ratio of the amount of ionization produced in the two gases.

The constancy of the results obtained with the bulb in which the pressure of the gas remained fairly constant, together with the inconstancy of the results from the bulb in which the pressure did not remain steady, indicated that the difference in type of the rays due to the variability of the pressure in the bulb was one at least of the prime factors in causing the discrepancy in the ratios obtained for the relative ionization in air and hydrogen. To investigate this matter the Röntgen-ray bulbs which had been previously used were replaced by one which was attached to a Toepler pump, so

that the pressure in it might be varied while the other con-
ditions remained constant. The bulb was exhausted until
the pressure was just low enough for Röntgen rays to be
produced which were strong enough to produce an appre-
ciable effect in the leak apparatus. The air was then exhausted
from the one cylinder until the ionization in it balanced that
in the hydrogen in the other cylinder. The pressure in the
bulb was then slightly lowered by means of the pump, and
thereby harder rays were produced, and on doing this it was
found that the balance was destroyed. The deflexion of the
electrometer-needle was in the direction which indicated that
the harder rays produced more ionization in hydrogen com-
pared with air than did the softer rays. More air had to be
let into the cylinder containing air in order to balance the
ionization in the hydrogen. When the balance was again
restored by thus letting in more air, the pressure in the bulb
was again lowered, still further changing the type of rays
given out. This again caused the balance to be destroyed
and the deflexion of the electrometer-needle to be in the same
direction as before. The balance was restored again as before
and the same process repeated. The pressure in the bulb
was continually lowered and observations taken at intervals
in this manner, until the pressure was so low that the coil
would no longer produce a steady discharge in the bulb.
These experiments were repeated several times and the same
result was always obtained, namely, that the harder the rays
the greater was the ionization which they produced in
hydrogen as compared with that which they produced in
air.

Experiments were made to see if there could be found any
relation between the hardness of the rays and the ratio of the
ionization produced by them in air and in hydrogen. To
test this, the penetrability of the rays at the different pres-
sures of the air in the bulb was tested by measuring the
absorption of the rays by a known thickness of aluminium,
It was, however, found to be very difficult to obtain definite
results by this method, for the pressure in the bulb varied so
rapidly, especially at the higher pressures, owing simply to
the running of the bulb, that when a balance had been
obtained for the ionization produced by rays of a given
penetrability, the rays changed before sufficient readings could
be taken to measure this penetrability to any degree of
accuracy. It was necessary therefore, to get consistent
results, that simultaneous measurements should be made
by some means. The following method was therefore
adopted :—In parallel with the Röntgen-ray bulb was placed

an alternate spark-gap. When a balance was being obtained, the length of this spark-gap was adjusted at the same time so that the discharge would pass across this air-space just about as easily as through the bulb. This sparking-distance was the distance between two brass spheres. The length of this alternate spark-gap depends upon the pressure in the bulb, and the hardness of the rays also depends upon this pressure. Although the relation between this alternate sparking-distance and the hardness of the rays is not a very definite one, yet the greater this sparking-distance the harder are the rays, and the length of this spark-gap therefore gives some indication of the hardness of the rays. It is not an absolute measure, but serves as an indication of the relative hardness of the rays. The experiments made by this method showed that the relative ionization in air and hydrogen depended in a very definite manner upon the hardness of the rays. A number of experiments were made on this question, and a series of readings is shown in Table II. just as an example.

TABLE II.

Length of Alternate Spark-gap in mms.	Pressure in vessel containing Air.
3·99	41·5 mm.
8·02	50·5 ,,
10·77	55·4 ,,
14·06	63·4 ,,
17·24	66·4 ,,

The first column shows the alternate sparking-distance, while the second one shows the pressure of the air in the one cylinder at which the ionization in it just balanced the ionization in the hydrogen in the other one at atmospheric pressure. As the rays became harder more air had to be let into the cylinder containing air in order to maintain a balance. These results given in this table are only relative ones, for in doing these experiments a balance was not obtained in each case with both cylinders filled with air in the first place before introducing the hydrogen into one of them. The numbers representing the pressure of the air given in the table do not therefore indicate the absolute values of the pressure, but only relative values to show how the pressure varied as the rays varied. These results show conclusively that the variation of the pressure in the bulb producing the

rays has a very marked effect on the relative ionization in these gases.

Later on some absolute determinations were made for these two gases, using much harder rays than those employed in the earlier experiments. Bulb No. 3 was used, and before making the determinations it was run for a long time, using a mercury interrupter so that it might give out very hard rays. When making the experiments, a sheet of lead about 0·3 mm. in thickness was placed between the bulb and the cylinders to cut off all the soft rays that might be given out and only allow the hard rays to enter the cylinders and produce the ionization. When these hard rays were used, the relative value for the ionization in air and hydrogen was quite different from that obtained for the softer rays, as was to be expected from the previous results. The value of the ratio $\frac{P'}{P}$ was considerably increased. The mean value of three experiments which were in fair agreement was 0·177, which is much higher than any of the values obtained for the softer rays. This is quite in agreement with the results obtained from the experiments in which the hardness of the rays was varied by directly altering the pressure in the bulb.

Comparison of the Ionization in Oxygen and Air.

Other gases besides hydrogen were also investigated to ascertain whether this peculiar behaviour observed in hydrogen and air was common to these other gases as well. Among other gases oxygen was examined. The experiments were performed in an exactly similar manner to those with hydrogen, the ionization in oxygen being in every instance compared with that in air. The ionization in oxygen of course is higher than that in air, and in this case the pressure of the oxygen had to be reduced in order to obtain a balance. When the comparatively soft rays given out by bulb No. 1 were used, fairly steady results were obtained as in the case of hydrogen. The mean value for the ratio $\frac{P}{P''}$ for these rays was 1·30. When, however, the hard rays given out by bulb No. 3 were used, the value of this ratio was reduced to 1·17. This lower value is about the same as those obtained by previous observers, but the value for the softer rays is somewhat larger.

Experiments were also made with oxygen similar to those made with hydrogen to test the effect of varying the pressure in the bulb. It was found that an alteration in the hardness

of the rays produced a variation in the relative amount of
ionization in air and oxygen, as in the case of hydrogen and
air.   In this case, however, the ionization in oxygen decreased
relatively to that in air.   As the pressure in the bulb was
lowered, and the rays consequently became harder, it was
necessary to let more oxygen into the cylinder containing
oxygen in order to restore the balance.   This variation of
the relative amount of ionization with the change in the
type of rays was quite marked in the case of these two gases.
A set of observations illustrating this is shown in Table III.
As before the first column gives the length of the alternate
spark-gap, while the second column gives the pressure in the
oxygen cylinder necessary to balance the ionization of the
air at atmospheric pressure in the other cylinder.

TABLE III.

Length of Alternate Spark-gap in mms.	Pressure in the cylinder containing Oxygen.
5·03	623·3 mm.
7·01	636·2 ,,
10·1	652·3 ,,
16·49	656·0 ,,
20·48	663·0 ,,

As in the case of hydrogen and air, the tendency seems to be
for the amount of ionization in oxygen and in air to become
more nearly equal to one another as the rays producing the
ionization become harder.

Comparison of the Ionization in Carbon Dioxide and Air.

Experiments were also made to compare carbon dioxide
and air in a manner similar to the other gases.   The ioniza-
tion in carbon dioxide being greater than that in air, the
pressure of the carbon dioxide was lowered so as to balance
the ionization in it with that in air at atmospheric pressure.
For the softer rays the mean value of the ratio $\dfrac{P}{P''}$ was found
to be 1·46, while for the harder rays the value was reduced
to 1·33.   The change took place in the same direction as in
the case of oxygen and air.

In the experiments in which the pressure in the Röntgen-
ray bulb was altered similarly to the experiments with the
previous gases, an effect was shown similar to that in the case

of oxygen, and the change took place in the same direction
but was not quite so marked as in oxygen. The change in
the type of rays did not have quite so great an effect in
changing the relative amount of ionization in carbon dioxide
and air as it did in the case of oxygen and air. The effect
was, however, quite distinct, as is shown by the results given
in Table IV.

<div align="center">TABLE IV.</div>

Length of Alternate Spark-gap in mms.	Pressure in cylinder containing Carbon Dioxide.
6·51	564·1 mm.
9·79	574·2 ,,
13·97	578·8 ,,
19·74	580·6 ,,

Comparison of the Ionization in Sulphur Dioxide and Air.

Sulphur dioxide was experimented upon and treated in a
manner exactly similar to that which has been described for
the other gases. Results were obtained very similar to those
obtained for oxygen and carbon dioxide, but the effect pro-
duced by altering the type of rays was more marked in the
case of sulphur dioxide than with oxygen or carbon dioxide.
The change of ionization with change of rays took place in
the same direction as it did in the case of the other two
gases. The value of $\frac{P}{P''}$ when using the softer rays was
11·05, but when the harder rays were used this value was
reduced to 4·79. The same result is shown by the experi-
ments in which the type of rays was varied by altering the
pressure in the bulb. The effect was quite marked, as is
shown by the series of observations given in Table V.

<div align="center">TABLE V.</div>

Length of Alternate Spark-gap in mms.	Pressure in cylinder containing Sulphur Dioxide.
6·09	69·3 mm.
11·9	76·3 ,,
16·75	81·0 ,,
19·09	82·7 ,,

General Comparison of the Ionization in Different Gases.

A series of determinations of the relative amount of ionization in the several gases already mentioned was made both for the case of comparatively soft rays and for considerably harder ones, each gas being as usual compared with air. The softer rays were produced by the automatic regulating bulb No. 1, and were comparatively easily absorbed. A sheet of lead of about 0·3 mm. in thickness placed in the path of the rays was capable of stopping practically the whole effect of the rays given out by this bulb. The harder rays were produced by bulb No. 3, which, previous to making this series of experiments, was run by means of a large coil and mercury break for a considerable time, so that it produced comparatively hard rays. The rays given out by it were much harder than those which it produced when the determinations given in Table I. were made with this bulb. In making these comparisons with the hard rays, a sheet of lead of about 0·3 mm. in thickness was placed in the path of the rays so that all the softer rays were cut off, and only the harder ones which were able to penetrate the lead produced the ionization in the cylinders. Even with this thickness of absorbing lead, very considerable effects were produced in the ionizing vessels. The two series of determinations are given in Tables VI. and VII.

TABLE VI.—Relative Ionization for Soft Rays.

Name of Gas.	Ionization relative to Air taken as unity.	Relative Density of Gases, Air taken as unity.
Air	1·0	1·0
Hydrogen	0·105	0·069
Oxygen	1·30	1·105
Carbon Dioxide......	1·46	1·529
Sulphur Dioxide ...	11·05	2·193

TABLE VII.—Relative Ionization for Hard Rays.

Name of Gas.	Ionization relative to Air taken as unity.
Air	1·0
Hydrogen	0·177
Oxygen	1·17
Carbon Dioxide...........	1·33
Sulphur Dioxide	4·79

In the former series of experiments the automatic regulating bulb was used, and as previously mentioned this bulb was very constant in the type of rays which it gave out ; and the numbers given in Table VI. can be very fairly compared with one another for that particular type of rays. In the latter case, in which much harder rays were used, such strict constancy was not attainable, as is of course almost invariably the case when working with very hard rays. It is not possible to maintain a very "hard" bulb so constant in its radiation as a "soft" one. The numbers in Table VII. cannot therefore be quite so strictly compared with one another as those in Table VI., but nevertheless they show the relative ionization in these gases for hard rays of approximately the same type. The chief point which they illustrate, however, is the considerable difference in the relative values for the ionization in these gases for the two types, hard and soft rays.

General Consideration of Results.

The series of experiments which have been described in this paper show conclusively that the relative ionization produced by Röntgen rays in different gases is not constant but depends very much upon the type of rays which are used as the ionizing agent. The change from hard to soft rays appears to affect different gases to quite a different extent. As far as these experiments go, the chief factor in producing this effect appears to be the state of the vacuum in the Röntgen-ray bulb which produced the rays. The pressure of the gas in the bulb may not be the sole cause of this difference, but it certainly is a very important factor in producing this effect, as is shown very conclusively by the results given in Tables II. to V. Other conditions of the Röntgen-ray bulb, such as the sort of electrodes used in it, &c., may also aid in producing a difference in the type of rays which affect different gases to a different extent. To such causes may be partly due also the differences in the type of rays which produce the differences in the relative amount of ionization in air and hydrogen obtained by using different bulbs, as shown in Table I. Whatever may be the causes which produce rays of different types, it is quite evident from these experiments that the relative amount of ionization produced in any two gases by Röntgen rays is not a fixed ratio, but that the ratio is a function of the type of rays which produce the ionization.

This variation in the relative ionization does not appear to depend in any definite manner upon the intensity of the radiation so long as the nature of the rays remains the same.

In some of the experiments, especially with the harder rays, the intensity of the radiation, as measured by the standard apparatus, varied considerably ; but this variation of the intensity did not seem to affect the balance between the ionization in the two gases in the cylinders in any definite manner like the alteration of the pressure in the bulb did. The general results obtained from the experiments tended to show that this change of relative ionization is not a question of the intensity of the rays but of the type of rays.

In considering the manner in which this variation of the relative ionization in the different gases with change of rays takes place, we observe that in all cases examined it always takes place in a definite direction.   In the case of hydrogen and air, where the ionization in hydrogen is less than in air, it increases relatively to that in air, or the ionization in air decreases relatively to that in hydrogen, as the rays become harder.   In the case of the other gases where the ionization is greater than in air, it decreases relatively to that in air as the rays become harder.   The tendency appears to be, therefore, for the value of the ionization in the denser gas to approach the value of the ionization in the less dense one as the rays become more penetrating.   The total relative ionization does not appear to be a function of the densities of the gases.   In comparing oxygen and sulphur dioxide with air, the ratios for the relative ionization approached more nearly to the ratios for the densities the more penetrating the rays became ; but in the case of hydrogen and carbon dioxide, the more penetrating the rays became the farther from the ratios for the densities became the ratios for the relative ionization. This is easily seen by referring to Tables VI. and VII.   The relative densities of the gases are given in Table VI. for convenient comparison.   Mr. Eve [*] has shown a short time ago in an article in ' Nature,' that for very penetrating rays the relative ionization in several gases was approximately in the same ratio as the densities of the gases.   It is very probable, from the experiments described in this paper, that for extremely penetrating rays the ratios for the ionization in oxygen and sulphur dioxide compared with air would become approximately equal to the ratios of the densities of the gases. Hydrogen and carbon dioxide show, however, quite the opposite tendency.   The general tendency shown by all the gases examined was for the amount of ionization in the denser gas to approach nearer to the amount of ionization in the less dense one as the rays become harder.

[*] Nature, March 10th, 1904 (p. 436).

Summary of Results.

The results obtained from this series of experiments may be summed up as follows:—

(1) Different types of Röntgen rays produce different relative amounts of ionization in various gases.

(2) One, at least, of the primary factors in causing this difference in the type of rays, which thus affect different gases differently, is the condition of the vacuum in the Röntgen-ray bulb which gives out the rays.

(3) The ratio of the total ionization in oxygen and sulphur dioxide to that in air tended to become more nearly equal to the ratio of the densities of the gases as the rays became harder ; while in the comparison of hydrogen and carbon dioxide with air, the variation of the ratio for the relative ionization was in the opposite direction. In all cases, the general tendency appeared to be for the ionization in the denser gas to become more nearly equal to that in the less dense gas as the rays became more penetrating.

(4) This variation of the relative ionization in the different gases does not appear to have any connexion with the intensity of the radiation so long as the nature of the rays remains the same, but appears to be a direct result of the change in type of the rays.

In conclusion I would express my thanks to Prof. Thomson, at whose suggestion this research was undertaken, for the interest shown and suggestions given during the course of these experiments.

Cavendish Laboratory, Cambridge,
June 15, 1904.

XXXVI. *The Decay of the Excited Radioactivity from Thorium, Radium, and Actinium.* *By* Miss H. BROOKS, *M.A.*[*]

WHEN a negatively charged metal plate has been exposed in the presence of the emanation from thorium for two or three days, the radiation from the activity excited on the plate steadily decays after removal according to an exponential law, and falls to half its original value in about eleven hours. If, however, a plate is exposed to the emanation for only a short interval of time, it was found by Prof. Rutherford[†] that the radiation from a body thus excited increases at first after removal, reaches a maximum in the course of a few hours, and then decays at the same rate as the radiation from a plate exposed for a long interval.

[*] Communicated by Prof. Rutherford, F.R.S.
[†] Phil. Mag. Jan. 1903.

The excited activity on a negatively charged conductor is due to the deposit on it of positively charged carriers[*], one of the decomposition products of the emanation. The initial increase after a short exposure, it has been pointed out[†], can be explained if it be assumed that a double change takes place after the positive carriers have been deposited on the electrode. If the first of these changes be unaccompanied by the production of rays, while the second gives rise to rays which ionize the gas, then for a very short exposure the radiation from an active body will continue to increase as the first change proceeds, reach a maximum when most of the matter deposited has undergone the first change, and then gradually decrease as the second change takes place. That the initial rise in the radiation is to be observed only for short exposures is to be expected since, after a long exposure, the decay of the radiation as a whole more than compensates for the increase due to the primary change in the radioactive particles deposited in the last few hours of exposure. This increase after removal lasts only a few hours for the shortest exposures, thus the primary change must be completely effected in that time.

These two changes each follow an exponential law with the time, and may be represented by the equation $N_t = N_0 e^{-\lambda t}$, where N_t represents the number of particles present unchanged after an interval t and N_0 the original number.

Let λ_1 = coefficient of the primary change,
and $\lambda_2 =$ „   „   secondary   „
then, if the plate is exposed to the emanation for one second and n_0 particles are deposited, the number of particles dq which have undergone the first change but not the second at a time t after removal is given by

$$dq = \frac{n_0 \lambda_1}{\lambda_1 - \lambda_2} \{ e^{-\lambda_2 t} - e^{-\lambda_1 t} \}$$

$$= n_0 f(t).$$

Now if the rod is exposed for a time t, the corresponding number of particles q_t which have undergone the first change at the time of removal is given by

$$q_t = n_0 f(t) dt + n_0 \{ f'(dt) \} dt + \ \cdot \ \cdot \ \cdot \ \cdot \ \cdot \ n_0 f(t) dt$$

$$= \int_0^t f'(t) dt.$$

* *Phys. Zeit.* no. 10, 1902.
† E. Rutherford, ‘Radioactivity,’ p. 27.

At a time t_1 after removal the corresponding number is

$$n_0 f(t)dt + n_0\{f(t+dt)\}dt + \ \ldots \ldots \ + n_0 f(t+t_1)dt$$

$$= \int_t^{t+t_1} n_0 f(t)dt.$$

Since the radiation arises only from the second change, and is proportional to the value of q, the ratio of the current r at a time t_1 after removal to the initial current is given by

$$r = \frac{\displaystyle\int_t^{t+t_1} n_0 f(t)dt}{\displaystyle\int_0^t n_0 f(t)dt}$$

$$= \frac{\dfrac{e^{-\lambda_2 t_1} - e^{-\lambda_2(t+t_1)}}{\lambda_2} - \dfrac{e^{-\lambda_1 t_1} - e^{-\lambda_1(t+t_1)}}{\lambda_1}}{\dfrac{1 - e^{-\lambda_2 t}}{\lambda_2} - \dfrac{1 - e^{-\lambda_1 t}}{\lambda_1}}$$

$$= \frac{e^{-\lambda_2 t}\left[\dfrac{1 - e^{-\lambda_2 t}}{\lambda_2} - e^{-\overline{\lambda_1 - \lambda_2} t_1}\left(\dfrac{1 - e^{-\lambda_1 t}}{\lambda_1}\right)\right]}{\dfrac{1 - e^{-\lambda_2 t}}{\lambda_2} - \dfrac{1 - e^{-\lambda_1 t}}{\lambda_1}}$$

Let
$$\frac{1 - e^{-\lambda_2 t}}{\lambda_2} = A, \quad \ldots \quad \ldots \quad (1)$$

and
$$\frac{1 - e^{-\lambda_1 t}}{\lambda_1} = B. \quad \ldots \quad \ldots \quad (2)$$

Then
$$r = e^{-\lambda_2 t_1}\left\{\frac{A - e^{-\overline{\lambda_1 - \lambda_2} t_1}B}{A - B}\right\}. \quad \ldots \quad (3)$$

When a plate has been exposed for an interval of only a few minutes, it may be assumed that only a small proportion of the radioactive matter deposited has undergone the first change at the time of removal; thus the curve of decay for such an exposure will be approximately that of particles all deposited simultaneously. From such a curve the values of λ_1 and λ_2 can be obtained. The curve of decay for an exposure of ten minutes shows that half the matter undergoes the first change in about fifty-five minutes. Substituting this value in the equation $\dfrac{I_t}{I_0} = e^{-\lambda_1 t}$ the value $\lambda_1 = \cdot00021$ is obtained, t being expressed in seconds. The rate of the second change is a decay to half value in about eleven hours, which gives $\lambda_2 = \cdot0000175$. By substituting these values in (1) and (2), A and B and A$-$B can be determined for different

times of exposure, and from (3) the current at different in-
tervals after removal may be obtained.

In fig. 1 are given the curves thus obtained for the initial
increase in the radiation to a maximum for exposures of 1, 2,
3, 4, and 6 hours; and the points are also given which were
obtained by observations on a negatively charged body ex-
posed to the thorium emanation for a corresponding length of
time. The agreement between the curves obtained by obser-
vation and those by calculation from equation (3) for exposures
of one hour and upwards is very close. For exposures of
less than one hour the initial increase indicated by the theory
is not quite realized in practice, as it is almost impossible to
prevent the deposit on the electrode of small amounts of
radioactive matter which have already undergone the first
change. The effect of this, in the case of long exposures,
is negligible, but when, as in the case of exposures of less
than one hour, the whole of the resulting activity is very
small, it becomes an appreciable factor and causes the increase
after removal to be less than it would otherwise be.

The arrangements for the experimental observations were
as follows :—A quantity of thorium hydroxide was placed in
the bottom of a cylindrical brass vessel with closely fitting
ends of ebonite, and through a small hole in one of these a
brass rod was slipped into the vessel. The outside of the
cylinder was kept connected to the positive pole of a storage-
battery of 100 volts, and the central rod to the negative pole.
The electric field was kept on the cylinder during the whole
course of the experiments, to prevent the collection in the
vessel of radioactive dust particles which greatly complicate
the results, as will be explained later. When a series of
observations was to be taken, a clean brass rod was intro-
duced in the place of the central electrode, as rapidly as
possible, so that the current might not be off for an appre-
ciable length of time. After an exposure for the required
interval the rod was removed, placed inside a vessel containing
a sensitive gold-leaf electroscope, and the radiation from it
measured at intervals by the rate of collapse of the gold leaves.

Effect of Dust.

It has already been mentioned that the curves found by
experiment for short exposures probably do not really
represent the decay of the activity excited only in the
time indicated by the time of exposure. In the course of
the experiments, the great importance was recognized of the
necessity of avoiding the deposition on the electrode of matter
from the gas which had already been made radioactive and

Fig. 1.—Variation with time of the excited activity of thorium for different times of exposure.

+ Values calculated from equation (3).

⊙ Experimental values.
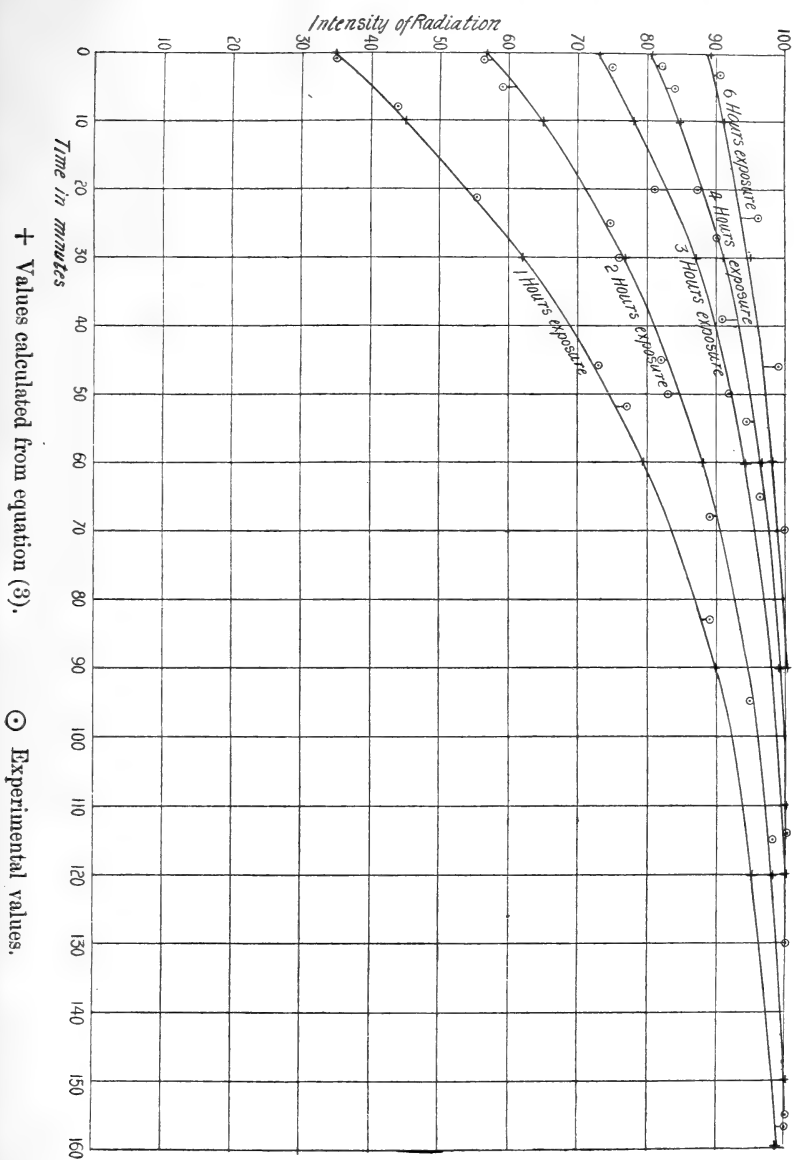
had begun to undergo its changes before the electrode was introduced into the vessel.

Such an action from the dust in the air of the laboratory was quite sufficient to mask entirely, even for exposures of only a few minutes, the initial increase in the radiation after removal. If air, which had not been previously rendered free from dust, was left undisturbed in the presence of the thorium emanation for a period of about eighteen hours, and a negatively charged rod then introduced into the vessel for one minute, the decay of the radiation from the rod after removal was the same as that of an electrode which had been exposed for a period of eighteen hours. The amount of excited activity thus obtained is many times that obtained for the same time of exposure in dust-free air, or air which has been in the presence of the thorium emanation for only a short time. As the emanation from thorium decays very rapidly, falling to half its value in about a minute, the strength of the emanation in a closed vessel should reach a maximum in a few minutes, and then remain practically constant. Such an increase in the amount of excited activity obtainable from it, then, for a given time of exposure must be ascribed to the presence in the gas of matter which becomes active after exposure to the emanation for a length of time, and is driven to the electrodes when an electric field is applied. If the air is freed from dust by passing it through a tightly packed plug of glass-wool before it enters the emanation vessel, or if an electric field is kept on, as in the case of the experiments mentioned earlier in the paper, this increase in the strength of the emanation is not observed. The rate of decay of a body excited in the presence of an " old " emanation also receives a sufficient explanation, if it be assumed that the dust particles suspended in the air of the vessel have become radioactive, and are driven to the electrode in the electric field. The resulting activity then manifested on the electrode is made up of the rays from these radioactive dust particles and rays from the newly excited radioactivity on the rod itself; the latter is very small in comparison with the former, in the case cited only about 1/20th, so that the rate of decay observed is that of radioactive matter which has had a long exposure. A number of observations were made in which the air was left exposed to the thorium emanation for intervals of one half-hour and upwards, and a charged rod then inserted for one minute. The rate of decay after removal was always very nearly the same as that calculated for an electrode exposed for the length of time that the gas has been left in contact with the thorium. In the following table are given a few examples:—

Length of exposure of the gas to the emanation.	Observed ratio of maximum to original radiation.	Calculated ratio of maximum to original radiation.
One hour	3·9	3·3
Two hours............	1·9	1·82
Three ,, ...	1·38	1·35
Six ,, ...	1·3	1·13

By means of this property which dust possesses of becoming radioactive, some information can be gained concerning the change that takes place after the positively charged carrier has become attached to a solid body. If the air in the emanation vessel is dust-free or has just been introduced, when an electric field is applied, no radioactivity is manifested on the positive electrode, it is all confined to the negative. When, however, air which is not dust-free has been closed up in the presence of the emanation for a time and an electric field then applied, a certain percentage of the whole activity obtainable from the gas appears on the positive electrode. When the gas has been left undisturbed for a length of time, to give to every dust particle its maximum amount of activity, almost equal amounts of the active particles are carried to the positive and negative electrodes. This appearance of radio-activity on the positive electrode when exposed to " old " emanation has been noticed before by Sella*, who ascribed it to a change in the nature of the emanation.

Excited Activity from Radium.

The same effect is to be observed in the case of the radium emanation; but owing to the more rapid decay of the excited activity from radium, the error due to the presence of dust is not so great.

In a corresponding series of experiments on the decay of the excited activity from the radium emanation for different times of exposure, the rate of decay of the α, β, and γ rays was measured separately. It has already been shown† that the decay of the α rays from a body made active by a short exposure to the emanation from radium, gives evidence that three changes occur after the positive carriers are deposited on the solid body. The first change gives rise to α rays only and is very rapid, half the matter being changed in about three minutes. This change causes a rapid initial decrease in the intensity of the radiation for the first ten minutes. The

* *Il Nuovo Cimento,* [5] iv. p. 131 (1902).
† E. Rutherford, ' Radioactivity,' p. 274.

second change produces no rays, so that the radiation then remains constant for a length of time which decreases with the increase of the time of exposure. This flat portion of the curve of decay lasts for about forty minutes for very short exposures. The third change has a period of about twenty-eight 28 minutes, and gives rise to α rays, so that the curve of

Fig. 2.—Decay of excited activity of Radium for different times of exposure measured by the *a* rays.
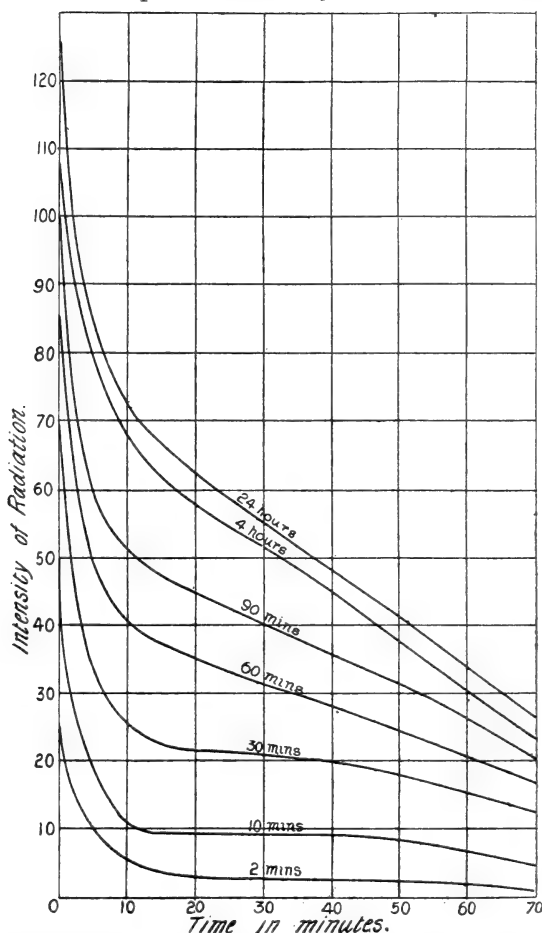
decay measured by the α rays falls regularly to zero several hours after removal. In fig. 2 are given several curves for different times of exposure. The scale to which these curves are plotted shows the relative amounts of activity obtainable in the time of exposure indicated. The decay of the α rays for times of exposure from ten seconds to six days has already

been given by Curie and Danne*. The curves I have obtained
differ from those given by Curie and Danne in two respects.
The latter do not give the rapid initial decrease except for expo-
sures of five minutes' duration and less, and their curves for all
exposures under one hour show a rise near the beginning which
I have observed only when the radiation is measured by the
β rays. In these experiments on the α rays special precautions
were taken, by means of a constant current of air through the
testing vessel, to prevent the results being complicated by any
trace of emanation which might cling to the radioactive wire.

Fig. 3.—Variation with time of the excited activity of Radium for different times
of exposure measured by the β rays.
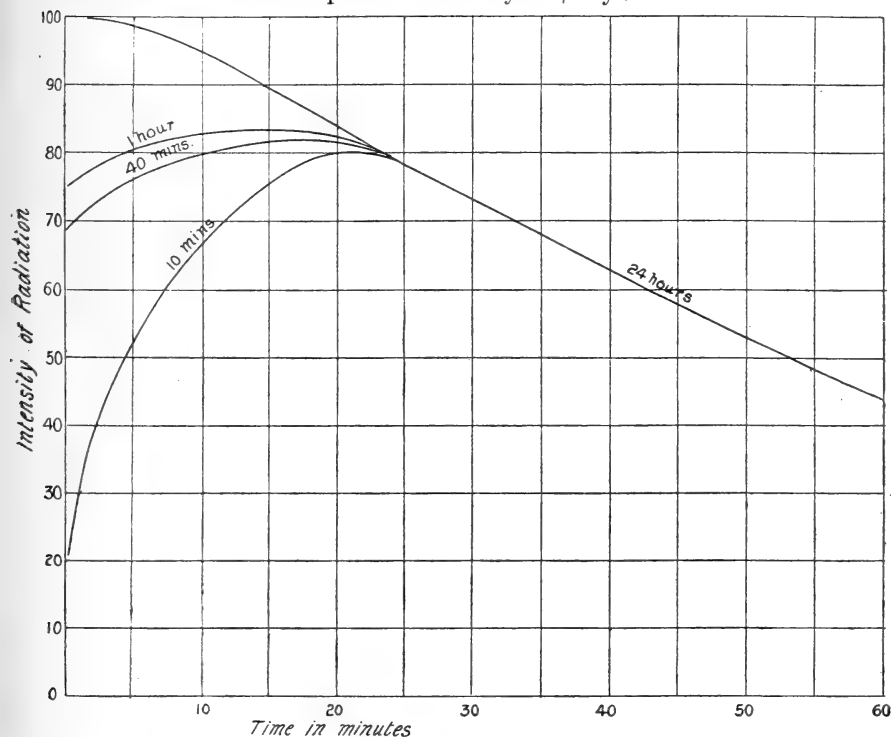
The rate of decay of the β rays was obtained by placing
the radioactive body outside the testing electroscope, whose
walls of tin 1 mm. thick would completely absorb the
α rays. For short exposures the radiation rises at first,
reaches a maximum in about twenty minutes, and then
decays at the same rate as the α rays in the last change. It
is evident, then, that only the last of the three changes oc-
curring in the matter which causes excited radioactivity

* *Comptes Rendus*, cxxxvi. p. 364 (1903)

gives rise to the production of β rays. In fig. 3 are given curves for the β rays for different times of exposure. The curves for all times of exposure have a common rate of decay after a certain time; the first part of the curves here given indicates the rise in the radiation for the different times of exposure plotted to the scale of the curve of decay for a long exposure.

The γ rays were measured in a similar manner: a screen of lead was interposed between the radioactive body and the electroscope to cut off the α and β rays. The γ rays evidently accompany the production of β rays, as their rate of decay was the same in all cases examined. Fig. 4 shows the decay

Fig. 4.—Excited activity of Radium for different times of exposure measured by the γ rays.
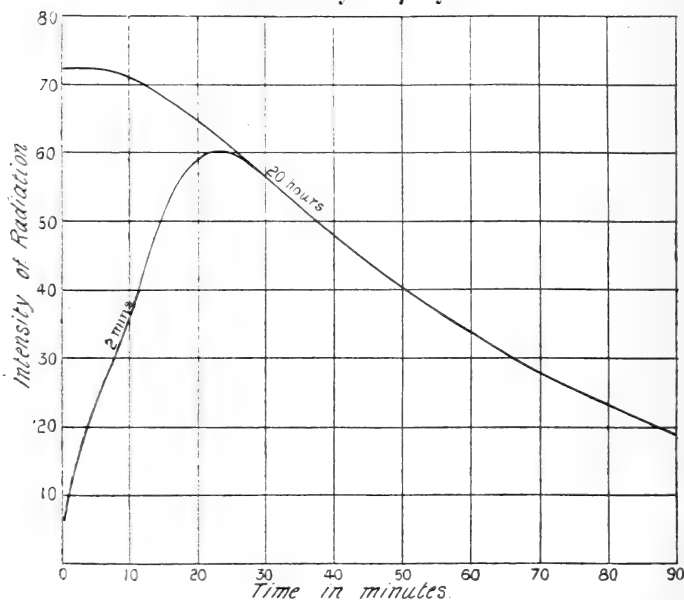
of the γ rays for a long and a short exposure, both plotted to the scale of the long exposure.

Excited Activity from Actinium.

Actinium, like radium and thorium, continuously emits a radioactive emanation which has the power to excite a temporary radioactivity on a negatively charged conductor exposed to it. The rate of decay of this excited activity has been given in a recent paper by Debierne[*] as a gradual

* *Comptes Rendus*, no. 138, p. 411 (1904).

decrease according to an exponential law, half the radio-active matter being changed in about forty minutes. There is, however, in addition an increase at the beginning for short exposures to the emanation, analogous to that observed in the case of thorium, and pointing to the existence of an initial "rayless" change after the concentration of the positive carriers on the electrode. The initial rayless change in the case of the actinium excited is very rapid, half the matter being changed in one minute and a half. For exposures to the emanation of a few minutes' duration, the radiation after removal rises to a maximum in about seven minutes and then begins to diminish, decaying regularly after a time according to the rate determined by Debierne. The equations given for the decay of the thorium-excited activity will be applicable also in this case. The coefficients of the primary and secondary changes can be determined as before from the equation $\frac{N_t}{N_0} = e^{-\lambda t}$. This gives $\lambda_1 = \cdot 0077$ and $\lambda_2 = \cdot 000288$. Inserting these values in the expressions for A, B, and A—B, the curves of decay for different times of exposure can be determined. Fig. 5 gives the curves obtained by experiment for

Fig. 5.—Decay of excited activity of Actinium for different times of exposure.
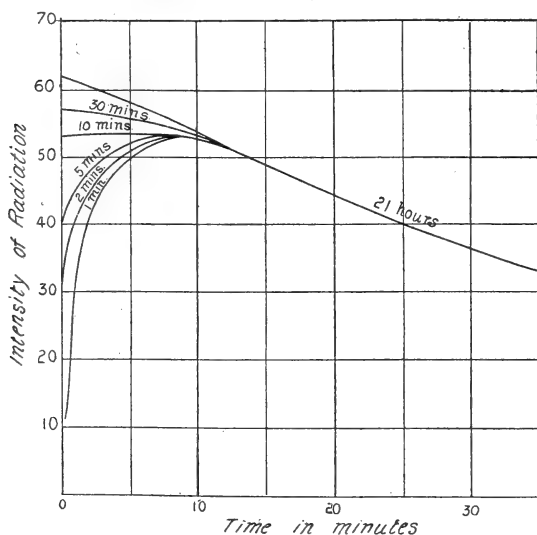
times of exposure from one minute to twenty hours, all plotted to the scale of the long exposure. These experiments

were made with a sample of "emanating substance" of
activity about 300 times uranium, obtained by Prof. Ruther-
ford from Dr. Giesel.   The rate of decay of its emanation
and excited activity leave little doubt that it contains the
same radioactive constituent as the actinium of Debierne.

The excited activity obtained from any of these three
substances may be almost completely removed by placing
the temporarily radioactive body in boiling dilute hydro-
chloric acid.   The activity is not destroyed but remains in
solution, and part of it may be recovered by electrolysing
the active solution between platinum electrodes *.   The
activity thus recovered appears chiefly on the cathode, and,
in the case of thorium, the decay of the radiation from a
cathode thus made active is very capricious, varying from a
decrease to half value in eleven hours to half value in two or
three hours.   When, however, a solution containing the
activity dissolved off a platinum plate made active by ex-
posure to the actinium emanation is electrolysed, the activity
thus concentrated on the cathode has a fairly constant rate
of decay, falling to half value in about a minute and
a half.   The amount of excited activity thus recovered is
always a very small proportion of the whole activity which
is in solution in the acid.

I am indebted to Prof. J. J. Thomson for his kind interest
and assistance in a portion of the work done in the Cavendish
Laboratory, and to Prof. Rutherford for his direction
throughout the whole course of the investigation.

XXXVII. *On the Kinetics of a System of Particles illustrating the Line and Band Spectrum.*

To the Editors of the Philosophical Magazine.

University College of Wales,
Aberystwyth,
May 11th, 1904.

GENTLEMEN,—

IN your issue of May there appears an article by Prof.
Nagaoka, of Tokyo University, "On the Kinetics of a
System of Particles illustrating the Line and the Band Spec-
trum," in which he considers the stability and oscillations
of a ring of negative electrons revolving in a circle about
a positive charge at the centre.   In a letter to 'Nature' of
March 10 I pointed out that such a system is essentially

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* Von Lerch, *Annalen der Physik*, Nov. 1903, p. 745.

unstable and breaks up with extreme rapidity. Nagaoka in his paper states that the system is unstable for some vibrations, but lays little stress on that, and nowhere indicates how unstable the system really is. In fact, he suggests it as a model of the radium atom.

Consider the following equations given by Nagaoka :—

$$\omega^2 = S - \mu K. \quad . \quad . \quad . \quad (11) \text{ p. } 450$$

This is the equation of steady motion. The sign of K is wrong in the text ; it would correspond to an attraction instead of a repulsion between the electrons of the ring.

$$n'^2 = S - \mu J ; \quad . \quad . \quad . \quad . \quad (9) \text{ p. } 449$$

and lastly

$$n^4 - \{3\omega^2 - 2S + \mu(N - L)\}n^2 + 4\omega\mu Mn$$
$$- \mu N(\omega^2 + 2S + \mu L) - \mu^2 M^2 = 0 \ . \quad (12) \text{ p. } 450$$

This equation is also given wrongly by Nagaoka. In Maxwell's paper on Saturn's Rings (Collected Papers, vol. i. p. 316, equation (22)) we have the same equation for attracting satellites in the ring in place of electrons. That case gives the one under consideration by changing the sign of the mass of the satellites. This change gives the equation written down.

The notation in all these equations is Nagaoka's, as given in his paper.

The mistakes just alluded to arise from dropping a minus sign on the left-hand side of the first of equations (7) on p. 448.

Using equations (11) and (9) in (12), it becomes

$$n^4 - \{\omega^2 + \mu(N - L - 2K)\}n^2 + 4\omega\mu Mn$$
$$- \mu N\{\omega^2 + 2n'^2 + \mu(2J + L)\} - \mu^2 M^2 = 0.$$

Now suppose we consider a ring with an even number $2p$ of electrons ; and the most influential disturbing wave, for which Nagaoka's number h becomes p. His equations (6) p. 448 show that in this case $M = 0$.

By the same equations we have

$$L = \Sigma\left(\frac{1}{2} \frac{\sin^2 h\theta \cos^2 \theta}{\sin^3 \theta} - \frac{\cos^2 h\theta}{\sin \theta}\right), \quad K = \Sigma \frac{1}{2 \sin \theta},$$

$$J = \Sigma \frac{\sin^2 h\theta}{2 \sin^3 \theta}, \quad N = \Sigma\left(\frac{\sin^2 h\theta \cos^2 \theta}{\sin^3 \theta} + \frac{\sin^2 h\theta}{2 \sin \theta}\right),$$

where a few misprints have been corrected. (Maxwell, *loc. cit.* p. 314, equations (8).)

We see at once that

$$N - L - 2K = J - \Sigma \frac{\sin^2 h\theta}{2 \sin \theta} = J - K,$$

$$2J + L = 3J - K, \quad N = 2J - K *,$$

because $\theta = \frac{\pi i}{2p}$, $h\theta = \frac{\pi i}{2}$, where i is an integer. Hence the equation is

$$n^4 - \{\omega^2 + \mu(J - K)\}n^2 = \mu(2J - K)\{\omega^2 + 2n'^2 + \mu(3J - K)\}.$$

With Nagaoka, $\mu = \frac{e^2}{2ma^3}$, where e is the charge of an electron in electrostatic units, m is its mass, and a is the radius of the orbit. Taking $\frac{e}{m} = 5 \cdot 5 \times 10^{17}$, $e = 3 \cdot 4 \times 10^{-10}$, $a = 10^{-8}$, all in C.G.S. units, we have $\mu = 10^{32}$ approximately.

I find that with sufficient accuracy for most purposes, at any rate for $2p = 10$ or more, we have

$$\tfrac{1}{2}K = \cdot 366 \times (2p) \log_{10} (2p).$$

$$\tfrac{1}{2}J = \cdot 017 \times (2p)^3.$$

For $2p = 10$, $\tfrac{1}{2}K = 3 \cdot 6$, $\tfrac{1}{2}J = 17$.

Suppose the period of revolution to be the period of sodium light ; then $\omega^2 = 10^{31}$. Unless the angular velocity be much greater, we may clearly neglect ω^2 ; and similarly we shall neglect n'^2. The above values give

$$n^4 - \mu \times 30 \times n^2 = \mu^2 \times 2500,$$

$$n^2 = 67\mu \text{ or } -37 \times \mu.$$

The modulus of the imaginary roots is about 6×10^{16} or about 120 times the number of revolutions per second. Thus these disturbances would increase $2 \cdot 7$ times during $\frac{1}{120}$ of a revolution. It is difficult to see how such a system could represent even so unstable a thing as the radium atom ; nor do I think that Nagaoka is justified in speaking of his system as " generally stable."

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* *Note* (June 9th).—These values are only approximate : strictly $N - L - 2K$ lies between J and $J - K$, $2J + L$ between $3J - K$ and $3J - 2K$, and N between $2J$ and $2J - K$; the order of n^2 is not materially altered.

If the velocity of the corpuscles were as great as that of light, the stability would be about 1400 times greater, that is, the same disturbance would only be produced after about 12 revolutions.

[NOTE (*June 12th*).—In a letter to 'Nature' of June 9th Prof. Nagaoka states that his system is not electrically neutral, but that the central charge is very large compared to the total charge of the ring; the remaining negative electrons necessary to make the whole system neutral are left out of account. This no doubt renders part of my criticism inapplicable, because by sufficiently increasing the central charge the ring can certainly be made stable; but is this allowable?

It is not clear that the negative electrons outside the ring can be left out of account in discussing the ring, for their charge is comparable in amount with the central charge; let us however assume, with Prof. Nagaoka, that they can. The equation of steady motion, (11) p. 450, gives $\omega^2 \doteq S$, for K is small because the negative electrons of the ring are few. With $S = \dfrac{\nu e^2}{ma^3}$ we get $\omega^2 a^2 = \dfrac{\nu e^2}{ma}$; ωa is the velocity of the ring: this must be less than the velocity of light, else the problem is fundamentally altered; with the usual values of $a, \dfrac{e}{m}$, and e, this gives $\nu < 50,000$. Next, the equation (12) p. 450 gives for the imaginary frequency $-n^2 = 6\mu J$, whence

$$\sqrt{-1}\,\frac{n}{\omega} = \sqrt{\frac{3J}{\nu}};$$

for a ring of 10 electrons $J \doteq 34$, and with $\nu = 50,000$ we get

$$\sqrt{-1}\,\frac{n}{\omega} \doteq \frac{1}{20};$$

in other words, the disturbance would increase 2·7 fold in about 3 revolutions. The system is again far too unstable.]

<div align="right">Yours faithfully,</div>

<div align="right">G. A. SCHOTT.</div>

XXXVIII. *The Relation of Electrode Fall in Gases to the Contact Potential Series.* By CLARENCE A. SKINNER, *Ph.D., Assistant Professor of Physics in the University of Nebraska, Lincoln*.*

IN a former paper † the writer suggested that the series of metals, obtained when these were arranged according to the magnitude of the fall of potential at the electrodes when the glow-discharge passes through a rarefied gas, is closely related to the contact-potential series.

In this paper are presented the results of experiments made to study this relation. The hypothesis is : The fall of the potential at the *cathode* in a given gas decreases as the *electropositive* value of the metal increases ; the *anode* fall, on the other hand, decreases as the *electronegative* value of the metal increases.

Plan and Preparation.

The metals tested were mainly chosen with reference to their position in the contact-potential series. They were: platinum, silver, gold, copper, steel, nickel, bismuth, antimony, tin, lead, cadmium, zinc, aluminium, and magnesium. The gases used were : hydrogen, on account of the great difference it gives between the cathode-fall of platinum and aluminium; nitrogen, because of its chemical neutrality; and oxygen, because of the vital relation, in theory, between the contact potential of the metal and its affinity for oxygen.

The plan of procedure was to measure in the given gas the electrode fall of all the metals under, so far as possible, exactly the same conditions. It was found impossible to obtain the fall sufficiently definite by testing first one metal, dismantling the discharge-tube, then introducing a new one and repeating the measurement. The fluctuation in value given by the same metal under successive tests of this kind was generally great enough to prevent a satisfactory comparison of the whole series. Neither were satisfactory results obtained by mounting the several metals in similar discharge-tubes and setting these up in open connexion with each other. A discharge passing through one tube produced often a much greater variation of the electrode fall in the neighbouring tubes than in the active one.

The scheme finally adopted, which proved wholly satisfactory, was to bring, in rapid succession, the metals to be tested into the same position in one discharge-tube and measure the electrode fall under invariable conditions, the results being discarded if a disturbing change took place in the gas during the series of observations.

Fig. 1 represents a vertical section of the discharge-tube ending below in a larger cylindrical chamber (axis vertical), the whole of glass construction. Ten electrodes E (circular disks 15 mms. in diameter) were attached to the face of a circular glass plate at equal intervals by hard rubber caps K (see also plan in fig. 2). This glass plate, mounted in the cylindrical chamber, could be rotated about its axis by the key introduced through the ground joint J, and by this means any electrode brought into position at the lower end of the

·discharge-tube. A hard rubber disk, D, studded with brass rivets C, and fixed to the circular plate, served to complete

Fig. 1.   Fig. 2.
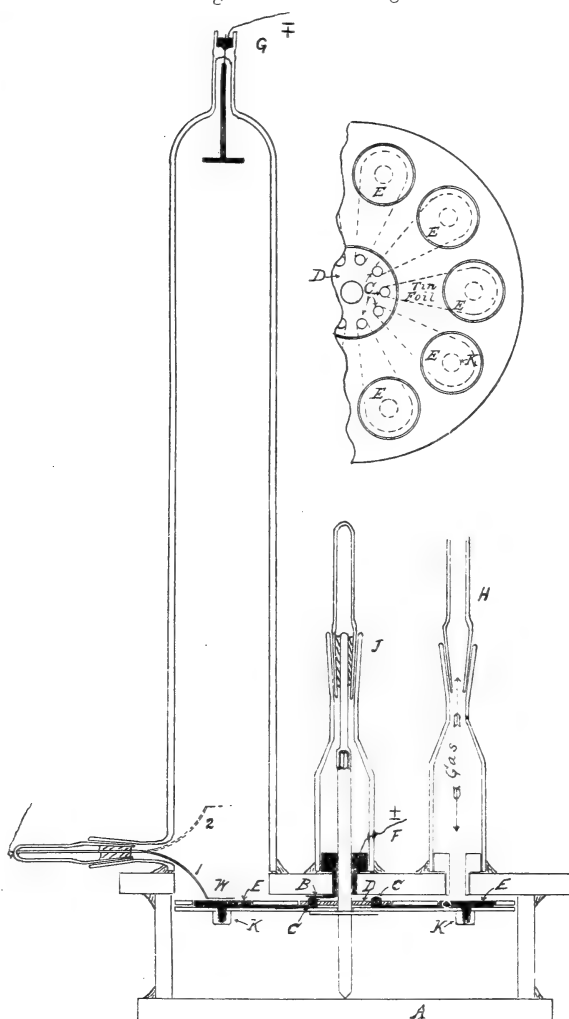
the [electric circuit from F through the metal collar and attached spring-brush B to the tinfoil strip leading to its corresponding electrode E. By this device all electrodes, with the exception of the one in use, were isolated from the

circuit. To confine the discharge solely to the face of the electrode, a second circular glass plate properly cut was pressed closely on top of the first, covering the tinfoil strips and fitting the rim of the electrodes. Leakage from the brush B to the gas was avoided by reducing the space between the upper face of the electrode-plate and the roof of the chamber to about $\frac{1}{2}$ millim. thickness. The discharge-tube was 3 cms. in diameter and about 20 cms. long. A fine aluminium wire W, sheathed up to about 6 millims. of the end with fine glass tubing (outer diameter $\frac{1}{2}$ millim.), served at position 1 for obtaining the fall of potential at the anode. If turned into position 2, $2\frac{1}{2}$ centims. from the electrode, the same wire served for obtaining the cathode-fall, and, if desirable, the fall in the gas over the intervening space. All permanent joints of this apparatus which could not be fused were cemented first with water-glass (sodium silicate), then, in addition, they were covered on the outside with de Khotinsky's laboratory-cement. The base-plate A, which had to be removed to allow for repolishing the electrodes, was sealed by simply applying the latter cement on the outside of the juncture, care being taken that it did not extend to the inner part. This tube proved to be in all cases absolutely air-tight.

The platinum, gold, copper, lead, and aluminium electrodes were made from sheets as obtained commercially; silver and nickel from plates used in electroplating; bismuth, antimony, tin, cadmium, zinc, and magnesium were cast. The steel electrodes were made of good tool-steel not hardened. All electrodes were given for *each* observation, when not otherwise designated, a fresh mirror-polish, using *new* felt as a polishing-cloth and *new* cotton-cloth for cleaning. As polishing material rouge was used for platinum, silver, and gold ; but for the rest of the metals infusorial earth mixed with tallow proved more satisfactory. After polishing, the metals were very carefully cleaned by rubbing lightly with a dry cloth. They were then mounted quickly in the tube, which was immediately evacuated and flushed several times, and finally left standing in open connexion with a drying-chamber in a few millimetres of the gas. When the gas was dry the metals usually preserved their surface untarnished for several days—even in oxygen, lead excepted. At least two specimens of each metal were tested, but in no case was a notable difference in the different samples observed.

The gases were carefully prepared and stored in large flasks which had been previously cleaned, then dried and freed from occluded gas and moisture by standing highly exhausted

several days in connexion with a drying-chamber containing phosphorus pentoxide. Pressure readings taken from day to day during this time indicated the rate at which occluded gas was given off. In these flasks mercury valves were used, so that while stored the gas came in contact with only glass and mercury. Observations made with freshly prepared hydrogen and that which had been stored for weeks were at no greater variance than those obtained from different tests with the stored gas.

Hydrogen was generated by electrolysis of dilute phosphoric acid ; it was purified by passing through a solution of potassium permanganate, a tube containing pieces of caustic potash, another filled with phosphorus pentoxide, and finally through a chamber containing pure sodium to the storage-chamber.

Nitrogen was obtained by passing air over heated phosphorus and through the same series as hydrogen.

Oxygen was obtained by heating a mixture of potassium chlorate and manganese dioxide. It was purified by passing through a solution of caustic potash, a tube containing sticks of the same, and finally a drying-tube of phosphorus pentoxide.

Gas-pressures were measured by means of a MacLeod gauge. The electric current was furnished by a battery of storage-cells and measured by a Weston milliammeter. A Kelvin quadrant-electrometer was used for the potential-measurements—the anode-fall being placed between the quadrants and a definite potential on the needle; the cathode-fall being placed on the needle and a definite potential between the quadrants. The anode-fall could be observed accurately to one-tenth volt ; the cathode to less than one volt.

Preliminary Tests.

In hydrogen and oxygen a preliminary study of both cathode and anode fall was made, the controlling conditions in nitrogen* having been previously determined. The metals and discharge-tube were prepared and left over-night as described above. On the next day the tube was again flushed several times with the gas, great care being taken to avoid entrance of moisture through the pumping apparatus. Gas was finally let into the tube at the desired pressure, and after standing half an hour or more the fall of potential at the cathode (or anode) for the metals quickly observed—no metal

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* Wied. *Ann.* lxviii. p. 752 (1899) ; Phil. Mag. *l. c.*

being kept in use longer than was absolutely necessary.   This plan was followed throughout the investigation.

Cathode Fall in Hydrogen.—The cathode-fall in hydrogen, as in nitrogen (*l. c.*), increases for small currents in a linear manner with the current-density; it also increases, for the same current-density, as the gas-pressure diminishes—this latter, however, not in the simple manner found for nitrogen. Here also the rate of increase both with current-density and with decreasing gas-pressure is notably greater for those metals possessing the larger minimum, or normal, cathode-fall.   In nitrogen the difference in fall between the metals is so small that this factor, though observable, was considered negligible. Observations in hydrogen are represented by curves in fig. 3. They show the relation between the electric current and the cathode-fall for platinum, zinc, and aluminium—each with the gas at a pressure 1·0, 1·72, and 3·0 millims. of mercury. At each pressure the observations were made for all three metals without changing the gas.   Those at the different pressures were also taken, in the sequence 1·72, 3·0, and 1·0 millims., without in the meantime repolishing the electrodes.   The sequence of observations for each curve, indicated by the arrows, shows the magnitude of the change which took place with use.   This was always an increase, generally more rapid with those metals possessing the greater fall.   The curves end on the left at about the minimum current-density, at which point the area of the discharge begins to contract with a further reduction of the current, and the cathode-fall becomes constant, as shown by the curve Al 1·72 *.   In addition to these, a set of observations were made at 2 mms. gas-pressure, comparing platinum and gold as cathodes.   The difference in cathode-fall of these remained 5 volts throughout a range from 0·6 m.a. to 3 m.a.   From these results it is to be concluded that the cathode series for hydrogen is the same whatever be the current-density and gas-pressure at which it be taken.

The Cathode Fall in Oxygen.—This exhibits the same qualities as hydrogen, with the interesting distinction that while the minimum cathode-fall is of about the same magnitude, its increase with current-density, and with diminishing

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* While making these observations it was also noticed that the minimum current-density is smaller for the metal possessing the larger cathode-fall; for example, at 3·0 mms. pressure the discharge area for aluminium began to contract when the current was reduced below 1·3 m.a.; for zinc, below 0·8 m.a.; and for platinum, below 0·65 m.a. At lower pressures this transition-point becomes unobservable.

gas-pressure, is strikingly less than for hydrogen. This is
evident when the curves for oxygen (fig. 4), taken at pressures
0·8 and 1·17 mm., are compared with those for the same
metals in hydrogen (fig. 3) at 1 mm. pressure. This increase
for oxygen is also smaller than for nitrogen, though the
minimum fall for nitrogen is about 50 per cent. less than for
oxygen.

Fig. 3.
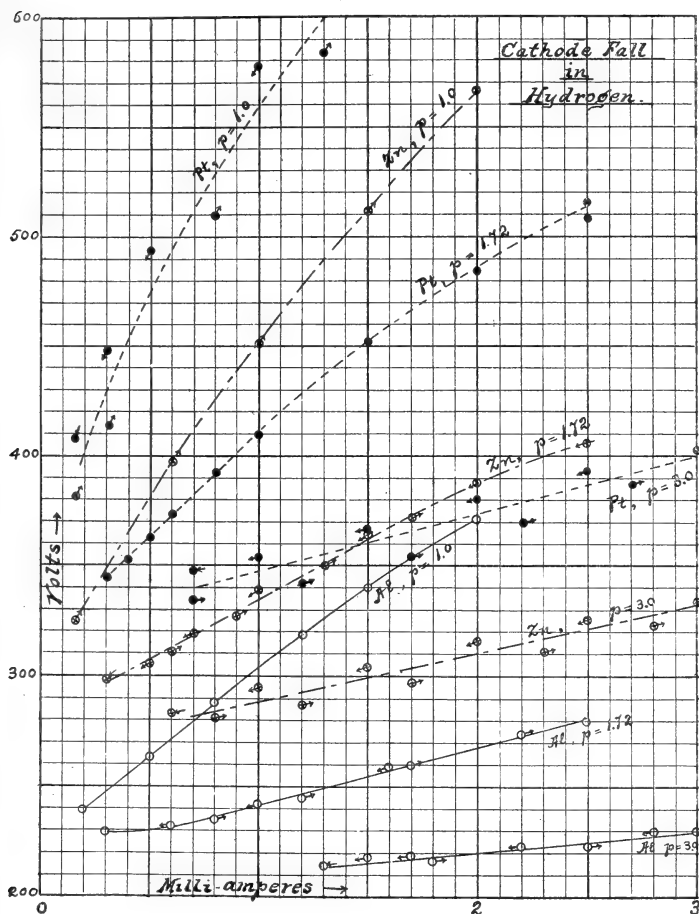
Of present interest, however, is the fact shown by the
experiments with all three gases. *The series of metals
arranged according to the magnitude of the cathode-fall is the*

same whatever be the gas-pressure and current-density under which it be obtained.

Fig. 4.
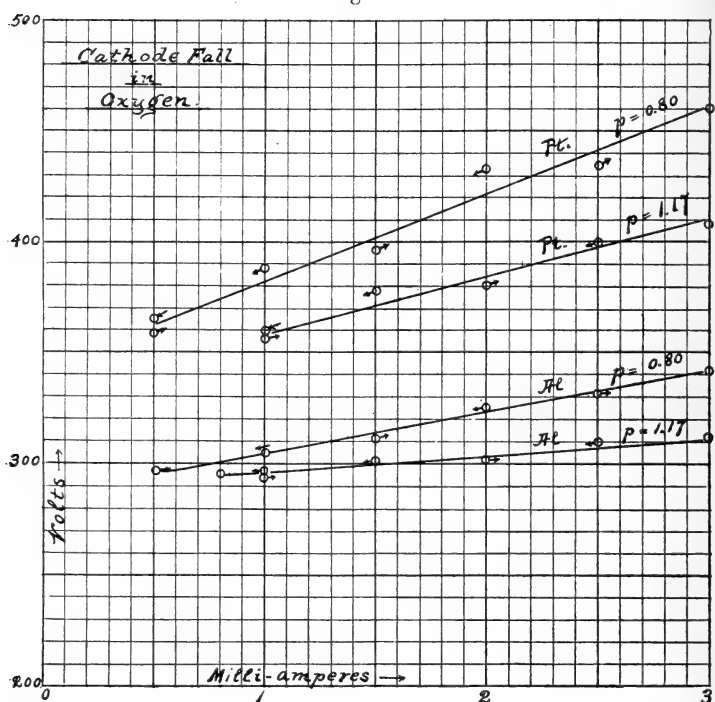
The Anode Fall.—In hydrogen and oxygen, as in nitrogen, the anode-fall for gas-pressures not lower than that giving an unstriated luminosity in the gas, is practically unaffected by changing the current-density. Earlier observations in nitrogen[*] indicated that the anode-fall increased with gas-pressure—tests being always confined to conditions giving an unstriated luminosity in the gas. Similar experiments performed in hydrogen and in oxygen, however, indicate the anode-fall to be *constant* with increasing gas-pressure. These latter observations, placing the former results from nitrogen in question, called forth in the present investigation a careful study of the anode-fall in this gas also. A notable difficulty in determining the anode-fall arises from the gas in the immediate vicinity of the anode possessing a potential gradient of negligible magnitude only at the lower pressures (approaching that giving the striated discharge). The observed

[*]Wied. *Ann. l. c.*

fall may be partly made up of that in the gas in a region beyond a space covered by the real anode-fall. Since the potential gradient in the gas increases with the pressure, an observed increase in the anode-fall with the gas-pressure may then be attributed to this extra quantity. Such a view was strengthened on comparing the potential gradient in hydrogen and oxygen with that for nitrogen. Both hydrogen and oxygen were found to possess near the anode a gradient only about one-third the value of that observed for nitrogen. Thus we find the disturbing element with these gases considerably smaller than it is for nitrogen.

To obtain as nearly as possible the abrupt fall of potential between the anode and the gas, the test-wire W (fig. 1) was first adjusted parallel to and in contact with the face of the anode, giving a zero potential between them when the current was passing. Observing the electrometer, the wire was cautiously shifted out of contact, this being indicated by a sudden deflexion of the electrometer-needle. The smallest deflexion thus obtained was taken as measuring the anode-fall. The observations for the three gases are given in Table I. The

TABLE I.—Anode-Fall in Volts.   Different Gas-pressures.

Gas.	Gas-press.	Pt.	Ag.	Au.	Cu.	Fe.	Ni.	Sn.	Pb.	Cd.	Zn.	Al.
Hydrogen	2·02	19·8	20·5	22·7	22·8	22·7	23·0	......	22·8	23·7	23·6	
	2·95	19·7	20·5	22·9	23·9	22·5	23·3	......	23·6	23·7	23·6	
Nitrogen.	1·37		18·5	18·6	......	19·0	19·4	19·4	......	19·1	18·5	21·9
	1·80		18·8	18·8	......	19·6	19·9	19·9	......	20·2	19·5	22·5
Oxygen.	1·20	22·2	......	24·3	23·2	23·8	23·5	24·2	......	24·2	......	23·9
	2·04	23·3	......	24·2	23·4	23·6	23·6	25·4	......	25·6	......	25·9

metals having been prepared as usual, their anode-fall for the lower gas-pressure was observed, then without repolishing them it was observed again in the fresh gas at the higher pressure. Several different metals were tested for the purpose of eliminating from the conclusions the effect of a change produced by use. The results with hydrogen show for seven metals the same value at the two pressures used ; for copper and lead there is about 5 per cent. increase in fall for 50 per cent. increase in gas-pressure, which is undoubtedly due to a change in the electrode surface. In oxygen, four metals give the same value at the two pressures observed, while four show an increase in value of the fall with pressure. From the fact that there is generally an increase in electrode

fall with use, the conclusion is warranted that here, as in hydrogen, the unchanged values are the true ones. On the other hand, nitrogen gives for all metals a greater fall at the higher pressure, there being an increase of about 3 per cent. in the fall with a 30 per cent. increase in the gas-pressure. This is smaller than that previously reported, yet the question as to the change with gas-pressure in nitrogen must still remain unsettled. For the present investigation, however, the above results at least show the anode series, like the cathode series, to be independent of the gas-pressure and current-density under which it may be obtained.

The Electrode Series.

In Hydrogen.—Observations were first made on the cathode. Using metals representing the contact series, preparations as already described were carried out. To facilitate the rapidity with which observations of the electrode fall were made, the contact-brush (B, fig. 1) was so arranged that the discharge could take place from the next metal used just before the circuit from the preceding one was broken. By this device, the current through the tube remained uninterrupted during the series of observations, and oscillation of the electrometer-needle avoided. This arrangement allowed the cathode-fall to be obtained in less than ten seconds after it was brought into action, which was generally advantageous as the cathode-fall tended to increase under action of the current. For this reason also the fall was obtained by a single deflexion of the electrometer-needle from its zero position—the latter being observed at the beginning and at the end of a series. Several independent tests were made, and though there was a marked variation in the magnitude of the fall under the different tests, yet they agreed with one another in placing the metals in the same series. Steel gave extremely erratic values; it also increased in fall so rapidly under action of the current that no definite value could be ascribed to it [*].

The results of three consecutive determinations of the cathode series are given in Table II., for each of which the metals were newly polished. They are arranged in the table according to their position in the contact-potential series [†]. It is to be seen that, with the exception of gold, the cathode-fall decreases in going from the platinum to the aluminium

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[*] For the same reason, tin could not be satisfactorily used as anode in either hydrogen or nitrogen.

[†] The potential series is taken from Jahn's *Grundriss d. Elektrochemie*, p. 4. The position of nickel is not given by Jahn; it is placed here as slightly less than bismuth, as obtained from Winkelmann's *Handb. d. Physik*, III. i. p. 119.

end of the series. In hydrogen, then, our hypothesis concerning the cathode series is supported by the experimental results. Copper, tested in two other series of observations not here recorded, fell consistently between silver and nickel in both cases.

TABLE II.—Cathode-Fall. Metals compared. Current 1·0 m.a.

Gas.	Gas-press.	Pt.	Ag.	Au.	Cu.	Fe.	Ni.	Bi.	Sb.	Sn.	Pb.	Cd.	Zn.	Al.	Mg.
Hydrogen.	2·03	399	406	386	......	......	397	......	......	389	382	359	326	269	266
	2·01	392	398	361	......	......	386	......	......	372	366	363	326	255	251
	2·00	373	380	......	......	......	381	......	......	378	375	375	324	252	255
Nitrogen.	1·23	252	258	258	258	255	237	......	......	......	243	266	265	217	
	1·25	260	264	264	264	26	243	......	......	......	240	270	271	219	
	1·24	253	263	......	262	258	......	254	255	......	247	......	......	219	228
Oxygen.	1·21	362	371	364	350	339	......	364	......	......	370	349	347	302	
	1·20	356	367	358	353	336	......	357	......	......	370	346	345	301	
	1·20	360	......	361	354	339	331	......	358	360	......	348	... ..	305	294

TABLE III.—Anode-Fall. Metals compared.

Gas.	Gas-press.	Pt.	Ag.	Au.	Cu.	Fe.	Ni.	Bi.	Sb.	Sn.	Pb.	Cd.	Zn.	Al.
Hydrogen.	1·73	18 0	18·4	20·1	18·9	22·1	......	19·9	20·6	20·8	......	20·7	20·4	
	1·71	18·4	18·8	19·5	19·7	18·5	19·9	......	......	......	20·3	......	20·2	20·1
	1·70	17·3	17·7	20·7	20·0	......	19·3	18·0	......	20·0	......	19 9	19·1	19·7
Nitrogen.	1·39	18·8	19·1	21·1	19·7	19·7	20·3	......	......	......	20·6	19·7	19·6	22·2
	1·37	18·5	18·6	19·9	19·0	19·4	19·4	......	......	......	20·0	19·1	18·5	21·9
Oxygen.	1·20	22·2	......	24·3	23·2	23·8	23·5	......	23·5	24·2	......	24·2	......	23·9

The Anode Fall.—Three similar sets of observations of the anode-fall in hydrogen are recorded in Table III. The fall for most metals remaining very constant under the action of the current, it was measured more deliberately than the cathode-fall as a mean of the " right " and " left " deflexions of the electrometer-needle. Here we find in general the anode series to be the reverse of that of the cathode; that is, the reverse potential series. Gold, however, is high here, while it is low in the cathode series. The values for steel fluctuate considerably, and for magnesium the anode-fall for all gases increases so rapidly with use that it was impossible to obtain a reliable value for it.

The Electrode Fall in Nitrogen and Oxygen—In Table II.

are also given the results of observations on the cathode-fall in nitrogen and in oxygen; in Table III. correspondingly the anode-fall. In nitrogen, the cathode, anode, and potential series are consistent with each other for eleven metals; but in relation to the potential series the cathode-fall for cadmium and zinc is abnormally high, while the anode-fall for these metals is correspondingly low. In nitrogen, tin increased with use so rapidly that its value as an electrode could not be reliably fixed.

In oxygen, results were duplicated with a greater degree of accuracy than in either hydrogen or nitrogen. The electrode series is in the main consistent with the potential series, steel and nickel being marked exceptions. The high value of the cathode-fall for lead is attributed to its surface becoming tarnished while standing in the gas. Silver, which maintains a high polish in the inactive gas, was vigorously attacked by it when an electric discharge was passed through the tube. Though it was isolated from the electric circuit, its surface turned a rich brown colour in less than ten seconds after the discharge started. Its cathode-fall was constant, and about

Fig. 5.
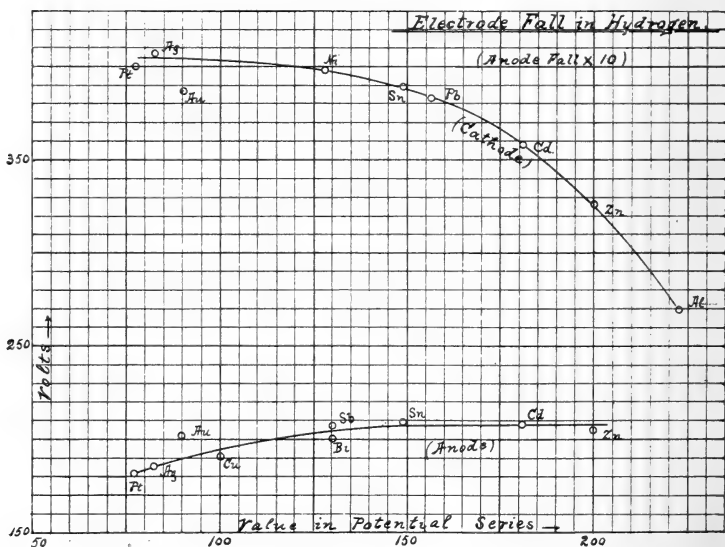
the same as that given by lead. With bismuth a more striking phenomenon occurred. Isolated in the conducting gas or used as cathode, its polish was maintained; but the moment it served as anode its surface darkened as rapidly as

did that of silver. The anode-fall increased rapidly with this change.

To facilitate comparison, the variation of electrode fall with the contact potential of the metal in the different gases is represented by curves in figs. 5, 6, and 7. These are taken
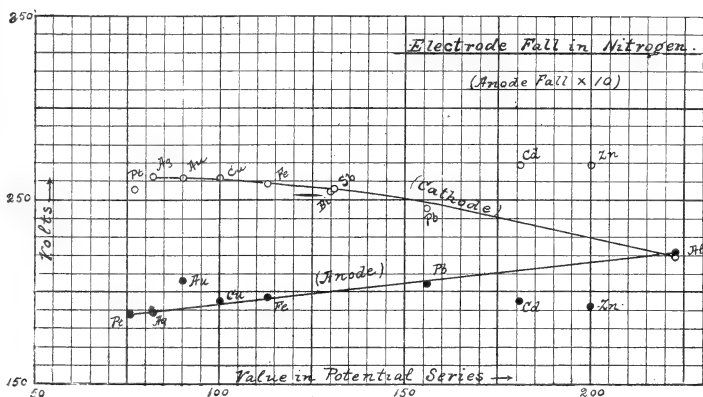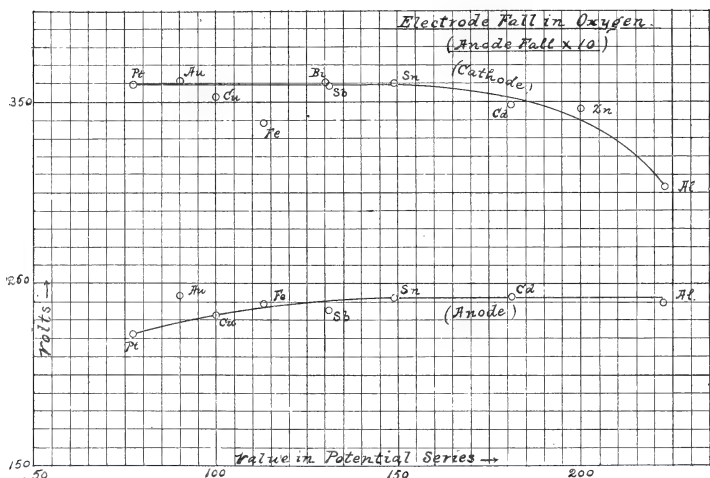
Fig. 6.

Fig. 7.

from the observations given in Tables II. and III. In each figure the abscissæ represent the value of the metal in the contact-potential series (copper being given the value 100, zinc 200). The ordinates represent in the upper curve the

corresponding cathode-fall; in the lower, the anode-fall multiplied by 10.

Our hypothesis appears to be supported throughout by the experimental results. The electrode fall may be represented as a continuous function of the contact potential of the electrode. The curves show some interesting exceptions. Platinum exhibits a slight tendency to drop below its normal value as cathode, but is in line as anode. Gold appears in general to be displaced in the electrode series to the neighbourhood of tin and lead. This is ascribed to the effect of mercury-vapour in that the surface of the gold always bleached while standing *in vacuo*. Gold-foil interposed between the discharge-tube and the parts containing mercury did not prevent this bleaching effect. Being heated to redness, the metal regained its natural tint.

In nitrogen, cadmium and zinc are displaced to the platinum end in the electrode series, the cathode-fall, as already noted, being for these metals abnormally high, while the anode-fall is abnormally low.

In oxygen, copper drops as cathode slightly below its normal position ; but steel and nickel form interesting exceptions in being displaced to the vicinity of zinc in the electrode series. A similar inconsistency is exhibited by these metals when the potential series in sulphuric acid and that given by the heat of combination of the metals with it are compared *.

In conclusion, it should be remarked that, as shown by these curves, the electrode fall is not a constantly increasing quantity in passing from one end of the potential series to the other ; but that, starting with a minimum value at one end, it approaches at a decreasing rate a constant maximum value for the rest of the series. This is clearly marked for all curves except that giving the anode-fall in nitrogen.

April, 1904.

XXXIX. *The Effect of a Luminous Discharge on the Ionization produced by Hot Platinum in Gases at Low Pressures. By* O. W. Richardson, *M.A., D.Sc., Fellow of Trinity College and Clerk Maxwell Student in the University of Cambridge* †.

THE present communication forms the continuation of an investigation the results of which were embodied in a paper read before the Physical Society on June 12, 1903, and published in the Philosophical Magazine for the following

month. In that paper it was shown that the positive leak from hot platinum *in vacuo* fell off in a definite manner with the time, so that a platinum wire which had been heated long enough at a low pressure lost the power of producing positive ionization. It was also shown that such a wire could have its leaking power restored in several different ways. It is with one of these, viz. exposure to a luminous discharge, that the present paper deals.

The sequel will be made clearer if we consider for a moment what happens when a platinum wire, which has been strongly heated so that it exhibits no detectable leak at a temperature of say, 650° C., is placed for a time near the cathode in a vacuum-tube whilst a luminous discharge passes. On heating the wire again, it will be found to discharge positive electricity at temperatures far below 650° C., although it is still incapable of discharging negative electrification. If such an exposed wire be maintained at a constant temperature, it will commence by discharging electricity at a rate which is greater the higher the temperature. Thus at a low temperature we have a small current for a long time, and at a high temperature a big current for a short time. On the view, suggested in the previous paper, that the ionization is due to the decomposition by heat of some foreign substance imparted to the platinum, we might expect that the integral of the current with respect to the time, *i. e.* the total quantity of electricity discharged, would be independent of the temperature. This question has not yet been tested directly, but experiments which it is hoped will decide it are at present in progress.

The alteration in the wire by virtue of which it possesses the power of discharging positive electricity at a lower temperature than heretofore, the author proposes to call the imparted emissibility; this appears to be preferable to the term "induced activity" employed in the former paper, since it avoids confusion with radioactivity. As a means of investigating these effects the measurement of the total quantity of electricity set free when the wire is maintained at any temperature below that at which the normal leak makes itself felt, has been employed; in comparative experiments this temperature was always made the same. Whether the quantity concerned is different for different temperatures is, as has been stated, at present under investigation. In the sequel the quantity of electricity emitted under the above conditions, which measures the imparted emissibility, has also been called the imparted emissibility; since the two uses of the term are not likely to cause confusion, and it is

necessary to give a name to a quantity which occurs so frequently.

It seems advisable to explain at the outset that, despite numerous experiments, the true inwardness of the pheno-mena under investigation is still uncertain. However, a number of definite results have been obtained, of which a brief account might be of some interest. The present paper, then, is merely a short summary of the leading results, as the author considers it inexpedient to publish the full experi-mental details and the numbers obtained until he is better able to judge which experiments are of most importance.

The general type of apparatus employed in these experi-ments consisted of a cylindrical discharge-tube furnished with two flat circular electrodes. Near one of the electrodes was inserted a spiral or loop of fine platinum wire which could be heated to any desired temperature. Most of the experiments consisted in measuring the emissibility imparted to this wire by passing the discharge between the two electrodes under various conditions. This arrangement of apparatus, which may be regarded as typical, was modified in connexion with special experiments in ways which naturally suggested them-selves. The temperature of the platinum wire was deter-mined from its resistance. To measure the emissibility the spiral was raised by means of cells to a potential which was generally 200 volts, but sometimes less ; whilst the nearest electrode was connected with one pair of electrometer quad-rants, the other being earthed. In most of the experiments the temperature to which the wire was heated in order to drive off the imparted effect was about 650° C. Except where otherwise stated, the vacuum-tube discharges used in these experiments were produced by a battery of storage-cells going up to 1000 volts.

In every case cited in the previous paper, the wire which was revived by the luminous discharge formed one of the discharge electrodes. Experiments were therefore first made to see whether this was a necessary condition or whether a wire would not be revived by a discharge passing between two separate electrodes near it. The result of the experiment showed that a large effect was imparted to an insulated platinum wire 3 cms. distant by the discharge between two aluminium wire electrodes 1 cm. apart.

Experiments were next made to test how the imparted effect varied with the distance of the testing wire from the discharge. As it will perhaps serve to make the method of experimenting clearer, a somewhat detailed description of the apparatus used for this purpose will be given.

The discharge passed between two parallel aluminium wire electrodes D D₁ (fig. 1) about 1·5 cm. apart. These were let in through a ground-glass stopper G₁ to the large discharge-bulb which had a diameter of about 20 cms. The electrode

Fig. 1.
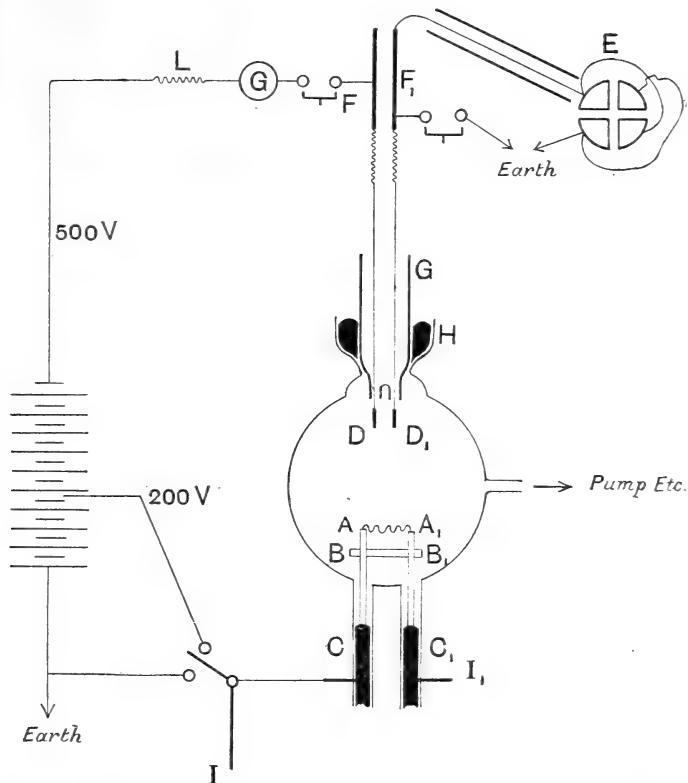
FD could be connected by means of a key with + 500 volts from a storage-battery, through the liquid resistance L and the galvanometer G. The galvanometer G measured the discharge-current, which was kept constant by altering the resistance L. The electrode $F_1 D_1$ was put to earth, except when it was used to measure the leak from the hot wire, in which case it was connected, by removing the key, with the insulated quadrant of the electrometer. The hot wire spiral AA_1 connected the ends of stout iron leads AC, $A_1 C_1$. The system, made rigid by the ebonite cross-piece BB_1, floated on the top of barometer columns at C and C_1 which could be raised or lowered by means of two external reservoirs not

shown in the figure. In this way the platinum spiral could be kept at any desired distance from the electrodes DD_1. It was heated by means of an electric current let in through the mercury (I, I_1) ; its resistance was measured and its temperature kept constant by the method previously used in experiments on the negative leak from hot platinum *. The system CAA_1C_1 was insulated when the discharge passed between D and D_1, but was maintained at $+200$ volts in measuring the imparted emissibility.

The experiments were conducted at a pressure of about ·42 mm., and yielded the following numbers :—

Imparted Emissibility (proportional to)	240	142	100	8	0	0
Distance of spiral from Electrodes (cms.).	1·0	1·8	2·5	3·0	6·2	6·5

Evidently the effect rapidly diminishes as the spiral is moved away from the electrodes, and at distances greater than 4 cms. practically vanishes.

It was shown in the previous paper † that when the testing wire itself was one of the electrodes, the imparted emissibility was considerably greater when it was the cathode than when it was the anode. This led to the suspicion that the effect was a function of the distance from the cathode only ; in which case no emissibility would be imparted by a discharge to a wire forming the anode when the distance between the electrodes was great. On trying this, it was found that with the electrodes 6·5 cms. apart no emissibility was imparted to the anode, whereas the effect on the cathode seemed to be much the same as with smaller distances.

Experiments were next made using the discharge in a cylindrical tube so that everything was symmetrical about the axis of the cylinder. An arrangement was used by which the testing wire could be placed in various positions in the path of the discharge. It was found that the imparted emissibility fell off asymptotically with the distance from the cathode, and had no relation to the distribution of luminosity in the tube.

Finally, an apparatus was set up in which a screen could be inserted at will between the cathode and the testing wire. This was found to cut off the effect, showing that it was due

to something shot off from the cathode, and not merely diffusing about in the neighbourhood of that electrode.

The relation between the imparted emissibility and (1) the duration of the discharge with constant current and (2) with the intensity of the discharge current when this was allowed to pass for equal intervals of time, was next examined. The imparted effect was found to be very nearly proportional to the first power of the duration of the current and to the second power of its strength.

It was shown in the previous paper that the initial positive leak from a hot platinum wire falls off with time in such a way that the current y is given at time t by the expression

$$y = Ae^{-kt} ;$$

where A and k are constants, A being proportional to the amount of substance present giving rise to the ions, and k being the velocity constant of the reaction in which this substance decomposes. A more rigorous examination of this formula has shown that it only applies over a limited range. The positive leak appears always to decay asymptotically with the time, although it does not follow an exponential law. It is probable that in general it is produced by the decay of more than one substance, in which case it would be expressible as the sum of a number of terms of the above type.

Another way of demonstrating the same thing is to measure the total quantity Q of electricity received by the electrometer at various times. Since $\dfrac{dQ}{dt}$ is equal to the current y we have

$$\frac{dQ}{dt} = Ae^{-kt},$$

if the above simple formula holds. Whence

$$Q = Q_0(1 - e^{-kt}),$$

if Q_0 is the charge received by the electrometer after an infinite time has elapsed. In seeking to verify this formula, curves such as those shown in fig. 2 were obtained. It was impossible to fit these curves by so simple a formula as the above; the theoretical curves become much flatter after a few minutes have elapsed, when the two coincide nearer the origin.

It may perhaps be thought that the fact of the leaking current not dying away rapidly enough, *i. e.* that after the lapse of, say, ten minutes quite a considerable fraction of the imparted effect still remains on the wire, would be a serious difficulty in measuring the imparted emissibility. As a matter of fact this difficulty did not arise, since the

measurements of this quantity were made at a temperature
so high that the fraction of the imparted emissibility which
was not driven off was negligible in comparison with the
total.   To make sure that there was no further emission,
readings were taken every minute for three or four minutes
after heating the wire.

Fig. 2.
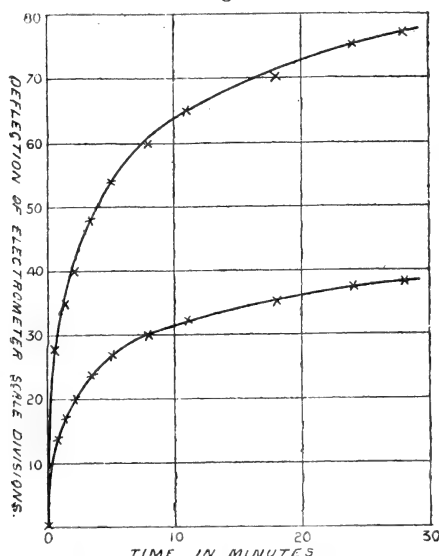
The preceding experiments were all made by passing the
discharge in air.   Oxygen and nitrogen were next tried, and
found to give about as great an effect as air.   Purification
of the nitrogen by heating sodium in it by Warburg's method
did not appear to influence the result.   On letting hydrogen
into the apparatus, a small imparted emissibility was obtained
at first which gradually diminished each time the experiment
was repeated.   Even with fresh hydrogen the effect was
small compared with that in air, so that the discharge in pure
hydrogen does not seem to produce this effect.   The effect
was obtained in mixtures of hydrogen and air, but the amount
appeared to be less than would be given by air at the same
pressure if it were not diluted with hydrogen.   It was also
noticed that when the apparatus had been filled with hydrogen,
the effect of the discharge in air was small at first, but
gradually increased during the first three or four times the
discharge was passed until it reached a comparatively steady
value.

The influence of the pressure of the gas in the discharge-
tube was also examined.   The discharge-current was kept

constant and equal to ·308 milliampere, and allowed to pass for 120 seconds. The imparted emissibility was found to increase from 8·5 to 58 arbitrary divisions as the pressure fell from ·81 to ·33 millimetre. Some experiments were also made at a pressure of ·025 mm., using the discharge from an induction-coil; it was found that for the same current-strength and duration of the discharge, the imparted emissibility at this pressure was about 50 times as great as at a pressure of ·4 mm.

Experiments were also made to see if the emission of positive electricity by a wire which had been exposed to the discharge was accompanied by the evolution of a measurable quantity of gas. The result showed that if any gas were given off, its volume at atmospheric pressure must be less than 1/100 that of the testing wire. This result does not prove that the effect is not due to occluded gas, for calculation shows that if each molecule of gas produced an ion, the volume required would only be one ten-thousandth of the above amount.

The effect of exposure to a luminous discharge on the permanent negative leak from a platinum wire was also tried. It was found that the discharge reduced the leak temporarily to a small value, from which it gradually recovered. At the temperatures at which the experiments were made, it was found that the wire took about fifteen minutes for the negative leak to reach its normal value again. The reduction appeared to be considerably greater than that which would correspond to the positive ions set free.

In all the experiments on the imparted emissibility the testing wire was of platinum. Similar effects were obtained, however, with cathodes of platinum, aluminium, and carbon. The emissibility in a tube which consisted only of platinum and glass did not appear to be materially reduced by cleaning the tube with hot nitric acid and distilled water.

Experiments were next made to see at what temperature the emission of positive electricity from an exposed wire commenced, and how the rate of emission varied with the temperature. It was found that at low temperatures, when the leak first began to be perceptible, it varied with the temperature according to the formula $Ae^{-b/\theta}$. In this respect the imparted effect only furnishes another example of the general law, that in all cases of steady ionization by hot bodies the number of ions produced per second is given by a formula of the type $A\theta^p e^{-b/\theta}$, where A and b are constants and p is a numeric which does not differ sufficiently from zero to make the variation with θ of θ^p comparable with that of the exponential term. When the reaction which produces

the ions is reversible the constant b, with certain restrictions, measures the change of energy when an ion is set free.

Since the emission of the imparted effect follows a formula of this type, it is evident that there is no true temperature at which the leak commences. With the comparatively rough arrangement used in these experiments, the leak was not perceptible till about $350°$ C. Using a much more sensitive arrangement, Strutt[*] was able to detect the ordinary positive leak from hot bodies at temperatures $150°$ lower than this.

It is only at very low temperatures that the imparted leak can be said to be a function of the temperature at all. At higher temperatures, owing to the imparted effect being driven off, the current at constant temperature falls off very rapidly with time. As the theory mentioned previously would lead us to expect, the magnitude of the current and its rate of decay both vary with the temperature in a similar manner.

An interesting way of investigating these phenomena is to maintain a previously exposed wire for equal intervals of time at successively increasing temperatures whilst the leak is being measured. As has been pointed out already, for the first hundred degrees after the leak is capable of detection it rises rapidly with the temperature, of which it is a fairly definite function. As the temperature increases, so does the rate of decay of the current at constant temperature ; so that finally we reach a point where the increase in the value of the current due to raising the temperature is not great enough to make up for the loss of the imparted emissibility in the previous interval. We thus obtain a maximum in the current-temperature curve measured in this way. Provided the imparted effect is due to one substance only, the current will now fall off as the temperature is successively increased —the time the wire is heated at any one temperature being always constant—so that the curve will not rise again until we come to the normal leak. If, however, there are two separate substances producing the imparted emissibility, we might expect two maxima and so on. In this way we have a method of examining the effect which is analogous to fractional distillation.

The curve with points marked thus: $\times$ in fig. 3, represents the current at successive equidistant temperatures when the wire had been allowed to remain at the previous temperature for four minutes. The curve through the points marked thus: $\bigcirc$ represents the leak which this wire gave before exposure to the luminous discharge. It will be seen

* Phil. Mag. [6] vol. iv. p. 98.

that the curve after exposure furnishes two maxima before arriving at the final rise which represents the original leak. The curve with points thus : ⊙ represents the rate of decay of the current at the various temperatures; as has been pointed out already, the curve for the rate of decay closely imitates that for the current itself.

Fig. 3.
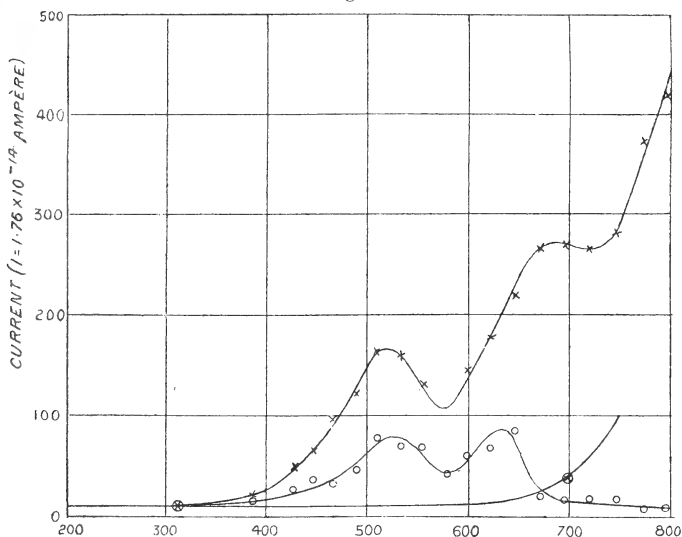
Experiments similar to that whose results are shown in fig. 3 were carried out under various conditions. All the curves showed at least one well-marked maximum whose position lay between 430° and 520° C. Most of the curves obtained with the discharge in oxygen and all of those with air gave a second maximum whose position varied between 600° and 693° C.; with nitrogen the second hump was not so well marked. The position of these maxima naturally depends on the length of time the testing wire is maintained at the various temperatures; in all these experiments the time in question was four minutes.

In the case of oxygen, experiments were made with pressures in the discharge-tube ranging from ·416 to ·02 mm. At the low pressures the discharge was produced by an induction-coil, and the walls of the tube were bright yellow with the phosphorescence due to cathode rays. The variation in the maxima seemed to depend on some obscure change in the cathode, or in the discharge, rather than on the pressure or nature of the gas, although this point requires further investigation. Experiments made with aluminium cathodes

in air gave curves with two maxima, similar to those obtained with platinum; but under the same conditions as regards strength of discharge-current, time of exposure to the discharge and pressure of air in the discharge-tube, the emissibility due to aluminium appeared to be smaller in amount and to furnish maxima at lower temperatures than that due to platinum.

At present it seems premature to discuss at any length the numerous theories which suggest themselves as to the mechanism by which these effects are produced. It seems probable, however, that the effect is connected with the deposition of a material substance on the surface of the testing wire, and it is likely that the phenomenon is connected with cathodic disintegration. The effect simulates in a qualitative manner the consequences of the theory of monomolecular decomposition suggested previously; but, perhaps owing to the complexity of the conditions, the author has not been able to establish the results quantitatively as yet.

Before concluding, the author wishes to correct an erroneous conclusion from the experiments on the emissibility imparted by exposure to air described on pp. 90, 91 of the paper already referred to[*]. Apparently the reviving effect obtained in these experiments was not due to air at all, but to some obscure gas or vapour which was let in along with it. Recent experiments seem to indicate that in some cases the apparatus gets full of a gas which has a very powerful reviving effect, a phenomenon which is under investigation at present. It may be of interest to mention that a platinum wire which has been heated until it has lost its leaking power, and subsequently sealed up in a good vacuum, does not show any appreciable recovery when left for a period of three weeks.

In conclusion the author wishes to thank Prof. Thomson for his kind encouragement and valuable suggestions during the course of this investigation, which was carried out in the Cavendish Laboratory.

XL. *Radium and the Electron Theory.*
By John Trowbridge *and* William Rollins[†].

THE mechanism of electric conduction through metals continues to be one of the greatest mysteries of electricity; and there is no plausible explanation of it unless we accept the theory of electrons.

This theory, as is well known, supposes the existence of small bodies called electrons, which move between the molecules of the metals during the passage of an electric current.

This theory has been much developed by Drude, and has been supported by the calculations of Lorenz. It seems to bring the electrical conduction in metals into close touch with conduction in gases; for in both cases we suppose a movement of small particles endowed with velocity. These particles have a greater free path in rarefied gases than they have between the molecules of a metal; and their action is much modified by the X-rays. This modification is usually attributed to a species of ionization due to a physical connexion between the energy emitted by the X-rays and the transformation of energy witnessed in the rarefied gas. A Geissler tube, for instance, which will not permit the passage of an electric current, when the degree of exhaustion is beyond the point of breaking down with the electromotive force employed, is made conducting when the X-rays fall upon it.

We mention this fact in order to give a significance to our experiments with radium. This remarkable substance also, in common with the X-rays, can affect by what we call ionization the conduction of electricity through gases. The X-rays and certain emanations of radium also can pass through thin sheets of metals, especially sheets of aluminium. We are therefore for the first time in the history of electricity in a condition to test the question, whether radiant energy exhibiting light and passing through a metal can affect the passage of a current of electricity.

One immediately is reminded, in this connexion, of Faraday's attempt to discover whether ordinary light is modified in passing through an electrolyte which is submitted to the action of a current of electricity. He thought that there might be a state of tension which could be detected by polarized light; and he therefore passed a beam of polarized light in the direction of the current and also at right angles to this direction.

The result of the experiment was negative; absolutely no effect was observed. Faraday's custom of publishing both positive and negative results has its advantages, especially in the early days of a science; and especially when it shows us the working of a great mind groping in a region not yet submitted to calculation. Lesser minds must, however, use caution in publishing negative results; for one must have a due regard for brevity of publication and the limits of experimentation.

Can we not, however, imagine Faraday continuing his efforts to discover some connexion between the passage of light through an electrolyte or a conductor and the passage of a current of electricity, if he could penetrate such a conductor by light? In other words, might he not have been tempted, if he had had command of the X-rays or radium, to

discover some action of the energy emitted by the new and wonderful manifestations on a current. Apart, however, from such a view of the working of the great physicist's mind, can we not get a foothold in mounting to the heights of the electron theory by endeavouring to show that the X-rays, or the emanations from radium, do or do not have any discoverable action upon the passage of an electric current through aluminium for instance ?

It must be premised that no mass is ascribed to the electron. Its supposable inertia is due to self-induction. On this conception it does not seem probable that particles shot off from radium, or ions resulting from the radiation of X-rays, could influence such immaterial bodies.

Nevertheless our view of the electron theory might be influenced by proceeding to an actual test ; and looking at the results and limitations of possible experiments. We therefore experimented as follows :—

A metre of aluminium wire, No. 24, was wound in five turns around a thick sheet of lead, which was eight centimetres in length and one centimetre in width. The wire was wound around the longest dimension of this shuttle-like piece of metal, and was insulated from it by thin sheets of vulcanite. The electric current, therefore, passed in one direction along the upper layer of the wire, and in the opposite direction along the lower layer. The lead intervening between the upper and lower layer could serve to confine the radiations from suitably placed radium either to the upper or the lower layer of wire. The lead shuttle with its layer of wire was enclosed in a lead cylinder, and a specimen of pure radium bromide was enclosed at one end of the layers of wire so that its emanations could sweep along the upper or the lower layer of this wire. A lead diaphragm could be used to shut off the entire effects of the radium from the wires.

The wire was made one branch of a Carey-Foster bridge ; a suitable key enabled one to reverse the current through the aluminium wire, and, after adjustment, the wire was exposed to the radiations from the aluminium under the varying conditions of reversals of current ; radiations confined to the lower layer and afterwards to the upper layer. The bridge was competent to detect a change in resistance of one hundredth thousandth of an ohm. On account of the difficulty of distinguishing between a heating effect and what may be called an electrodynamic effect, the observations occupied a comparatively short interval of time. No instantaneous effect was observed : a very slight creeping deviation of the galvanometer-mirror came after a considerable interval of time, which might have been due to

change of temperature. It could not be ascribed with reason to the presence of the radium.

The light from the radium could be seen through a slab of iron an inch thick ; yet this manifestation of energy passed through the aluminium without any apparent effect upon the mechanism of the electric current. Should we reason, therefore, from this negative experiment, that the theory of the immaterial electron is supported, or that a theory of dissociative effect on gases between each molecule of the metallic conductor produced by a current is also negatived ; for the radium emanations, like the X-rays, can produce this dissociative effect in the passage of electricity through gases?

Mr. McKay, graduate student working in this laboratory, has endeavoured to detect the effect of the X-rays in changing the apparent resistance of thin films of metals. The effect, if it exists, is extremely small. He, however, is still continuing his work upon this subject.

Jefferson Physical Laboratory,
    Harvard University.

XLI. *Proceedings of Learned Societies.*
GEOLOGICAL SOCIETY.
[Continued from p. 292.]

April 13th, 1904.—J. E. Marr, Sc.D., F.R.S., President,
in the Chair.

THE following communications were read :—

1. 'The Discovery of Human Remains under the Stalagmite-Floor of Gough's Cavern, near Cheddar.' By Henry Nathaniel Davies, Esq., F.G.S.

2. ' History of Volcanic Action in the Phlegræan Fields.' By Prof. Giuseppe De Lorenzo, of the Royal University of Naples.

In an introductory section the author sketches the general geological structure of the district around Naples, and shows the disposition of the chief lines of fracture by which the Triassic, Cretaceous, and older Tertiary formations were traversed, previous to the commencement of volcanic activity in this part of Italy. He recognizes three chief periods in the volcanic history of the district.

I. The eruptions of the first series took place under the sea during the Pleistocene Period. Their surviving products can be grouped in two distinct divisions, each recording a different eruptive phase. The older of these (*a*) is represented by the piperno and grey pipernoid tuffs of the Campania, which extend under the broad plain into the valleys of the Apeninnes. These deposits consist of grey trachytic tuff, with scattered black scoriæ, and with a varying proportion of non-volcanic sediment washed down from the hills. The vents whence they were ejected are now no longer to be traced as they have been obliterated or covered up by later accumulations. The piperno, well developed at the foot of the Hill of the Camaldoli,

has given rise to some difference of opinion as to its nature and origin. The author is disposed to regard it as a trachytic lava with schlieren, the dark lenticles being made up of such minerals as augite, ægirine, and magnetite, while the lighter matrix is felspathic (anorthose) with a spherulitic structure and microliths of ægirine and augite.

The second phase (*b*) of the first eruptive period is represented by ashes, lapilli, pumice, and sands, intercalated with marine shell-bearing clays and marls, and also with conglomerates and breccias, these coarser kinds of detritus overlying them and varying in thickness according to their proximity to, or distance from, the vents whence these materials were ejected. The accumulations of this epoch were pierced through in the artesian boring at the Royal Gardens, Naples, where they were 330 feet thick.

II. Above the records of the first volcanic period lie those of the second—the yellow tuff, which forms the most widespread and most characteristic of all the volcanic formations of the Phlegræan Fields. It is a yellow or cream-coloured, compact, well-stratified aggregate of trachytic detritus, through which are scattered fragments of tuff and lava. Its average thickness exceeds 300 feet. That it was a submarine accumulation is shown by the occurrence in it of *Ostrea*, *Pecten*, and other organisms. Owing to the general uniformity of its lithological characters, the yellow tuff has not furnished any satisfactory evidence of a definite order of succession in the eruptions to which it was due. Despite prolonged denudation and successive later volcanic vicissitudes, it is still possible to recognize some of the separate vents from which the tuff was discharged, such as the islet of Nisida, the hills of Posillipo, Vomero, Capodimonte, Camaldoli, and Gauro.

III. After the discharge of the yellow tuff from numerous cones and craters scattered over the sea-floor where the Phlegræan Fields now extend, the volcanic tract appears to have been upraised into land, and to have been thereafter exposed to a prolonged period of subaërial denudation. But volcanic activity was not extinct, for a number of vents made their appearance, and discharged a succession of fragmental materials, which differ from the yellow tuff in showing both macroscopically and microscopically a greater variety of composition, and in the proofs which they furnish of a succession of eruptions both in space and time, and a gradual southward shifting and diminution of the vigour of the eruptive energy. The largest and most ancient of the volcanoes of this latest period is that of Agnano, the crater of which is built up of layers of pumice, ashes, lapilli, soft grey tuff, and beds of scoriæ. Not improbably it was from this eruptive centre that the trachy-andesitic lava of Caprara issued. Other volcanoes of the same series are Astroni, Solfatara, the two small vents of Cigliano and Campana behind the north-western slopes of Astroni, the last-named example showing three concentric rings, within the innermost of which a beautifully-perfect little crater marks the last efforts of this vent. The crater-lake of Avernus belongs likewise to the latest group, and perhaps it was the water percolating from this basin to the thermal springs of Tripergole which, in September 1538, gave rise to the explosion that built up Monte Nuovo—the youngest of the cones of the Phlegræan Fields.

April 27th.—J. E. Marr, Sc.D., F.R.S., President,
in the Chair.

The following communications were read :—

1. 'On a New Species of *Eoscorpius* from the Upper Carboniferous
Rocks of Lancashire.' By Walter Baldwin, Esq., F.G.S., and William
Henry Sutcliffe, Esq.

2. 'The Genesis of the Gold-Deposits of Barkerville (British
Columbia) and the Vicinity.' By Austin J. R. Atkin, Esq.

The gold-bearing area of Cariboo (British Columbia) is roughly
confined, within a radius of 20 miles of Barkerville, to the band of
varied crystalline rocks known as the Cariboo Schists, generally
assigned to the Lower Palæozoic Group. The veins follow the strike
but not the dip of the rocks, the gangue is similar to that associated
with the nuggets in the placers, and the reefs show very little or no
oxidized ore. While all the reefs carry gold in greater or less
quantities, none have been found rich enough to account for the
placer-gold. It is the opinion of the author that the placer-gold
has probably been derived from the enriched outcrops of the veins
which once existed above water-level. Such enrichment is due to
two causes : firstly, the leaching-out of pyrites leaving the less
soluble gold in lighter, honeycombed quartz ; and, secondly, to actual
enrichment by precipitation. This may be due to the solubility of
gold in solutions of ferric sulphate, derived from the decomposition
of the pyrites. While the enriched zone was being formed, the
weathering of the surface kept removing the leached outcrop, and
constantly exposing fresh surfaces to atmospheric influences. To
the weathering of these outcrops the rich placers are attributed.
Some of the nuggets in the latter show no signs of attrition, as
though they had been carried to their present position enclosed in
a soluble matrix which was afterwards removed. The denudation
of the reefs and the deposition of gold in the gravels appear to have
taken place in Tertiary times.

May 11th.—Horace B. Woodward. Esq., F.R.S.,
Vice-President, in the Chair.

The following communications were read :—

1. 'On some Quartzite-Dykes in Mountain-Limestone near
Snelston (Derbyshire).' By Henry Howe Arnold-Bemrose, Esq.,
M.A., F.G.S.

At Snelston, $3\frac{1}{2}$ miles south-west by south of Ashbourne, there
is an inlier of Mountain-Limestone surrounded by Keuper Marl.
It is roughly elliptical in shape, the major axis extending for a
distance of about half a mile north-north-east and south-south-west.

The limestone is generally massive, with a few chert-nodules in
the upper parts ; the rock in many places has a broken appearance,
and it contains small hollow spaces ; the large portions of the lime-
stone have been partly or completely dolomitized. The floor and
faces of the quarry are traversed by vertical veins or dykes of
calcite, fluorspar, barytes, calcareous sandstone, and quartzite.

The quartzite of these ' dykes ' is described microscopically. It
consists of angular detritus, quartz-grains with enclosures, a few
small grains of felspar, and a few shreds of mica. The grains are

cemented by silica, and sometimes by calcite. The rock in contact with the dykes sometimes contains quartz in isolated bipyramidal crystals and granular aggregates. The silica is present in the limestone in two forms, which have had an entirely different origin.

Reference is given to examples of sandstone-dykes hitherto described, and then the origin of the quartzite-dykes at this locality is discussed. An important bed of sandstone was found by sinking for a well at Marston-Common Farm; and the same bed is found also in a quarry about 800 feet south of the farm. The microscopic aspect of the rock is precisely similar to that of the dykes. It is at a period later than the Keuper that the silica which cemented the sandstone of the dykes and of the Common Farm appears to have been introduced.
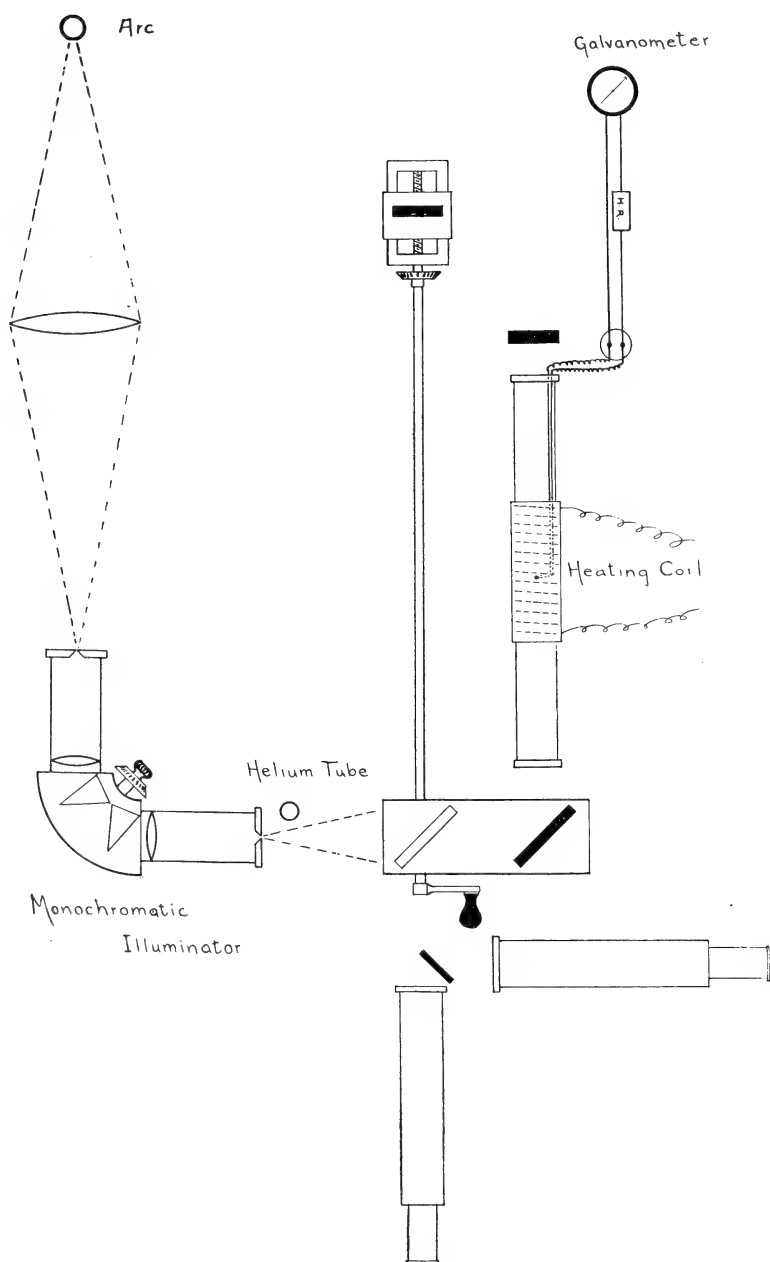
2. 'Phenomena bearing upon the Age of the Lake of Geneva.' By C. S. Du Riche Preller, M.A., Ph.D., A.M.I.C.E., M.I.E.E., F.R.S.E., F.G.S.

Following up his investigations concerning the age of the principal Alpine lake-basins, the author has, during a recent prolonged stay on the Lake of Geneva, examined the low-level gravel-beds and other alluvia in the Rhone Valley, from Geneva to the Jura-bar near Fort de l'Écluse, as well as the high-level gravel-beds of La Côte above Rolle and of the Jorat district above Lausanne, and, further, the rock-formations on both sides of the lake, in view of evidence of flexures as the primary cause of the formation of the present deep lake-basin.

After describing the phenomena around the Lake of Geneva, and comparing them with those around the Lake of Zurich, he is led to the following conclusions :—

(1) The low-level gravel-beds of the Rhone Valley near Geneva are, like the deep-level gravel-beds of the Limmat Valley near Zurich, fluviatile deposits of the second interglacial period, and were formed before the present deep lake-basin came into existence.

(2) The high-level gravel-beds of La Côte above Rolle and of the Jorat district above Lausanne are, like the corresponding deposits of the Uetliberg near Zurich, and of the Dombes and of Lyons, true Deckenschotter. Hence the term 'alluvion ancienne' should, in its proper acceptation, only apply to the high-level deposits.

(3) The formation of the present deep lake-basin of Geneva was, like that of Zurich, primarily due to the lowering of the valley-floor by flexures of the Molasse and its contact-zones, posterior to the maximum glaciation, as evidenced more especially by the reverse dip of the old erosion-terraces.

The author holds that the concord of evidence in the two cases strengthens the conclusion, already arrived at by analogy in his previous paper, that the Lake of Geneva, together with the other principal zonal lakes between the Alps and the Jura, was formed under similar conditions and at the same time as the Lake of Zurich, that is, towards the close of the Glacial Period; indeed, the phenomena in support of that view are, in the case of the Lake of Geneva, on a grander scale, more striking, and, if anything, more conclusive.
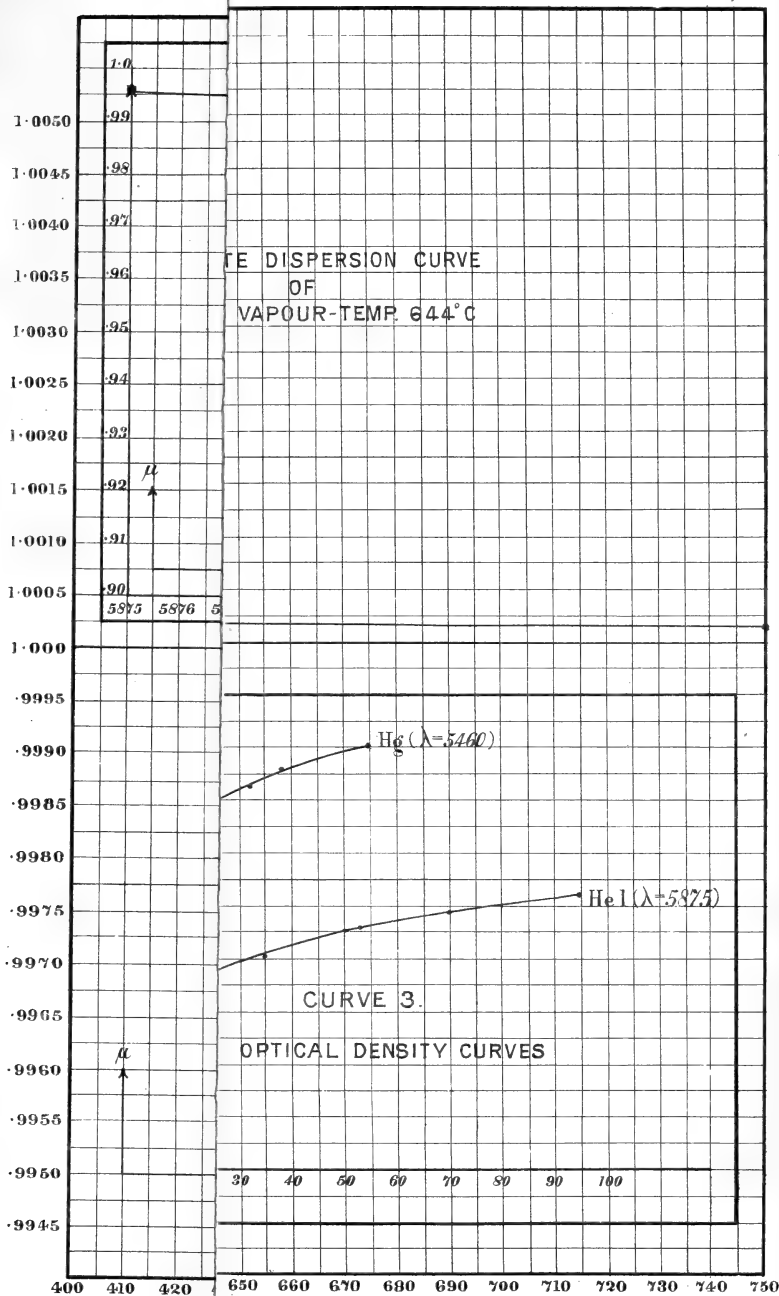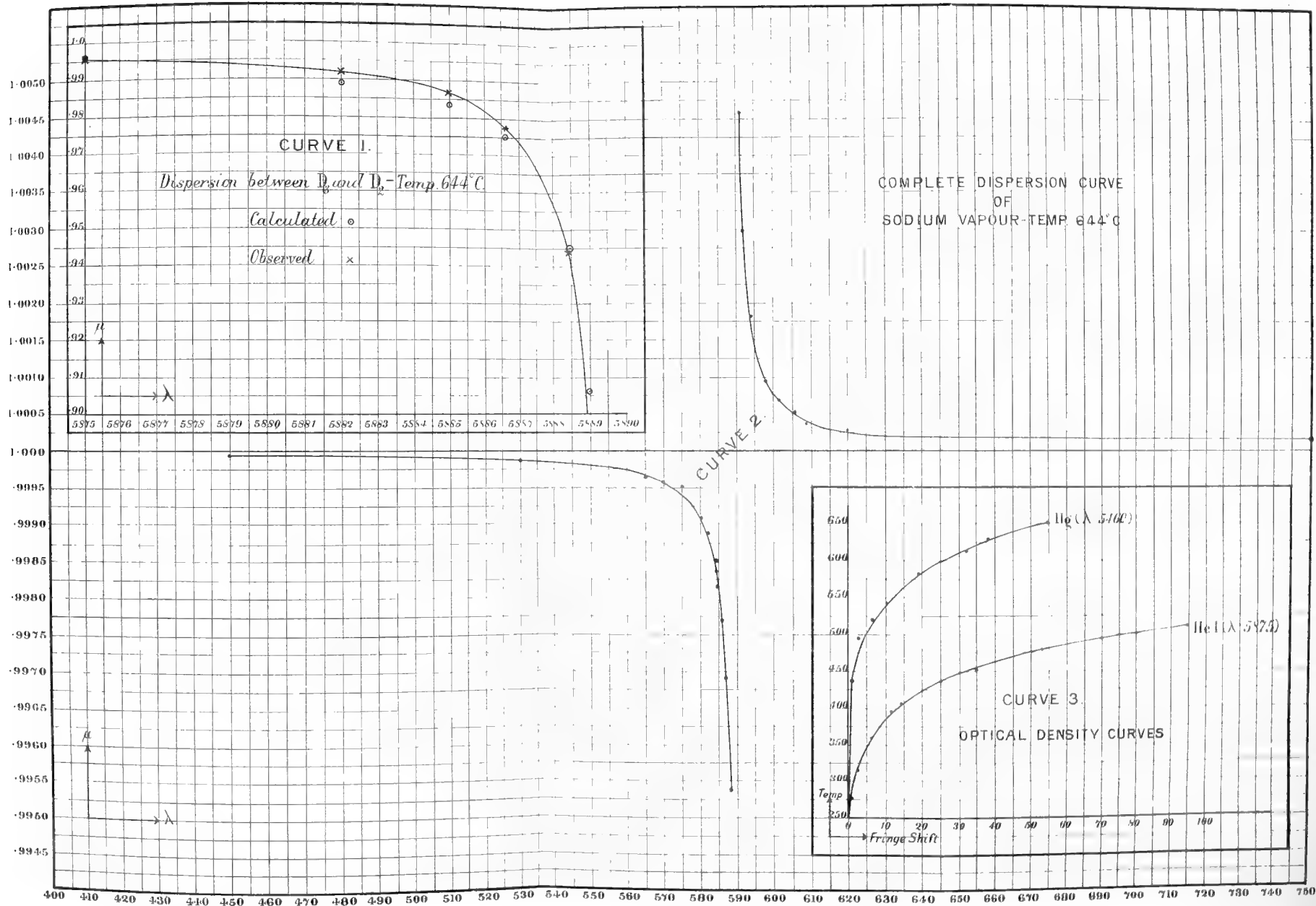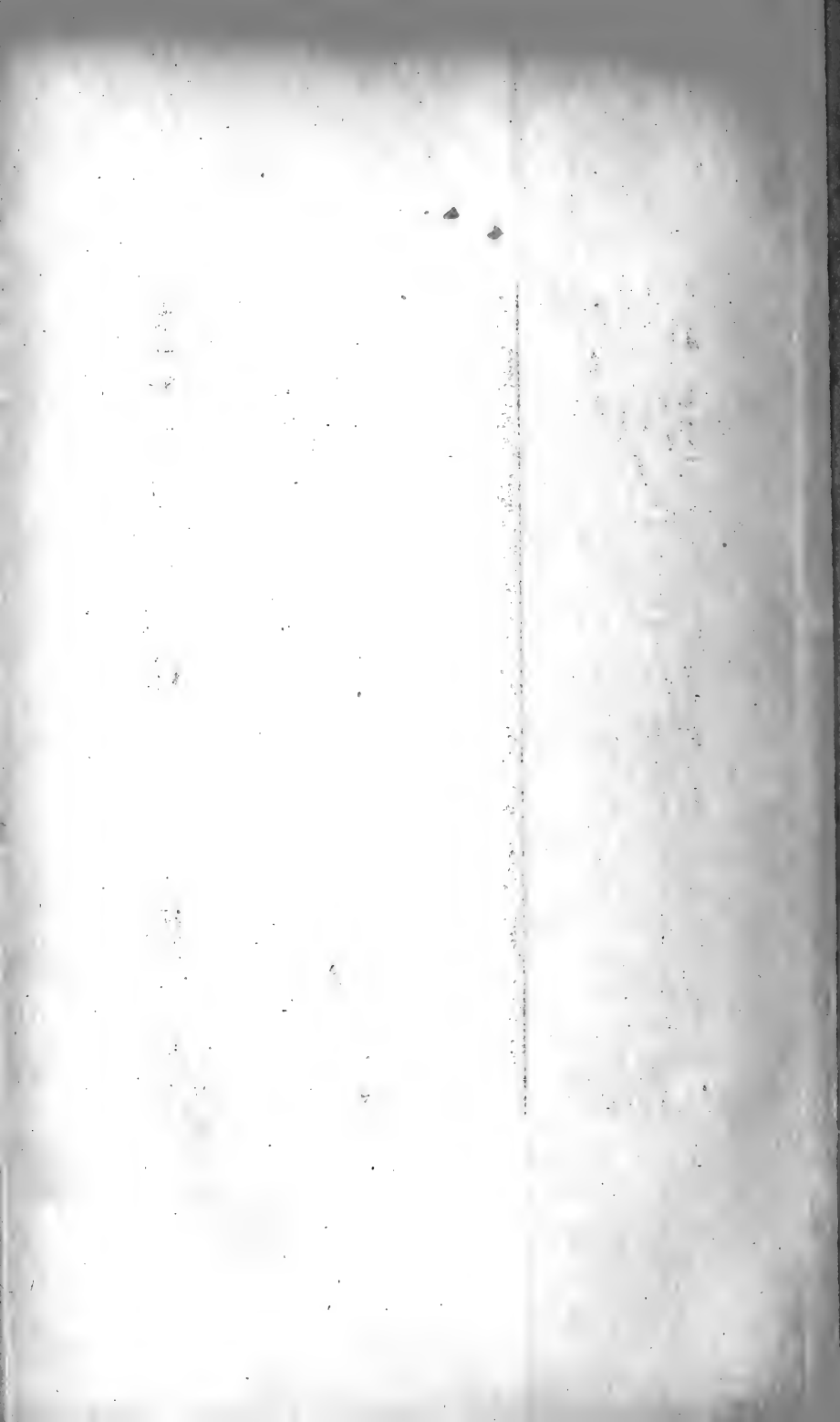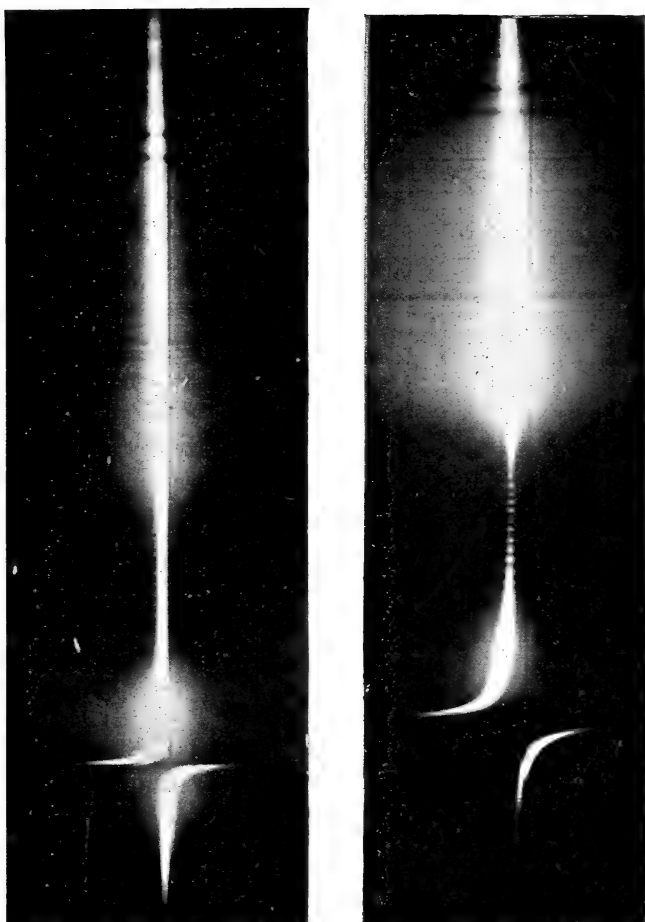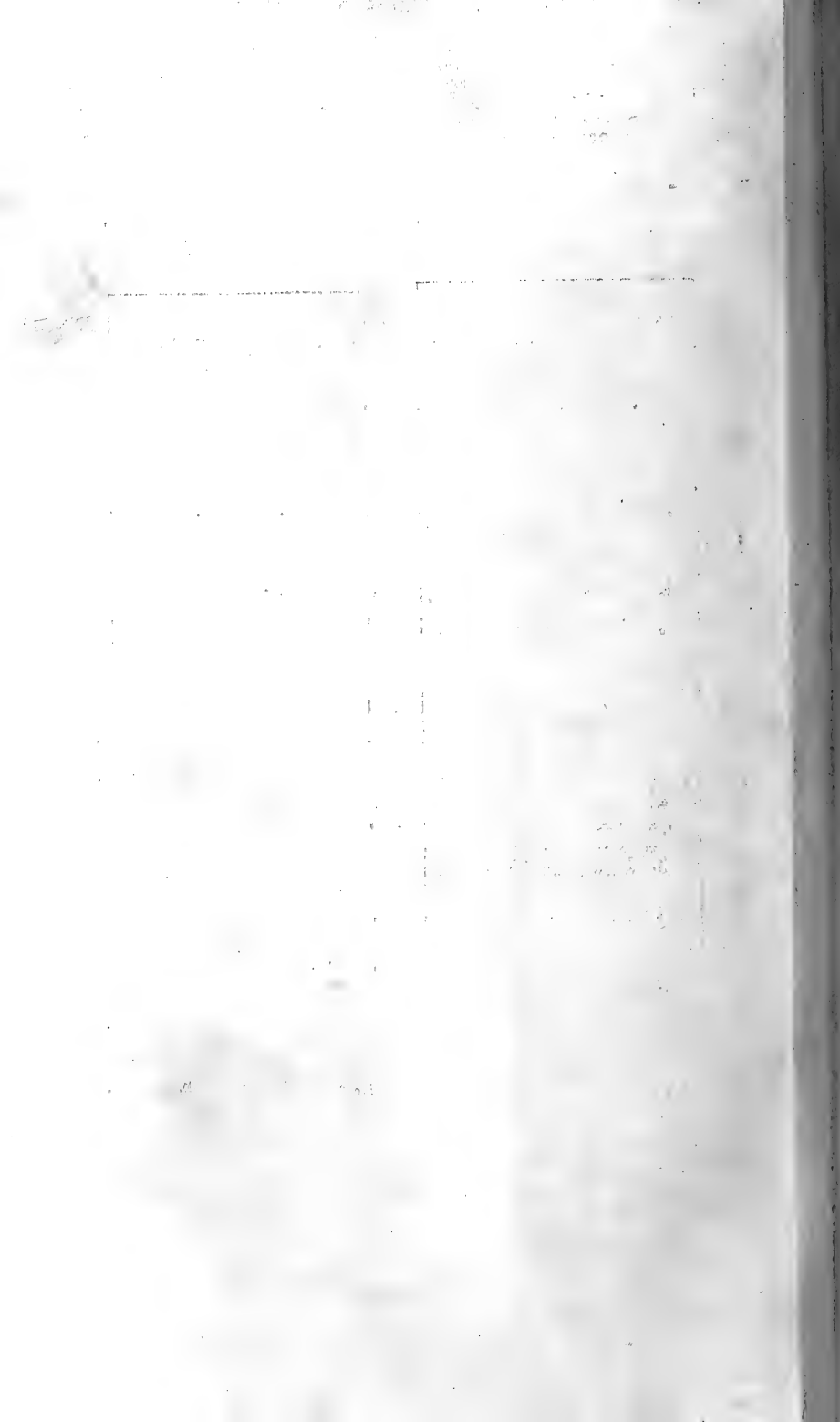
Arc

Galvanometer

Heating Coil

Helium Tube

Monochromatic
Illuminator

TE DISPERSION CURVE
OF
VAPOUR-TEMP. 644° C

CURVE 3.

OPTICAL DENSITY CURVES

Hg (λ=5460)

He 1 (λ=5875)

CURVE I.

Dispersion between D_1 *and* D_2 *—Temp. 644°C*

Calculated ○

Observed ×

COMPLETE DISPERSION CURVE
OF
SODIUM VAPOUR—TEMP. 644°C

CURVE 2

Hg (λ 5460)

He I (λ 5875)

CURVE 3.

OPTICAL DENSITY CURVES

Fringe Shift

H.K.

Fig. 8.

Blue. Green. Yellow. D. Orange. Red.

XLII. *The Propagation of Electric Waves along Spiral Wires, and on an Appliance for Measuring the Length of Waves used in Wireless Telegraphy.* By J. A. FLEMING, *M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London* [*].

[Plate XIII.]

THE propagation of electric waves along wires having helical form has engaged the attention of several physicists, but there are points of interest in connexion with it not entirely exhausted. Hertz has described an experiment in which he established stationary electric waves on a spiral wire, and compared the distance of the nodes with the corresponding distances when the wire was stretched out straight [†]. He found the internodal distances for the same frequency much smaller in the former case, and he suggests that Maxwell's theory is unable to account for this diminished velocity along the spiral conductor, as compared with that along a straight one.

G. Seibt devised apparatus for showing experimentally the production of stationary electric waves on helices of insulated wire, but his theory of the apparatus was not tested by quantitative experiments or measurements [‡].

H. C. Pocklington has treated the matter theoretically, and shows that the velocity of the wave along the axis of the spiral should be under some conditions less than that along a straight wire *.

As the production of stationary waves on spiral wires has found practical application in connexion with Hertzian Wave Wireless Telegraphy, particularly in apparatus for syntonization, it seemed desirable to investigate a little more closely the actual facts as a basis for further analysis. The following form of apparatus was therefore arranged, which is very suitable for the metrical study of stationary electric waves.

On a long circular-sectioned wooden rod was uniformly wound a close helix of silk-covered copper wire. The bare wire was No. 32 s.w.g. in size, and the covered wire was 0·4 mm. in diameter. It was laid on in one single layer of 5000 turns. The length of the helix was 200 cms., and had therefore 25 turns of wire per centimetre. The outside diameter of the helix was 4·1 cms., and the diameter of the circular axis of each single turn was 4·096 cms.

The electrical resistance of the wire at 15° C. was 153·2 ohms. This helix was supported horizontally in a wooden frame at a height of 45 centimetres above the table, and parallel with it was stretched a bare platinoid wire 0·2 mm. in diameter, which could be connected to the earth or removed. The distance between the helix and this bare wire was ascertained by a scale placed at each end of the spiral. As this bare wire was connected to the earth, it will be called the " earth-wire."

One end of the helical wire was attached to an electric circuit consisting of a condenser or a pair of leyden-jars, a thick bare copper wire wound in a spiral on a boxwood cylinder, having a sliding contact, thus forming a variable inductance ; and also a spark-gap included in a closed iron box. The spark-balls were connected to an induction-coil capable of giving a 10-inch spark. The electrical arrangements were as shown in fig. 1 (Pl. XIII.). The connecting wires joining the condenser, spark-gap, and variable inductance were formed of thick, insulated, stranded 19/16 copper cables. Parallel to the long helix, and at a distance of 15 cms. above it, was arranged a boxwood scale divided into centimetres.

When the induction-coil was set in action, electrical oscillations were generated in the leyden-jar circuit, and by a suitable adjustment of the variable inductance in

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* " Electric Oscillations in Wires," H. C. Pocklington, Proc. Camb. Phil. Soc. vol. ix. p. 324, Oct. 25, 1897.

connexion with the condenser, stationary electric waves were established on the long helical conductor. The position of the nodes and loops of electric force can be most easily ascertained by the employment of a vacuum-tube filled with rarefied Neon. The form of the tube found most useful was similar to that used in spectrum analysis, viz., a couple of bulbs connected by a straight narrow tube, but without platinum electrodes. Such a tube, when held near the helix when the coil was in action, glows brightly with an orange light. If the tube is held with its axis perpendicular to the axis of the helix at a little distance from it, and then moved along parallel to it, it glows most brilliantly when opposite to the antinodes, but more faintly or not at all when opposite to the nodes. It was found that by exploring the field round the helix in this manner, the position of the nodes could be fixed within a few millimetres *.

These experiments are preferably conducted in a partly darkened room. They can, however, be shown to a large audience in broad daylight provided suitable Neon vacuum-tubes are used. It is essential to enclose the active spark-gap in the condenser circuit in a metal box, not only to reduce the noise and obscure the light of the spark but also to prevent, as far as possible, the radiation of stray waves directly from the spark-balls. The induction-coil should not be too near, and should be screened by an interposed metal sheet.

The experiments here described were commenced by a series of careful measurements of the inductances and capacities of the various portions of the apparatus. The author has perfected of late years methods for measuring very small electrical capacities and inductances †.

These methods permit a capacity as small as 30 or 40 electrostatic units to be measured with very fair accuracy, and also an inductance of a few microhenrys.

The condenser first employed in these experiments consisted of two leyden-jars of which the capacity had been measured first separately, then both in parallel, and lastly, both in series ; these capacities being taken at a frequency of 100.

* Sir William Ramsay was so kind as to lend me a series of vacuum-tubes filled with argon, helium, neon, and other rare gases. Of these the neon tube gave the best results, and I am indebted to him for filling a couple of tubes for me with neon. These tubes glow with a bright orange light in the high-frequency electric field and define the nodes well.

† See J. A. Fleming and W. C. Clinton, "On the Measurement of Small Capacities and Inductances," Phil. Mag. 1903, vol. v., 6th series, p. 493. Also J. A. Fleming, "A Note on the Measurement of Small Capacities and Inductances," Phil. Mag. May 1904.

The means of the closely concordant results, obtained by three independent observers, were as follows :—

Jar No. 1	0·001531 microfarad.
Jar No. 2	0·001256 ,,
Both jars in parallel	0·002783 ,,
Both jars in series	0·000712 ,,

The variable inductance-coil was calibrated in microhenrys*. The inductance of the thick connecting wires joining up the jars, variable inductance, and spark-gap was also measured, and as first used was found to be 2600 centimetres. Subsequently, however, these connexions were shortened so as to make the inductance of the connexion almost negligible.

The next step was to measure the inductance and capacity of the long helix in the position in which it was to be used with the earth-wire at various distances. The inductance was measured by the modified form of Anderson's Bridge method, employing a telephone as a detector as described by the author. (See *loc. cit.*, Phil. Mag. May 1904.)

The following nine values were obtained for the inductance of this helix on different occasions :—

Table I.

Inductance measurements of long Helix of 5000 turns, 200 centims. in length and 4·096 centims. in mean diameter.

20.3×10^6 centimetres.		19.2×10^6 centimetres.	
19·2 ,,	,,	20·6 ,,	,,
19·3 ,,	,,	19·3 ,,	,,
20·1 ,,	,,	20·8 ,,	,,
20·3 ,,	,,	Mean of the above nine determinations $= 19.9 \times 10^6$ centims.	

The capacity of the Helix with respect to the earth was then taken by means of the Fleming and Clinton Commutator †.

The earth-wire was arranged parallel to the helix and at

* For a description of this inductance-coil see J. A Fleming, " A Note on the Measurement of Small Inductances and Capacities "; and " On a Standard of Small Inductance," Phil. Mag. May 1904. It consisted of a coil of No. 14 S.W.G. copper wire wound in a groove in a boxwood cylinder. The diameter of this cylinder was 10 cms., its length 45 cms., and it had 2·4 turns of wire per centimetre of length. The total inductance of the coil was 227,000 cms.

† For a description of this instrument and mode of using it see Phil. Mag. May 1903. J. A. Fleming & W. C. Clinton, " On the Measurement of Small Capacities and Inductances."

distances from the lower surface of 1, 2, 3, &c. centimetres successively, and the capacity between the helix and the earth-wire measured in micro-microfarads. (1 micro-microfarad $= 10^{-6}$ of a microfarad.)

In Table II. are given the results of the measurements.

Table II.

Capacity of the long Helix of 5000 turns, 200 centims. in length, with respect to the earth-wire placed parallel to it, and at different distances from it.

Distance of earth-wire below the helix.	Capacity of helix with respect to the earth-wire or the earth.
1 centimetre.	62 micro-microfarads.
2 centimetres.	51·2 ,,
3 ,,	48 ,,
4 ,,	45·8 ,,
5 ,,	44·4 ,,
6 ,,	43·2 ,,
7 ,,	42·2 ,,
8 ,,	41·2 ,,
9 ,,	40·4 ,,
10 ,,	39·8 ,,
Earth-wire removed.	36 ,,

The above values are not the actual figures of observation, but are taken from a curve (see fig. 2, Pl. XIII.) drawn through the plotted observed values to do justice to them all.

It was considered important to ascertain how far the closely wound helix had a capacity with respect to the earth different from that of a hollow metal cylinder of the same length and external diameter. Accordingly the helix was closely covered over with tinfoil and the observations repeated, taking the capacity of this tinfoil cylinder with respect to the earth-wire. These last observations are set out in the upper curve in fig. 2 (Pl. XIII.), and they show that at all distances of the earth-wire there is a nearly constant difference ; the circumscribing tinfoil cylinder being about 6 per cent. greater than the helix in capacity.

Since the tinfoil cylinder was of slightly larger diameter than the coil of wire, this shows that the error made in considering the helix as equivalent to a continuous metal cylinder in regard to electrical capacity is not large. Finally, the helix of wire was set upright and its capacity taken with respect to the earth with no earth-wire near it. This was found to be 35 micro-microfarads, or 0·000036 microfarad.

We have then to consider how the velocity of an electric wave along such a long helix can be determined. The point of view here adopted is to consider the closely wound helix as if it were a linear cylindrical conductor having a certain mean capacity and inductance per unit of length; these values being obtained from the quotient of the measured values of the total capacity and inductance, by the known length of the spiral. We then proceed to treat the problem as one of electric-wave propagation along a linear conductor in the usual manner.

Let C be the capacity, L the inductance, R the resistance, and K the dielectric conductance, all per unit of length of an infinitely long helix considered as a simple linear conductor immersed in a dielectric. Then, considering any element δx of the length of the helix, if we call v the potential and i the current at the beginning of this element, at any instant we have the well-known equations:

$$L \frac{di}{dt} + Ri = \frac{dv}{dx}, \quad \ldots \quad \ldots \quad (1)$$

$$C \frac{dv}{dt} + Kv = \frac{di}{dx}, \quad \ldots \quad \ldots \quad (2)$$

expressing the current i and potential v at any point in terms of the constants. If we consider i and v to vary in a simple harmonic manner with frequency $n = 2\pi/p$ and to have maximum values I and V (an assumption legitimate in the case of electrical oscillations), then the above equations can be written

$$\frac{dV}{dx} = (R + jpL)I, \quad \ldots \quad \ldots \quad (3)$$

$$\frac{dI}{dx} = (K + jpC)V; \quad \ldots \quad \ldots \quad (4)$$

where $j = \sqrt{-1}$. Hence writing P for

$$\sqrt{R + jpL} \cdot \sqrt{K + jpC}$$

we can put (3) and (4) in the form

$$\frac{d^2V}{dx^2} = P^2V, \quad \ldots \quad \ldots \quad (5)$$

$$\frac{d^2I}{dx^2} = P^2I. \quad \ldots \quad \ldots \quad (6)$$

A solution of (5) applicable in the present case is

$$V = a\epsilon^{+Px} + b\epsilon^{-Px}, \quad \ldots \quad \ldots \quad (7)$$

where a and b are constants.

If we apply this to the case of a semi-infinite cable having a simple harmonic electromotive force of maximum value E acting at one end and reckon x in the direction in which V *decreases*, then, since in this instance $V=0$ when $x=\infty$, we must have $b=0$, and therefore

$$V = E\epsilon^{-Px} ; \quad . \quad . \quad . \quad . \quad . \quad (8)$$

or, since P can be written in the form $\alpha+j\beta$, we have the following vector equation for the potential at x:

$$V = E\epsilon^{-\alpha x}(\cos \beta x - j \sin \beta x). \quad . \quad . \quad (9)$$

Hence, it follows that the wave-length of the potential distribution along the conductor is $2\pi/\beta$ and the wave velocity is p/β *.

In the above equations x is to be understood as a distance measured along the axis of the helix, not along the spiral path of the wire.

If we may consider, as we may in this case, since p is very large, that R and K are negligible in comparison with pL and pC, then P^2 reduces to $-p^2CL$; and it follows at once from (9) that the wave-length λ of the motion expressed by these equations under these conditions is given by

$$\lambda = 1/n \sqrt{CL}.$$

Hence the wave velocity $W=1/\sqrt{CL}$.

We can therefore calculate the wave velocity along the spiral if we know by experiment the capacity per unit of length (C) and the inductance per unit of length (L) of the helix.

The length of the helix is 200 centimetres and its total inductance $19\cdot9 \times 10^6$ cms., and its total capacity is C_0 micro-microfarads (m.mfds.) depending on the distance of the parallel earth-wire. Hence, the wave velocity along the helix is given by the equation

$$W = \frac{200 \times \sqrt{10^{21}}}{\sqrt{\text{Capacity in m.mfds.} \times \text{Inductance in cms.}}} . \quad (10)$$

* If we multiply this equation (9) by ϵ^{jpt} and take the real part of the resulting expression, we have $v=E\epsilon^{-\alpha x}\cos(pt-\beta x)$, which gives us the ordinary algebraic expression for the potential at any point and time in the cable. See J. A. Fleming, "On a Model illustrating the Propagation of a Periodic Current in a Telephone Cable, and the Simple Theory of its Operation," Phil. Mag. August 1904.

The conversion of micro-microfarads to electromagnetic units of capacity involves the factor 10^{21}.

Hence the above may be written

$$W = \frac{20000 \times 10^6}{\sqrt{199\ C_0}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Below is a Table showing the calculated values of the wave velocity W along the spiral for various observed values of the total capacity C_0 (in m.mfds.) for different distances of the earth-wire.

<div align="center">TABLE III.</div>

Electric-wave velocity along a long Helix of 5000 turns, 4·1 cms. diameter, and 200 cms. in length, calculated from the measured inductance and capacity of the helix.

Distance of earth-wire below helix. d.	Capacity of helix in micro-microfarads. C_0.	Wave-velocity along helix in centims. per sec. W.
1 centim.	62·0 m.mfds.	180×10^6
2 centims.	51·2 ,,	198 ,,
3 ,,	48·0 ,,	204 ,,
4 ,,	45·8 ,,	209 ,,
5 ,,	44·4 ,,	212 ,,
6 ,,	43·2 ,,	215 ,,
7 ,,	42·2 ,,	218 ,,
8 ,,	41·2 ,,	221 ,,
9 ,,	40·4 ,,	223 ,,
10 ,,	39·8 ,,	225 ,,
Earth-wire removed.	36·0 ,,	235 ,,

Hence the wave-velocity along the helix when isolated in space approximates to 235×10^6 centimetres per second or $\frac{1}{125}$ part of the velocity of æther waves in free space or along a straight wire.

Since the division of the total inductance and capacity by the length of the helix gives us only the mean inductance and capacity per unit of length, the above values of the wave-velocity are also mean values.

The author has shown that the inductance per unit of length of a spiral is numerically equal to the square of the length of the wire wound on per unit length of the helix, and also that the capacity of a helix per unit of length

diminishes as the turns per centimetre diminish. Hence, if we suppose such a helix gradually pulled out into a straight wire whilst being traversed by electric waves of constant frequency, the wave-velocity would gradually increase up to the velocity of radiation, viz., to 3×10^{10} cms./sec., which value it would reach when the helix was stretched out into a straight wire. In the case, however, of a closely-wound helix, the velocity of electric waves along it in the direction of the axis is much less than the velocity of free waves in space.

The simple analysis above given is inadequate to deal with the more difficult problem of a very open spiral. If a close-wound spiral were gradually stretched out, the velocity of the wave along it would increase, and at some pitch would reach that of radiation. The precise pitch of the spiral at which this would happen is of mathematical interest, but no doubt difficult to predetermine.

The next step was to measure the wave-lengths for various frequencies.

The inductance in the jar circuit was first adjusted to produce the fundamental oscillation of the long helix and establish a stationary wave on the spiral with a loop or antinode of potential at the free end ; and then subsequently adjusted to produce the series of odd harmonics with wave-lengths in the ratio of 1/3rd, 1/5th, 1/7th, 1/9th, 1/11th of that of the fundamental. The position of the nodes was then carefully ascertained by the aid of the Neon vacuum-tube, and found to be as shown in the diagram on fig. 3 (Pl. XIII.), where O is the open end of the helix and E the end at which the electromotive force is applied, and the numbers represent the distances in centimetres separating the nodes N_1, N_2, &c. from each other and from the free ends of the helix.

These observations have been very carefully repeated on many occasions, and the figures given are the means of the best results.

Coincidently with the measurements of the internodal distances the inductance inserted in the leyden-jar circuit was noted, and also the distance of the earth-wire below the helix.

Observations were made with the earth-wire at various distances in the case of the fundamental and 1st harmonic oscillations ; and the results are given in Tables IV., V., and VI. The frequency n of the oscillations in the jar circuit is calculated from the values of the inductance and the capacity

in that circuit by the formula *

$$n = \frac{5 \times 10^6}{\sqrt{\text{Capacity in microfarads} \times \text{Inductance in centims.}}}. \quad (12)$$

TABLE IV.—Measured Inductances and Calculated Frequencies in the Leyden-jar Circuit corresponding to the Fundamental Electrical Oscillation of the Spiral and to different distances of the Earth-wire.

Capacity of the leyden-jars $= 0.0028$ microfarad.

Distance of earth-wire from helix in centims.	Inductance of jar circuit in centims. L.	Calculated frequency of the oscillation. n.
2	193,600	0.215×10^6
3	157,600	0.238 ,,
4	150,600	0.243 ,,
5	141,600	0.251 ,,
6	132,600	0.260 ,,
7	123,600	0.269 ,,
8	111,600	0.283 ,,
8·5	107,600	0.288 ,,
Earth-wire removed.	102,600	0.295 ,,

TABLE V.—Measured Inductances and Calculated Frequencies in the Leyden-jar Circuit corresponding to the First Harmonic Electrical Oscillation of the Spiral and to different distances of the Earth-wire.

Capacity of leyden-jars $= 0.00071$.

Distance of earth-wire from helix in centims.	Inductance of jar circuit in centims. L.	Calculated frequency of the oscillation. n.
2	76,100	0.680×10^6
3	68,600	0.717 ,,
4	67,100	0.725 ,,
5	62,600	0.750 ,,
6	59,600	0.769 ,,
7	56,600	0.789 ,,
8	54,600	0.802 ,,
8·5	52,600	0.818 ,,
Earth-wire removed.	50,600	0.834 ,,

* See J. A. Fleming, Cantor Lectures before the Society of Arts of London. " On Electrical Oscillations of Electric Waves," 1901. Lecture I.

The inductances required in the jar circuit to produce the first, second, fourth, and fifth harmonics were also determined for |certain distances of the earth-wire and also when the earth wire was removed, the position of the nodes being at the same time taken. The first node is that nearest to the open end of the spiral farthest removed from the end at which the electromotive force is applied. All theories of the production of stationary waves on wires agree in making the distance between the first and second nodes equal to one half of a wave-length. Hence these distances are given in Table VI., and for comparison the frequency of the fundamental for the same earth-wire distance.

TABLE VI.—Measured values of the Inductances and Capacities in the Condenser Circuit and Calculated Frequencies required to produce the Fundamental and the various Harmonics on the Helix.

Glass Condenser.

Oscillation.	Inductance in centims. L.	Capacity in mfds. C.	Frequency. n.	Wave-length. λ.	Velocity. $n\lambda$.
Earth-wire 2 cms. down.					
Fundamental......	193,600	0·0028	$·215 \times 10^6$	920	198×10^6
1st Harmonic......	69,100	0·00071	·714 ,,	276	197 ,,
2nd ,, ...	26,600	,,	1·151 ,,	160	184 ,,
3rd ,, ...	15,100	,,	1·533 ,,	115	176 ,,
4th ,, ...	10,600	,,	1·823 ,,	88	160 ,,
5th ,, ...	8,600	,,	2·023 ,,	72	146 ,,
Earth-wire 3 cms. down.					
Fundamental......	157,600	0·0028	$·238 \times 10^6$	857	204×10^6
1st Harmonic......	68,600	0·00071	·717 ,,	280	201 ,,
2nd ,, ...	26,600	,,	1·151 ,,	160	184 ,,
3rd ,, ...	15,100	,,	1·533 ,,	115	176 ,,
4th ,, ...	10,600	,,	1·823 ,,	88	160 ,,
5th ,, ...	8,600	,,	2·023 ,,	72	146 ,,
Earth-wire removed.					
Fundamental......	102,600	0·0028	$·295 \times 10^6$	800	236×10^6
1st Harmonic......	50,600	0·00071	·834 ,,	280	233 ,,
2nd ,, ...	21,600	,,	1·306 ,,	160	209 ,,
3rd ,, ...	13,600	,,	1·610 ,,	115	185 ,,
4th ,, ...	10,100	,,	1·87 ,,	88	164 ,,
5th ,, ...	7,100	,,	2·23 ,,	72	171 ,,

Concerning the figures in Table VI., the following remarks may be made :—

In the first place, the wave-lengths for the five harmonic oscillations, which are equal to double the measured distance between the first and second nodes, are very nearly in the ratio of the numbers 3, 5, 7, 9, and 11, and the mean of the products 3×280, 5×160, 7×116, 9×88, and 11×72 is 807, or very nearly four times the length of the helix.

On the other hand, the calculated frequencies (n) for the higher harmonics do not follow those of the fundamental and first harmonic in the ratio of the numbers 1, 3, 5, 7, 9, 11; and there is therefore a continual falling off in the product $(n\lambda)$ of the frequency in the condenser circuit and the wavelength on the spiral. In the case of the fundamental and first harmonic when the earth-wire was removed, this product agrees with the velocity of the wave along the spiral as calculated from the capacity and inductance per unit of length of the helix. The first suggestion which occurred on considering this discrepancy was that the dielectric constant, and therefore the capacity of the glass condenser, might perhaps decrease as the frequency increased. It has been shown by Prof. J. J. Thomson and by M. R. Blondlot, that at a frequency of 25×10^6 the dielectric constant of glass falls to a value of $2 \cdot 7$ or $2 \cdot 8$ *, and it seemed possible that this decadence might set in at a frequency as low as 10^6. It has, however, been found that sulphur and ebonite do not exhibit this decrease of dielectric constant under increased frequency of electric force. Accordingly two other condensers were constructed of sheets of good ebonite, 3 mm. in thickness. These sheets were partly covered on both sides with rectangles of tinfoil, each having an area of $15 \times 17 \cdot 5$ centimetres, and two condensers were made up, each consisting of five of such coated plates. These were immersed in vaseline oil contained in an ebonite box.

The capacity of these ebonite condensers was measured and found to be as follows :—The two ebonite condensers in parallel had a capacity of $0 \cdot 0025$ microfarad, and in series had a capacity of $0 \cdot 00062$ microfarad.

Another source of error was at the same time discovered and correction made for it. It was found that the inductance of the spiral wire wound on a boxwood cylinder which was used as a variable inductance in the condenser circuit had a

* See Prof. J. J. Thomson, Proc. Roy. Soc. vol. xlvi. p. 293 (1889), "On Specific Inductive Capacities of Dielectrics under rapidly alternating Electric Forces"; also M. R. Blondlot, *Comptes Rendus,* vol. cxii. p. 1058 (1891).

less effective inductance when used with the high frequency oscillations than when measured with alternating currents of low frequency. This was discovered to be due to the proximity of the turns of wire permitting a certain dielectric current to pass from turn to turn, or else a true conductor current to flow through the boxwood.

Investigation showed that when employing a spiral wire as an inductance, measurements of the inductance at low frequency could not be considered as giving the true value of the inductance for high frequency oscillations. Even when the wire was wound as an open spiral with no solid dielectric as a core, there was still an uncertainty as to the true inductance. This led to a long series of experiments on the construction of small inductances of a form for which the inductance could be predetermined by calculation for high frequency oscillations. It was discovered that no form is so convenient as that of a long wire, bent into the form of a square with the ends brought out parallel at one corner and of the shape shown in fig. 4 (see Pl. XIII.).

If S is the length of the side of the square in centimetres, and if d is the diameter of the wire of which it is made, then the inductance L of the square in centimetres is given by the equation

$$L = 8S \left\{ 2 \cdot 303 \log_{10} \frac{16\,S}{d} - 2 \cdot 6 \right\}.$$

If the tails of parallel wire by which the current is brought to the square have a length l, and are at a distance D centimetres apart, then these create an additional inductance L′, where

$$L' = 2l \left\{ 4 \cdot 606 \log_{10} \frac{2D}{d} + \frac{1}{2} \right\}.$$

Hence the total inductance is $L + L'$. Five squares of wire were accordingly constructed of copper wire 2 millimetres in diameter. The sides of these squares in centimetres and feet and inches, and calculated inductances, were as follows :—

Square.	Length of side.		Calculated Inductance.
No. 1	823·5 cms.	= 27 ft.	= 56,000 cms.
No. 2	427 „	= 14 ft.	= 26,747 „
No. 3	256 „	= 8 ft. 3 ins.	= 15,011 „
No. 4	168 „	= 5 ft. 7½ ins.	= 9,274 „
No. 5	132 „	= 4 ft. 4 ins.	= 7,470 „

The above squares were found by a process of trial and failure to have inductances which, when used in conjunction with the two ebonite condensers in series having a capacity of 0·00062 mfd., tuned with the five harmonic oscillations of the spiral.

By the aid of these observations the scale-readings of the inductance on the boxwood cylinder were corrected to give true values, and these last values have been employed in Tables IV., V., and VI. The whole of the observations were then repeated, using the ebonite condensers and these square wire inductances of predetermined value to form the oscillating circuit. The position of the nodes was carefully determined by the aid of two neon vacuum-tubes, and the results are tabulated in Table VII. The capacity C is the measured capacity in the condenser circuit. The inductance L is the calculated value of the square inductance on wire of 2·03 mm. diameter in the form of a square used. The frequency n is calculated from the values of C and L. The wave-length λ is twice the measured distance between the first and second nodes on the helix, and the wave-velocity W is the product $n\lambda$ of the calculated frequency and the observed wave-length. The wave-length of the fundamental (840 cms.) is obtained by dividing the velocity obtained from the 1st harmonic by the calculated frequency for the fundamental.

TABLE VII.—Inductances and Capacities and Calculated Frequencies in the Condenser Circuit required to produce the Fundamental and Harmonic Oscillations in the long Helix.

Ebonite condenser and square inductances used.

Oscillation.	Capacity. C.	Inductance. L.	Frequency. n.	Wave-length. λ.	Wave-velocity. $W=n\lambda$.
Fundamental......	0·0055	56,000	0·28 $\times 10^6$	840	235×10^6
1st Harmonic......	0·00062	56,000	0·847 ,,	280	235 ,,
2nd ,, ...	,,	26,747	1·23 ,,	166	204 ,,
3rd ,, ...	,,	15,011	1·64 ,,	115	189 ,,
4th ,, ...	,,	9,274	2·08 ,,	88	183 ,,
5th ,, ...	,,	7,470	2·32 ,,	72	167 ,,

It is seen, therefore, that the velocity obtained from the measurements made with the 1st harmonic, viz., 235×10^6 cms./sec., agrees exactly with that calculated from the inductance and capacity per unit of length of the helix. On

the other hand, the velocity obtained from the higher harmonics is decidedly less. As there can be very little doubt but that the values given in Table VII. for the inductances and capacities for the condenser circuit corresponding to each harmonic are substantially correct, the only conclusion which can be drawn is that the wave-velocity along the helix is in some way or other diminished as the frequency increases.

After numerous experiments and measurements designed to eliminate other possible causes for this diminution, it was found that ordinary dry wood has decided conductivity for high-frequency currents of the order of 10^6. This can be shown in the following manner. If a stationary oscillation is excited in the spiral and a vacuum-tube placed near to, but not at a node, it glows feebly. If then a thick sheet of ebonite is interposed it does not affect the glow. If, however, a wood board is interposed, the glow ceases. The wood has conductivity enough to act as a screen for high-frequency oscillations; accordingly it appears that the wooden rod on which the silk-covered wire forming the helix is wound must have conductivity sufficient at or above a frequency of 10^6 to act as an earth-wire.

On referring to Table III. it will be seen that as the earth-wire was brought nearer to the helix, the velocity of the wave along it decreased because the capacity of the helix was increased.

When the earth-wire was removed, the velocity was 235×10^6 cms./sec. as calculated from the capacity and inductance. When the earth-wire was brought within 1 cm. distance of the helix, the velocity fell to 180×10^6 cms./sec. Hence it is not inconsistent to conclude that for frequencies above 10^6 the capacity of the helix is increased by the growing conductivity of the wooden rod within it, and the wave-velocity gradually falls from 235×10^6 down to 180×10^6 or 170×10^6 as the frequency is increased to obtain the various harmonics.

To confirm the above results, experiments were also made with oscillations set up in the helix when the end remote from the condenser circuit was earthed. In this case the harmonics resemble those established in an open organ-pipe, and their frequency increases in accordance with the natural numerals, whereas with the far end of the helix insulated they follow the law of the odd numbers.

A square inductance was made consisting of one turn of No. 14 copper wire bent into a square, of which the side was 6 feet $11\frac{1}{2}$ inches in length, or 212 centimetres. The perimeter was 846 centimetres and the calculated inductance 12,077

centimetres. This inductance was associated with an ebonite condenser of 0·0015 microfarad capacity, and it was found that when the long helix was connected to this oscillating circuit with the far end earthed, that stationary oscillations were set up in it which had a node at the centre of the helix. This was therefore the first overtone. From the capacity 0·0015 mfd. and inductance 12,077 cms., we find the frequency in the oscillating circuit was $1·174 \times 10^6$. Since the wave-length is equal to the whole length of the helix, viz., 200 centimetres, it follows that the wave-velocity should be

$$200 \times 1·174 \times 10^6 = 234·8 \times 10^6 \text{ cms./sec.}$$

This, however, is almost exactly the wave-velocity calculated from the constants of the helix.

Again, the capacity was increased to 0·006 microfarad, and it was then found that the stationary oscillation set up on the helix was the fundamental having an antinode at the centre.

This capacity 0·006 mfd. and inductance 12,077 centims. corresponds to a frequency of $0·587 \times 10^6$, and the wave-length is 400 centims. Hence, here also the wave-velocity is $400 \times 0·587 \times 10^6 = 234·8 \times 10^6$ cms./sec.

On taking a higher frequency still, it was found that an inductance of 22,000 cms. and a capacity of 0·00062 mfd. in the condenser circuit excited on the helix a wave of length 150 centimetres. This capacity and inductance correspond to a frequency of $1·37 \times 10^6$, and this again to a wave-length of 150 cms. Hence the wave-velocity is 205×10^6 cms./sec. Accordingly, here again we find that with increased frequency the velocity of the wave along the helix with a wooden core decreases.

To eliminate all doubt as to the effect of the conductivity of the wooden rod, a pair of new helices of wire were constructed, each consisting of a thick ebonite tube wound over with one layer of double silk-covered copper wire.

These helices will be called Helix A and Helix B.

The length of each tube was 215 cms., and outside diameter 4·75 cms. The wire was No. 30 s.w.g., and after being wound on in closely adjacent turns was lightly varnished with shellac varnish made up with absolute alcohol. The lengths, diameters, and turns of each helix were as follows :—

> Helix A. Number of turns = 5465. Length of helix = 210 centimetres. Mean diameter of one turn = 4·78 centims.

> Helix B. Number of turns = 5476. Length of helix = 210 centims. Mean diameter of one turn = 4·76 centims.

From the above values the inductance L of each helix was calculated from the formula

$$L = \pi^2 d^2 \left(\frac{N}{l}\right)^2 l, \quad \text{where } \pi^2 = 9\cdot87.$$

Hence   Inductance of helix A $= 32\cdot07 \times 10^6$ cms.,
and   Inductance of helix B $= 31\cdot93 \times 10^6$ cms.

The capacity of each helix with respect to the earth was measured with the helix supported horizontally and at a height of 50 cms. above the table.
The values were as follows :—

Capacity of helix A $= 0\cdot000045$ microfarad.
Capacity of helix B $= 0\cdot0000462$ microfarad.

From these figures the velocity of propagation of the wave along each helix was calculated as above described and found to be :—

Velocity along helix A $= 174\cdot8 \times 10^6$ cms./sec.
Velocity along helix B $= 172\cdot9 \times 10^6$ cms./sec.

Hence it may be taken as 174 million centimetres per second.

The above-described spirals were supported on insulating stands in a horizontal position, and one end was connected to an oscillating circuit consisting of a condenser made of sheet ebonite covered with tinfoil, an inductance-coil, and a spark-gap. The condenser consisted of 24 ebonite plates immersed in vaseline oil, the plates being grouped in six sections of four plates each. The capacity of the sections s me asured, and found to be as follows :—

Section 1 .... $0\cdot001030$ microfarad.
Section 2 .... $0\cdot001005$,,
Section 3 .... $0\cdot000913$,,
Section 4 .... $0\cdot000919$,,
Section 5 .... $0\cdot000994$,,
Section 6 .... $0\cdot000974$,,
Total capacity ...... $0\cdot005835$,,
Sections $4 + 5 + 6$... $0\cdot002887$,,
Sections $1 + 2 + 3$... $0\cdot002948$,,

Sections $1 + 2 + 3$ in series with sections $4 + 5 + 6$ $= 0\cdot001461$ mfd.

The variable inductance used with the condenser had been previously calibrated by comparing certain parts of it with the calculated inductance of squares of wire of known size.

The inductance and capacity were then arranged so as to

produce the frequency of oscillation required to set up the fundamental or higher harmonic oscillations in one of the helices, and the wave-lengths measured as already described with the Neon vacuum-tube.   The results of these observations are set out in the following Table VIII.

TABLE VIII.—Observations made with Helix A with Ebonite Core and with Ebonite Condenser, variable Inductance, and Neon Vacuum-tube, to determine the Wave-velocity along the Helix.

Oscillation.	Capacity in mfds. in condenser circuit. C.	Inductance in cms. in condenser circuit. L.	Calculated frequency *n*.	Observed wave-length. λ.	Calculated wave-velocity. $W = n\lambda$.
Fundamental......	0·005835	110,000	0·197 × 10⁶	(871)	(172 × 10⁶)
1st Harmonic......	0·002887	25,000	0·588 ,,	292	172 ,,
2nd ,, ...	0·001461	18,000	0·977 ,,	175	172 ,,
3rd ,, ...	0·001464	9,000	1·379 ,,	124	171 ,,
4th ,, ...	0·001461	6,000	1·70 ,,	95	163 ,,
5th ,, ...	0·001461	5,000	1·9 ,,	80	152 ,,

From the above observed values it is seen that the wave-lengths of the 1st, 2nd, and 3rd harmonics are very nearly in the ratio of 3 : 5 : 7 for $292 \times 3 = 876$, $175 \times 5 = 875$, and $124 \times 7 = 868$.   Hence the fundamental wave-length should be 871.   If we insert the value in Table VIII. and calculate the wave-velocity for the fundamental frequency, we find it to be 172×10^6 cms. per second, or exactly the same value as that obtained for the three succeeding harmonics.

Hence from the fundamental and the three succeeding higher harmonics, we obtain values for the wave-velocity which are closely identical and equal to 172×10^6 cms./sec., and in very near agreement with the velocity of 174×10^6 cms./sec. calculated from the constants of the helix itself.

There is a slight falling off in the velocity obtained from the 4th and 5th harmonics, but the difficulty of measuring the small inductance then used in the condenser circuit is considerable.

Hence we may say that when a helix wound on an ebonite core is used, observations confirm theory substantially, and show us that the stationary waves are formed as predicted, the wave-velocity being calculable from the observed inductance and capacity of the helix.

The conclusions which may be drawn from this research are as follows :—

(i.) In the construction of inductances and helices for such experiments as are here described, they should be wound on ebonite and not on wooden cores.

(ii.) Spirals of wire, if the turns are at all close, have less inductance for frequencies of the order of a million than for frequencies of the order of 100, on account of the dielectric current passing between the turns.

(iii.) The best form to give to a standard of inductance for high frequency work is a single wire bent into a circle or a square.

(iv.) The velocity of an electric wave along a closely wound helix of wire is measured by the reciprocal of the square root of the product of its capacity and inductance per unit of length.

(v.) The distance from the open end of the helix to the first node is always much less than a quarter of a wave-length, in fact, nearly one-fifth of a wave-length.

(vi.) The distance between the first and second nodes reckoned from the free or insulated end of the helix is one-half of a wave-length.

(vii.) If the fundamental oscillation is set up in a helix by attaching one end to an oscillating circuit tuned to it, the wave-length is rather more than four times the length of the helix, but not five times.

The above described apparatus can be used with some modification for the determination of the wave-lengths of the æther waves employed in Hertzian wave wireless telegraphy.

The frequencies used for the above purpose lie generally between the limits of 0.5×10^6 and 3×10^6.

The following is the arrangement required (see fig. 5, Pl. XIII.). In all the transmitters now used for Hertzian wave-telegraphy there is an oscillating circuit consisting of an inductance, a capacity, and a spark-gap. This is directly or inductively coupled to an aerial circuit A. One of the spark-balls S should be earthed, and a point at the opposite end of the inductance T_1 should be connected to an insulated metal plate P_2, which may be conveniently a zinc plate, 30 cms. or 12 inches square. A long helix H is prepared which may be 2 or $2\frac{1}{2}$ metres in length, consisting of fine insulated wire wound in one layer on ebonite rods or tubes about 4 or 5 cms. in diameter. This helix is supported in a horizontal position and well insulated, and one end of the helix is connected to an insulated metal plate P_1 of the same size

2 H 2

as the one P_2 on the oscillating circuit, and these plates are placed parallel to each other at a distance of a few centimetres more or less.   The helix is provided with a curved metal saddle Sl which slides on it, being made to fit closely by a few layers of interposed tinfoil, and this saddle is connected by a flexible wire with the earth.   Three vacuum-tubes V should be provided, which may be conveniently attached to a sort of lazy-tongs arrangement whereby they can be spaced out at equal distances.   The process of measuring the frequency of the oscillating circuit consists in sliding the earthed saddle along the helix until a position is found in which there is a node of potential N halfway between the saddle and the end plate, and an antinode An at the middle of each half of the section of the helix on either side of the node.   When this position is found, the length of the helix interposed between the end plate P_1 and the saddle Sl is equal to one wave-length on the helix.   If the velocity of electric waves along this helix has previously been determined as described in this paper, the quotient of this measured velocity by the observed length of the wave on the helix gives the frequency of the oscillation.   Having obtained the frequency of the oscillating circuit, we divide this frequency reckoned in fractions of a second into 1000 millions, a number representing very nearly the velocity of Hertzian waves in free space in feet per second, and the quotient is the length of the aerial æther wave in feet.

The velocity of free æther waves in space is sufficiently near to 10^9 feet per second to make the above rule quite accurate enough for practical purposes.

On comparing this wave-length with the length of the aerial we can decide at once whether the wave being sent out is the fundamental or a higher harmonic of the radiating antenna.

In another place the writer has suggested the term *kumascope* (from κυμα, a wave) as the name for appliances of all kinds used for detecting electric waves.   He would now suggest the term *kummeter* (analogous to ammeter) as the name for the above described instrument for measuring the length of electric waves.   An instrument of this kind is of great practical use in connexion with Hertzian wave-telegraphy, as it enables us to compare the wave-length of the radiation used with its telegraphic effectiveness.   Thus a wave 300 feet in length travels well over sea-surface, but it will not go well across towns and land, whereas a wave of five times that length travels well across land districts thickly covered with houses or trees or irregular elevations.   As an example of such a measurement the following figures may be given.   Employing the above described ebonite-core helix on

which the wave-velocity was 172×10^6 cms./sec. with a certain wireless transmitter, the wave-length on the helix was found to be 65 cms. Hence the frequency of the oscillations used was $172 \times 10^6/65 = 2.65 \times 10^6$, and the corresponding length of the "wireless wave" is 377 feet. Since the capacity used in the spark-circuit was 0·006 mfd., this measurement also tells us that the effective inductance of the oscillation transformer employed with it was 600 centimetres. By noting the increase in the wave-length on the helix produced by adding a small inductance or capacity to the oscillating circuit, it is obvious that the values of this addition can be calculated. In this manner the inductance of a foot or two of copper wire coiled into one or two turns can easily be measured.

XLIII. *Chemical Dissociation and Electrical Conductivity.* By A. E. GARRETT *and* R. S. WILLOWS, *B.A., D.Sc.*[*]

IN the Philosophical Magazine for July 1899 there appeared a paper by Beattie entitled "Leakage of Electricity from Plates." When various mixtures of substances were placed on the insulated plate of an air-condenser and heated, it was found that a leakage of electricity took place, this being, in several cases, apparently independent of the sign of the charge. The temperatures used were between 300° and 400°. Similar effects were obtained if the substances were placed on the earthed plate.

Shortly after the appearance of this paper one of us repeated the experiments and verified the results generally. The apparatus used was similar to Beattie's. It was found, however, in a good many cases, that the rate of leak depended in a great measure on the sign of the charge given to the insulated plate. In a later paper[†] Beattie found the same result. He showed that if positive electricity leaked from the plate when the substance was on it, then it was incapable of holding a negative charge when that substance was transferred to the earthed plate of the condenser.

The objects of the experiments described in the following paper were to discover the origin of the leak, or, in terms of the ionization theory, to find out the seat of the ionization, and also obtain information concerning the nature of the ions in some of the cases. As a good many of the new observations and facts have been anticipated in Beattie's second paper, only those are given which bear directly on these objects.

[*] Communicated by the Physical Society: read June 24, 1904.
[†] Phil. Mag. April 1901.

Here is a typical instance from Beattie's papers, and one which we have had under investigation. An air-condenser is formed of zinc plates; when iodine is placed in it no leak occurs beyond the small amount due to defective insulation of the supports, although the temperature is raised to 350°. All the iodine has long before this disappeared as vapour. Common salt alone in the condenser also produces no effect when heated to the same temperature; but if the salt is sprinkled with iodine and placed on the insulated plate, then a large leak of positive electricity takes place when the temperature is raised. If the mixture of salt and iodine is placed on the earthed plate, the sign of the discharge is changed, otherwise no difference is noticed. Bromine water can replace the iodine without much affecting the result.

Experience had shown that Beattie's form of apparatus was troublesome to use and unsuitable for quantitative experiments. That finally used is shown in fig. 1.

Fig. 1.
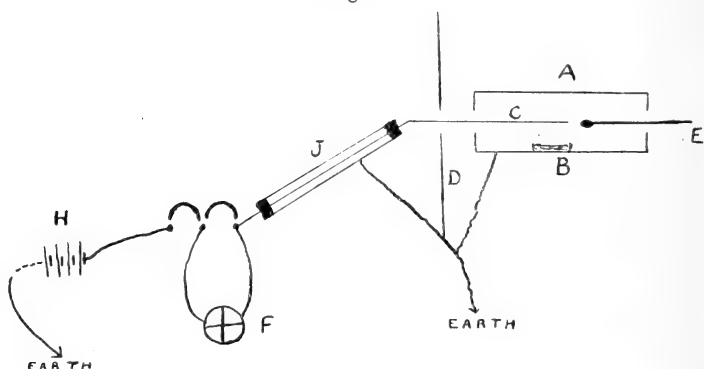
A is a metal cylinder about 12 cms. long and from 4 to 6 cms. in diameter. Caps of the same material partially close the ends. C is a wire placed along the axis of A, it passes through a hole in a metal plate D and then through ebonite plugs in a tube J ; the latter is to screen it from external disturbances. D serves the same purpose for all parts of the apparatus on the left. During the early part of the experiments the measuring instrument was a gold-leaf electroscope. This was connected to C and charged, the case of the instrument, D and A all being earthed. Finally, two hundred small cells were obtained and an electrometer was used. One pole of the battery was connected to C by means of a paraffin switch, the other pole was connected to earth. Both

pairs of quadrants were first connected together to C and to the insulated pole of the battery H. A, D, and J were permanently earthed. The quadrants were then disconnected, one pair was kept on the battery, the other pair to C. A leak of electricity from C is then shown by a constantly increasing deflexion of the electrometer-needle. E is a thermometer reading to 360°.

The insulation was tested when the apparatus was cold, and also when A was heated to 360°. The heating was done electrically by means of a coil of wire wound non-inductively on A and carefully insulated therefrom by a layer of asbestos paper. In series with the heating-coil was an adjustable rheostat which enabled the temperature to be varied within the limits desired, or kept constant within 1° for a considerable time. The substance to be experimented with was placed in a shallow metallic dish B.

Influence of the Nature of the Plate when Salt and Iodine are used.

When the plate B was zinc, Beattie's results were first verified. It was found, however, that the nature of the plate, and, in some cases, of the tube A had an influence. Up to a temperature of 360°, with salt and iodine placed on plate B, the following results with different metals were obtained. The electroscope was used in these experiments; the leaves were observed through a microscope having a scale in the eyepiece, and the time taken for the leaves to fall through the same five divisions was recorded. Cylinder A was made of brass.

Metal.	Charge on insulated electrode.	Time to leak through 5 divisions.		Temperature.
		min.	sec.	°
Zinc ...	Negative	4	0	20
		4	5	112
		3	55	175
		2	50	225
		1	45	242
			35	255
			30	259
			25	262

With a positive charge a slightly increased leak was obtained on heating; this was irregular.

Iodine vapour was visible at about 100°; its appearance did not coincide with an increased leak, sometimes, in fact,

a charging up took place. This might easily be due to the projection of solid particles of iodine. The plate with its mixture could be used after standing for several days, when on heating, and without the addition of fresh iodine, the same behaviour was observed.

Metal.	Charge.	Time to leak through 5 divisions.		Temperature.
		min.	sec.	°
Copper.	Negative	1	15	20
		1	20	185
		1	20	236
		1	15	256
		1	5	272
		1	0	286
			45	291
			45	293
			40	300
			27	302
			15	307

These are results of the same order as for zinc plates. Only a small leak was obtained with a positive charge. It is noticeable that the temperature at which the leakage begins is higher with copper than with zinc.

Metal.	Charge.	Time to leak through 5 divisions.		Temperature.
		min.	sec.	°
Brass ...	Negative	3	5	22
		2	25	225
		3	0	260
		2	20	279
		1	20	290
		1	20	296
		1	0	305

With a positive charge a small leak was obtained. The observations show that the effects are not so great with brass as with copper and zinc.

Metal.	Charge.	Result on heating.
Aluminium............	−ve	Practically no increased leak.
	+ve	Practically no increased leak.
Iron	−ve	No increased leak.
	+ve	No increased leak.
Tinned iron	−ve	Increased leak.
	+ve	Scarcely any increased leak.
Platinum	−ve	No leak.
	+ve	No leak.

From these results it is seen that the metal is a necessary part in the changes causing the conduction; the changes might therefore be looked for at the metallic surface.

From a zinc plate that had been used the adherent mixture was lightly brushed off; this caused no change in the electrical conductivity. If the surface was then cleaned, either with glass-paper or by washing well in distilled water, it was found that its activity had been removed, but it could be restored by fresh addition of salt and iodine.

The washing water was tested, after filtering, for free iodine and for iodine in combination. No free iodine was found, but a large amount was present in combination.

If a zinc plate that had been used was washed in dilute HCl instead of in water, a very large leak was obtained when it was replaced in the apparatus without drying and reheated. It was found then that a clean piece of zinc if dipped in HCl gave the same result, and this although it was previously heated to nearly 200° to drive off any adherent acid and moisture. The zinc plate was now replaced by one of platinum, on which was put some zinc chloride. When heated, this gave a very great leak with a negative charge on C, a smaller one with a positive charge. For example, at ordinary temperatures the time taken to leak through 5 divs. was 5 minutes, when heated to 300° the time required was 12 sec. only, the charge being negative.

The outer cylinder A had been made of brass to this point, but after heating in it zinc chloride it was found to be impossible to make it insulate properly at the higher temperatures. The electroscope also was not suitable for exact measurements. At this point, therefore, an iron cylinder was obtained, the insulated electrode C was formed from a platinum wire, and the dish B was also platinum. No increased leak was then noticed on reheating, no matter what substance had been used. The electroscope was replaced by an electrometer, and cells were used for charging the apparatus.

Experiments with Zinc Chloride.

A small piece of solid $ZnCl_2$ was placed on B, and the current between C and A measured at different temperatures. While an observation was being made the temperature was allowed to rise very slowly, the mean of the values at the beginning and the end of the reading being taken as the temperature of the experiment.

Greater regularity could be attained in this way than by trying to keep the temperature quite constant. The numbers

given in the following table will show the magnitude of the results. In some cases a capacity was connected to the quadrants to reduce the rate of deflexion.

Leak with $ZnCl_2$ at different Temperatures.

Temp.	Leak.	Temp.	Leak.	Temp.	Leak.
125	2 in 30ˢ	275	17 in 10ˢ	319	200 in 10ˢ
157	2 ,,	284	32 ,,	322	250 ,,
208	1 ,.	297	60 ,,	329	160 in 5ˢ
223	0 ,,	303	65 ,,	332	200 ,,
248	8 ,,	307	70 ,,	336	240 ,,
263	32 ,,	310	80 ,,	340	390 ,,
		315	130 ,,		

C in this case was charged to −400 volts.
The numbers are represented graphically in fig. 2.

Fig. 2.
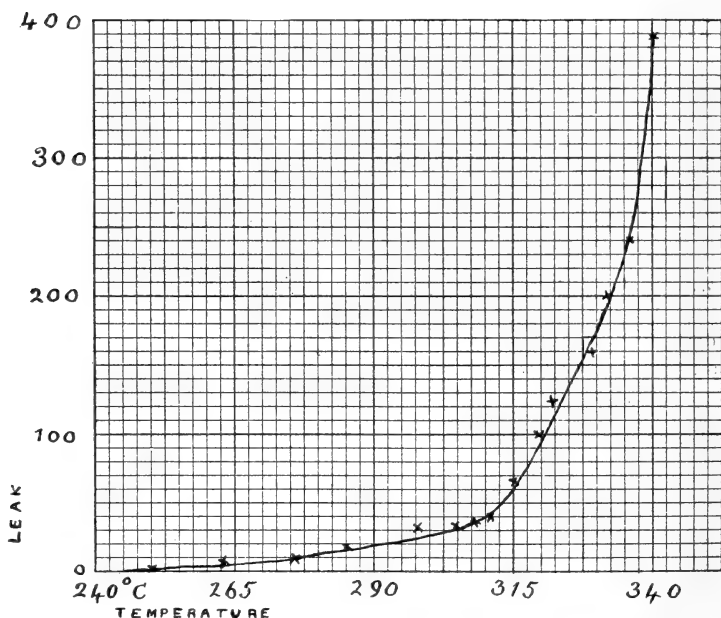
It is seen that the rate of increase with temperature is much quicker after passing 312°. This, as is seen below, is due to another source of ionization being tapped. In such a

case it is hardly to be expected that the results can be expressed throughout their whole range by any simple law.

Fig. 3 gives the relation between the logarithms of the

Fig. 3.
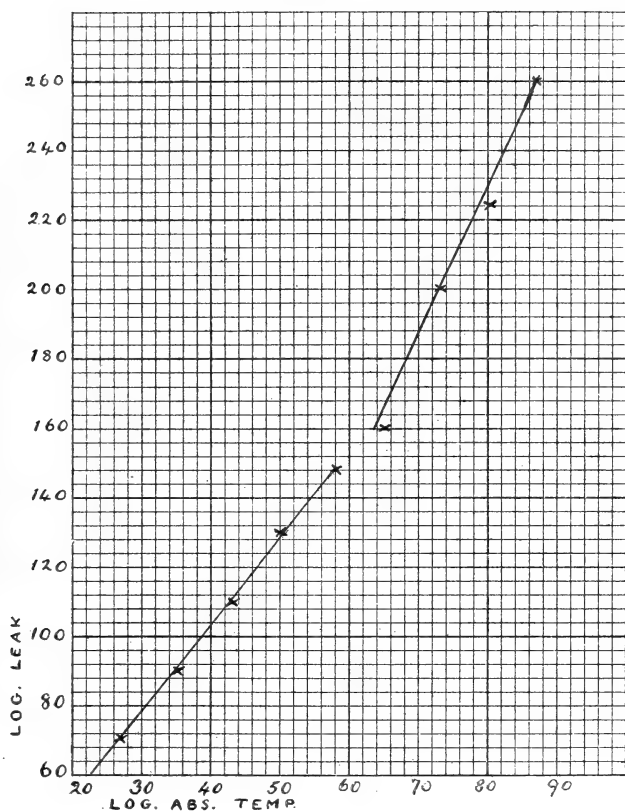
leak and the logarithms of the corresponding absolute temperatures. It shows, while the conduction is produced by the same mechanism, a straight-line law. Within these limits of temperature, therefore, the current I through the gas can be expressed by a formula of the type $I = aT^n$, a and n being constants, of which the former depends on the units employed. For the lower part of the curve $n = 24$, for the upper part $n = 41$.

When the direction of the field is reversed the current is

greatly diminished, the proportion depending upon the temperature. The following is a typical set of readings:—

<div align="center">

C charged to + 200 volts.
Leak with $ZnCl_2$ at different Temperatures.

</div>

Leak.	Temp.	Leak.	Temp.
4	20	21	344
4	150	36	348
4	300	60	351
4	320	106	354
5	330	148	356
12	338		

<div align="center">Fig. 4.</div>
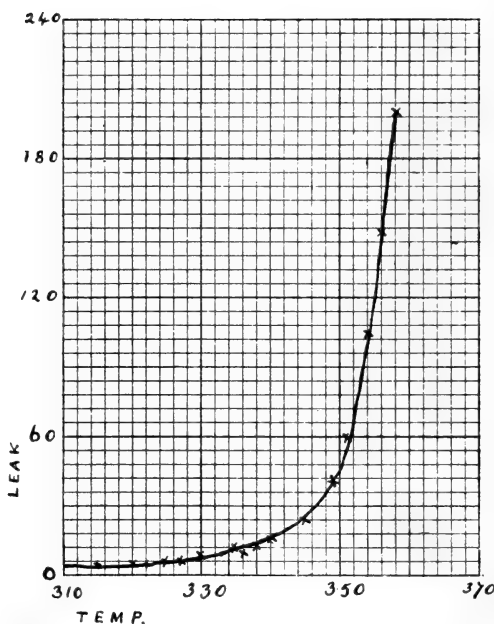
Fig. 4 is plotted from these readings. It shows that the conduction starts at a much higher temperature in this case than in the previous one; 246° in the one and about 322° in the other. It also throws light on the rapid increase in conduction shown in fig. 3 after temperature 320° is passed. At this temperature a new source of ionization appears which produces negative as well as positive ions.

Fig. 5 gives the relation between the logarithms of the

leak and of the absolute temperature. It follows that the current is again expressed by a formula of the type $I = aT^n$. In this case $n = 100$.

Fig. 5.
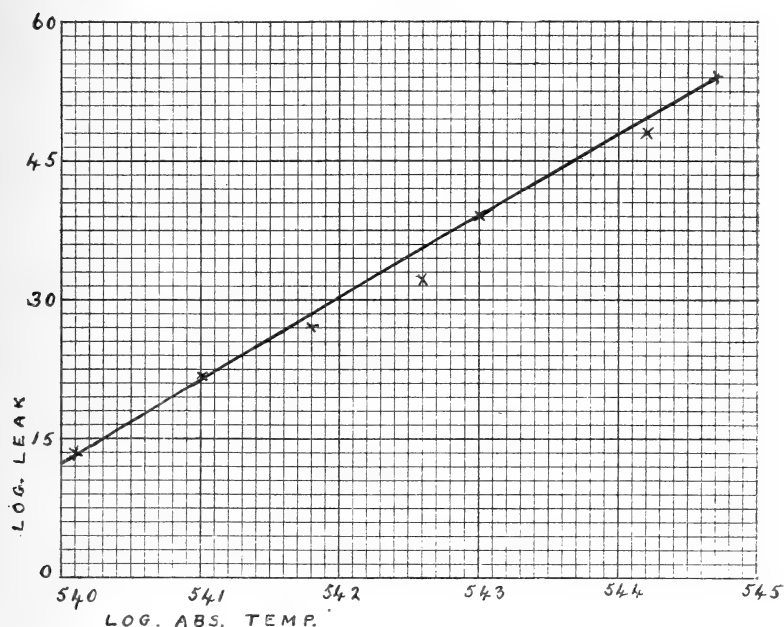
The alteration in current caused by an increase in voltage was next investigated. The temperature was kept constant within $0°\cdot5$. The table following is a typical set of readings; the temperature of the experiment was $345°$.

C charged $-$ve.		C charged $+$ve and leak multiplied by 2.	
Volts.	Leak.	Volts.	Leak.
40	8	18	11
80	17	40	45
120	30	80	70
160	40	120	82
200	62	160	90
238	73	200	105
276	92	238	112
314	110	276	120
352	140	314	120
390	180	352	125
		390	132

The curves in fig. 6 are plotted from these numbers. They resemble the figures given by H. A. Wilson [*] for the

Fig. 6.

VOLTS
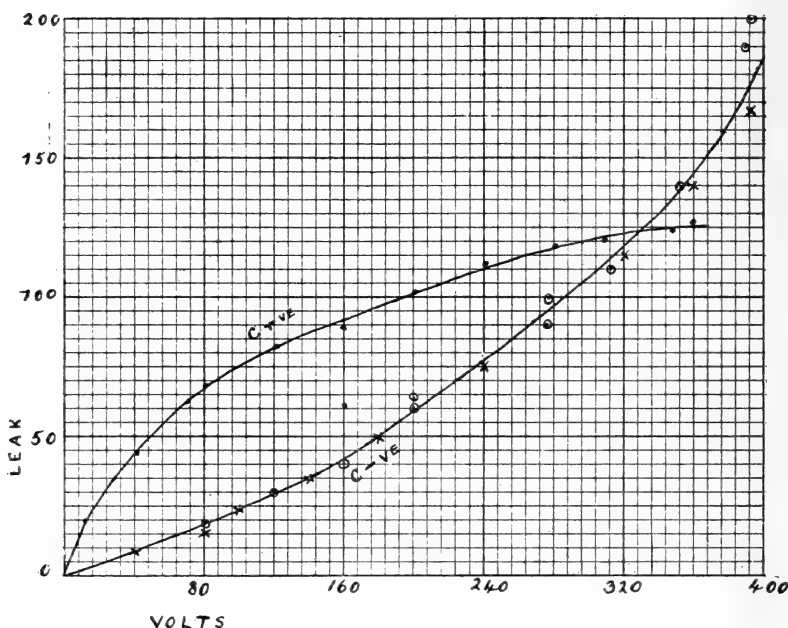
case of the current between a hot platinum wire and a hot platinum tube outside it. When the current is carried by positive ions (C negative) no evidence of a saturation value is evident : this is most probably due to their small velocity, which renders it difficult to drive them out from the thin layer in which they are produced. The negative ions moving more quickly, are more easy to remove.

Another explanation may, however, be given. Prof. **J. J.** Thomson advances the opinion that the emission of negative corpuscles (or electrons) from heated substances is a general property of the atom [†]. An experiment with sodium vapour is given as an instance where the emission takes place at a temperature as low as 300°. If the negative ions are produced in this manner in the present instance, they would be more or less distributed throughout the space between the electrodes. In that case, it is known that a saturation-current

* H. A. Wilson, Phil. Trans. A, cxcvii. p. 415 (1901).
† 'Conduction through Gases,' p. 168.

is more easy to produce than it is when the ionization is confined to a single layer near one electrode.

It can be shown, on certain assumptions*, that the current should vary as the square of the potential-difference between the electrodes when the ions are produced in a narrow space near an electrode. When the current is carried by positive ions, it was found in our experiments that it is expressed with considerable accuracy by a formula of the type $I = av + bv^2$, where $I =$ current, $v =$ voltage, and a and b are constants. The points on the diagram marked with a cross were calculated from such a one. The other curve of fig. 6 is nearly parabolic.

If the conduction caused by heating a mixture of zinc, salt, and iodine be due to the formation of $ZnCl_2$, one way of identifying this would be to measure the rate of decay of the current produced by $ZnCl_2$ when kept for some time at a constant high temperature, and show that the law is the same as for the mixture.

The conduction obtained with $ZnCl_2$ alone was very irregular in amount; it had decreased by less than 10 per cent. when kept for $3\frac{1}{2}$ hours at 360°. After standing for some hours in the apparatus, the current was largely increased on reheating, due probably to the absorption of moisture. The experiments are being repeated and extended with substances that have been carefully dried.

Experiments with a Mixture of Zinc, Salt, and Iodine.

About $\frac{1}{2}$ gram of a mixture of zinc, salt, and iodine was placed on the earthed plate of the condenser and heated, the current due to 200 volts being noted at different temperatures. One set of readings is given below.

C charged to -200 volts.

Temp.	Leak per min.	Temp.	Leak.
20	3	337	28
95	Irregular. Iodine-vapour coming off.	342	52
		346	80
240	3	351·5	120
302	3	354	155
312	2	357	210
323	4	359	250
332	13		

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* 'Conduction through Gases,' p. 175.

The current apparently starts at 323°. The numbers are represented graphically in fig. 7. The relation between current and absolute temperature is again of the form $I = aT^n$ with $n = 60$.

Fig. 7.
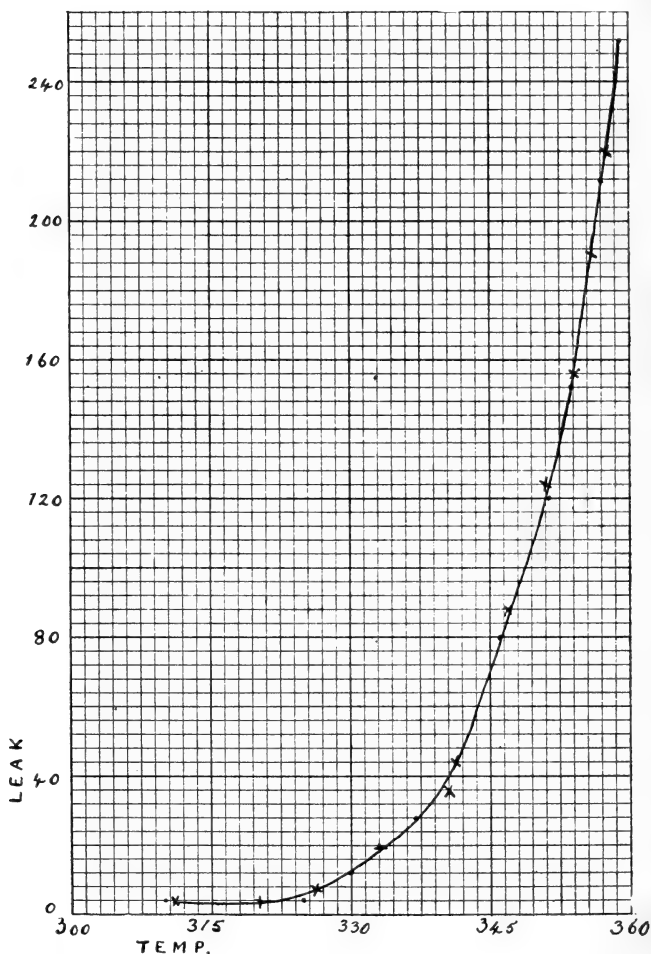
With the field reversed the leak was about 10 divisions per min. at 360°, altogether different from that due to $ZnCl_2$.

A fresh portion of the mixture was put in the apparatus and heated to 355° as quickly as possible. When the

temperature had been constant for 5 min., observations of the current were begun and were kept up for about three hours. The result is shown in fig. 8.

Fig. 8.
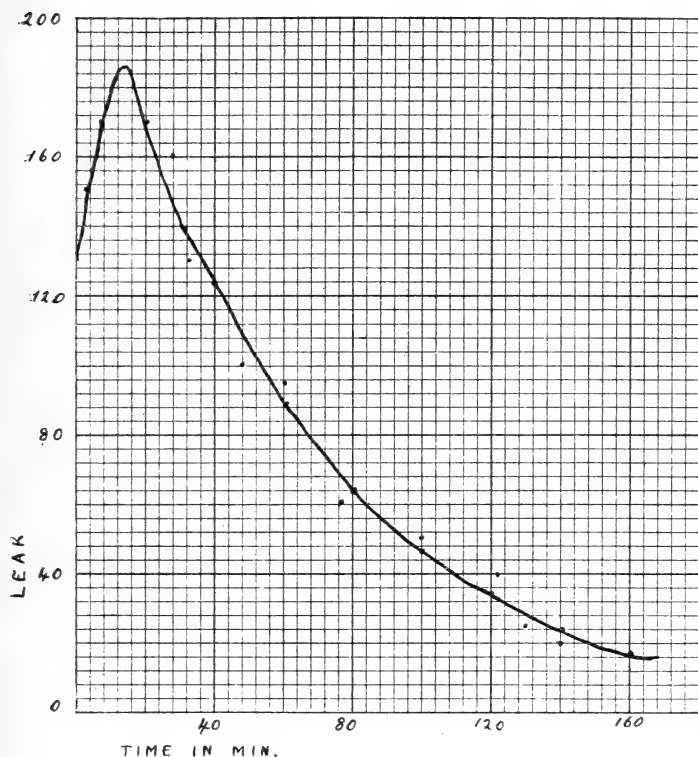
When the current had thus been reduced to 8 per $\frac{1}{2}$ min., no recovery was found to have taken place after standing for fifteen hours, neither did the addition of moisture cause any alteration. A further amount of iodine was added without effect on the leak when the inner electrode was negative, but caused a slightly increased leak when the field was in the opposite direction. After heating for some hours the leak after addition of iodine was, with a positive charge on C, equal to 10 per $\frac{1}{2}$ min., with negative charge 4 per $\frac{1}{2}$ min. The same weight of mixture freshly heated gave 10 per $\frac{1}{2}$ min. with a positive charge. Prolonged heating had therefore no effect on the number of negative ions produced.

Experiments with Zinc Iodide.

Sodium iodide and zinc iodide are two compounds that may be formed when the above mixture is heated.

No increased current could be observed when the former was heated to 360°; the latter gave large currents above 320° when C was negatively charged. If these currents are multiplied by a constant, they are represented by the same curve as that showing the leak from a mixture of zinc-salt-iodine. The crosses in fig. 7 show the leak from zinc iodide alone, the other points from the mixture of zinc-salt-iodine. The positive currents, *i. e.* C charged positively, are the same in the two cases. The rate of decay with time when kept at a constant temperature agrees pretty closely with that given by the mixture (see fig. 8). We scarcely think that the maximum shown here is an experimental error, due to the temperature not having become steady. Neglecting this part, the remainder is an exponential curve.

Suppose a gram of zinc iodide is giving, at a steady temperature, a current I. Let a certain fraction λ become inactive during each minute due to chemical dissociation. Then

$$-\frac{d\mathrm{I}}{dt} = \lambda\,\mathrm{I}$$

and

$$\mathrm{I} = \mathrm{I}_0 e^{-\lambda t},$$

where I_0 is the maximum value of the current.

The time, t, for the leak to fall to half its maximum value is given by $t = \dfrac{\log 2}{\lambda}$. This is equal to 42 min. for the mixture zinc-salt-iodine and 40 min. for zinc iodide. It thus becomes apparent that the conduction observed by Beattie when salt, sprinkled with iodine, is heated on a zinc plate is due to the formation of zinc iodide at the lower temperatures, and a subsequent breaking up as the temperature rises. The sodium chloride appears to have a catalytic action during the formation, as the direct heating of zinc and iodine together causes no leak.

It is to be noted that the expression $\mathrm{I} = a\mathrm{T}^n$ does not give a definite temperature at which the conduction begins; if, therefore, it accurately represents the current, a more sensitive apparatus would detect this at a lower temperature than in our experiments.

Experiments with Zinc Bromide.

As far as our experiments have gone at present, the results with zinc bromide are similar to those obtained with the iodide. The current, however, alters much more rapidly with the time.

In the preceding experiments, the zinc chloride was not specially dried. When heated, it is known that HCl is formed. In order to see whether this would account for the leak from the chloride, the apparatus was gradually heated and dry HCl gas admitted. An appreciable current was noticed at a temperature of 200°, and this did not depend on the direction of the field. When air was drawn over the chloride and bubbled through $AgNO_3$ a precipitate was at once formed ; but if the nitrate was replaced by water and the bubbling took place for half an hour, no zinc could be found in the solution. When the bubbling took place through starch and KI solution, no coloration was noticed, so that no free chlorine was present. Next the air was pulled through an insulated test-tube packed with cotton-wool and coated on the outside with tinfoil. This was connected to the electrometer quadrants. When the foil was charged negatively, there was a further charging up when the air was drawn through ; when charged positively, a greatly increased leak. There would therefore appear to be an excess of negative electricity drawn off from $ZnCl_2$ at this temperature (360°). .

Some experiments were next carried out with the object of discovering the nature of the ions concerned in the conduction. For this end their velocity was determined at a temperature of 360°.

Fig. 9.
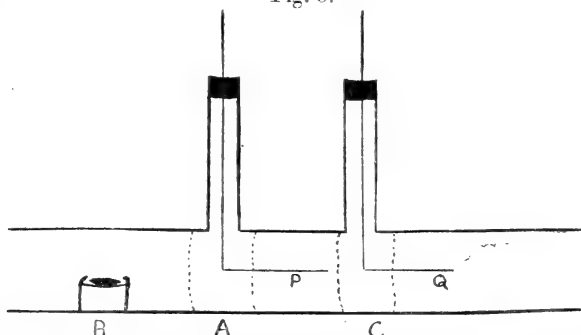
A method previously used by McClelland * to measure the velocities of ions from arcs was used. The apparatus is shown in fig. 9. The substance is heated at R, and air is drawn

* Proc. Camb. Phil. Soc. x. p. 241 (1899).

over it from left to right. The tube and electrode P are earthed and a field is put on at Q sufficiently large to remove all the ions reaching it; this number being proportional to the rate of deflexion of the electrometer-needle which is connected to Q. A field is now put on at P, and so some ions are removed, the reduced rate of deflexion of the electrometer connected with Q measuring this amount. Then, if ρ is the proportion removed by the field at P; A the difference of potential between P and the tube; b=radius of tube, a=radius of P; and t=time taken by the current of air to pass along P, the velocity of the ions u is given by*

$$u = \frac{\rho(b^2 - a^2) \log_e (b/a)}{2At}.$$

To ensure insulation, the tubes carrying P and Q were made several inches long, and to facilitate the fixing of the electrodes the sections of the tube A, C, could be screwed off the other parts. The electrodes were made sometimes of platinum, sometimes of aluminium, the outer tube was iron.

The velocities for the positive ions under a potential gradient of one volt per centimetre were as follows:—

ions from $ZnCl_2 = \cdot0062$,
ions from $ZnBr_2 = \cdot0059$,
ions from $ZnI_2 = \cdot0057$;

i. e., the velocities were the same in all cases within the limits of experimental error. The relative velocities are probably much more accurate than the absolute velocities. In order to be able to interpret definitely the results of the measurements, the field at Q should be sufficient to produce the saturation-current. Owing to the smallness of the velocity we could not fulfil this condition as well as would be desirable. The results for the negative ions from zinc chloride were very irregular. The mean of a large number of results, which differed considerably from each other, gave a velocity $= \cdot02$ cm. per sec. under one volt per centimetre.

During some of the experiments, we had occasion to draw air over the substances into a large bottle containing water. We usually, but not always, noticed in these circumstances an exceedingly dense fog, which was produced without expansion, and which hung in the bottle for half an hour before dispersion†. The bottle was several yards away from the

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* 'Conduction in Gases,' J. J. Thomson, p. 48.

† Similar effects have been noticed by Townsend, Wilson, and Meissner (see 'Conduction in Gases') for gases from electrolysis, &c.

apparatus. The air was drawn through a tube much narrower than that shown in fig. 9, and a strong field created between it and an electrode along the centre. We were unable to stop by this means the formation of fog. Bubbling through several inches depth of various liquids also failed to stop it, as did also a tight plug of cotton-wool when placed near the water-bottle. When the plug was placed near the apparatus, the fog was greatly reduced. The air could be allowed to remain in the connecting tubing for several minutes before admission to the water-bottle without greatly impairing its efficiency as a fog producer. The introduction of iodine vapour into the bottle, brought about by sucking air over boiling iodine, caused no fog.

These fog experiments seem to show that the ions are bodies of considerable size, while those on their velocity would, if trustworthy, show that they are similar whether they come from the chloride, iodide, or bromide, and would thus preclude the halogens themselves. Their source would thus be narrowed down to either the zinc or, perhaps, the oxygen of the air. The former of these views would correspond to the ejection of α rays from radioactive substances, the fact of the chemical dissociation of the compound, bromide, iodide, or chloride into its elements rendering this ejection possible. The latter of the two explanations would proceed on the lines marked by Prof. J. J. Thomson * to explain the power of oxygen to change a negative into a positive electrification, in the gas near an incandescent solid. Experiments to test these views are now in progress.

The energy required to ionize a gram-molecule of zinc-chloride vapour may be calculated, if we take the curve in fig. 4 and assume that the negative ions are ejected from the $ZnCl_2$ vapour. This quantity, q, in calories, is given by the formula †

$$\frac{q}{2}\left(\frac{1}{\theta_2} - \frac{1}{\theta_1}\right) = \log_e \frac{n_1^2}{n_2^2},$$

n_1 and n_2 being the small currents, in arbitrary units, at absolute temperatures θ_1 and θ_2, where $\theta_1 - \theta_2$ is only a small interval of temperature. For a mean temperature of $335°$ C. the result is 165,000 calories. Taking the charges on the ions to be the same as in electrolysis, an estimate may be formed of the potential-difference, V volts, through which the charge must fall freely in order to gain energy enough to ionize a molecule. Since it requires 96,000 coulombs to

* ' Conduction in Gases.'
† H. A. Wilson, Phil. Trans. cxcvii. A. p. 429 (1901).

electrolyse a gram-molecule of a substance, the energy spent $= 96,000 \times 10^7$ V ergs, and this is the equivalent of 165,000 calories. Hence

$$96,000 \times 10^7 \, V = 165,000 \times 4 \cdot 2 \times 10^7$$

and $$V = 7 \text{ volts,}$$

in good agreement with the estimates of Townsend, H. A. Wilson, J. Stark, and Langevin.

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XLIV. *On the Front and Rear of a Free Procession of Waves in Deep Water.* (Continued from *Phil. Mag.,* June, 1904.) *By* Lord KELVIN [*].

§ 11. THE present communication is substituted for another bearing the same title, which was read before the Royal Society of Edinburgh on January 7th, 1887, because the result of that paper was rendered imperfect and unsatisfactory by omission of the exponential factor referred to in § 10 of my paper of February 1st, 1904. I shall refer henceforth to the last-mentioned paper as §§ 1 . . . . 10 above.

§ 12. I begin by considering processions produced by superposition of static initiating disturbances, of the type expressed in (12) of § 4 above ; graphically represented by fig. 1 ; and leading to motion investigated in §§ 1–3, 5–10. The particular type of that solution which I now choose, is that chosen at the end of § 4, which we, with a slight but useful modification †, may now write as follows :—

$$-\zeta = \phi(x,\, t) = \sqrt{\frac{2}{\rho}} \cos\!\left(\frac{gt^2 x}{4\rho^2} - \tfrac{1}{2}\chi\right) \epsilon^{\frac{-gt^2 z}{4\rho^2}} \left.\begin{array}{c} \\ \\ \end{array}\right\} \quad . \quad (17).$$
$$\text{where } \rho = \sqrt{(z^2 + x^2)}, \text{ and } \chi = \tan^{-1}(x/z)$$

Here $-\zeta$ denotes the upward vertical component of the displacement of the fluid at time t from its undisturbed position at point $(x,\, z)$, which may be either in the free surface or anywhere below it. Taking $t = 0$ in (17), we

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† The substitution of $\tfrac{1}{2}\chi$, for $\tfrac{1}{2}\pi - \tan^{-1}\sqrt{\dfrac{\rho + z}{\rho - z}}$, saves considerable labour and use of logarithms; especially when, as in our calculations, $z = 1$.

have, for the initial height of the free surface above the undisturbed level,

$$-\zeta_0 = \phi(x, 0) = \sqrt{\frac{2}{\rho}} \cdot \sqrt{\frac{\rho+z}{2\rho}} = \frac{\sqrt{(\rho+z)}}{\rho} \quad . \quad (18).$$

§ 13. We shall first take, as initiating disturbance, a row extending from $-\infty$ to $+\infty$ of superpositions of (18) ; alternately positive and negative ; and placed at equal successive distances $\frac{1}{2}\lambda$: so that we now have

$$-\zeta_0 = \mathrm{P}(x, 0) = \sum_{i=-\infty}^{i=+\infty} (-1)^i \phi\left(x + i\frac{\lambda}{2}, 0\right). \quad . \quad (19).$$

or, as we may write it,

$$-\zeta_0 = \mathrm{P}(x, 0) = \sum_{i=-\infty}^{i=+\infty} \mathrm{D}(x + i\lambda, 0) . \quad . \quad . \quad . (19'),$$

where

$$\mathrm{D}(x, 0) = \phi(x, 0) - \phi\left(x + \frac{\lambda}{2}, 0\right) \quad . \quad . \quad (20).$$

In (19), P denotes a space-periodic function, with λ for its period. This formula, with t substituted for 0, represents $-\zeta_t$, being the elevation of the surface above undisturbed level at time t, in virtue of initial disturbance represented by (19).

§ 14. Remark now that whatever function be represented by ϕ, the formula for P in (19) implies that

$$\mathrm{P}(x+\lambda, 0) = \mathrm{P}(x, 0) \quad . \quad . \quad . \quad . \quad (21),$$

which means that P is a space-periodic function with λ for period. And (19) also implies that

$$\mathrm{P}(x + \tfrac{1}{2}\lambda, 0) = -\mathrm{P}(x, 0) \quad . \quad . \quad . \quad (22) ;$$

which includes (21). And with the actual function, (18), which we have chosen for $\phi(x, 0)$, the fact that $\phi(x, 0) = \phi(-x, 0)$ makes

$$\mathrm{P}(x, 0) = \mathrm{P}(-x, 0) \quad . \quad . \quad . \quad . \quad (23).$$

Thus (19) has a graph of the character fig. 5, symmetrical

Fig. 5.
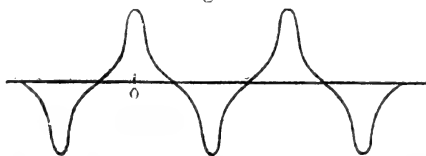
on each side of each maximum and minimum ordinate. The

Fourier harmonic analysis of $P(x, 0)$, when subject to (22) and (23), gives

$$P(x, 0) = A_1 \cos \frac{2\pi x}{\lambda} + A_3 \cos 3\frac{2\pi x}{\lambda} + A_5 \cos 5 \frac{2\pi x}{\lambda} + \dots \quad (24).$$

§ 15. *Digression on periodic functions generated by addition of values of any function for equidifferent arguments.* Let $f(x)$ be any function whatever, periodic or non-periodic; and let

$$P(x) = \sum_{i=-\infty}^{i=+\infty} f(x + i\lambda) \quad . \quad . \quad . \quad . \quad (25);$$

which makes

$$P(x) = P(x + \lambda) \quad . \quad . \quad . \quad . \quad . \quad (26).$$

Let the Fourier harmonic expansion of $P(x)$ be expressed as follows:—

$$\left. \begin{array}{l} P(x) = A_0 + A_1 \cos \alpha + A_2 \cos 2\alpha + A_3 \cos 3\alpha + \dots \\ \qquad + B_1 \sin \alpha + B_2 \sin 2\alpha + B_3 \sin 3\alpha + \dots \end{array} \right\} \text{where } \alpha = \frac{2\pi x}{\lambda}$$
$$. \quad . \quad . \quad (27).$$

Denoting by j any integer, we have by Fourier's analysis

$$\tfrac{1}{2}\lambda \begin{array}{l} A_j \\ B_j \end{array} = \int_0^\lambda dx P(x) \begin{array}{l} \cos \\ \sin \end{array} j \frac{2\pi x}{\lambda} \quad . \quad . \quad . \quad (28);$$

which gives

$$\left. \begin{array}{l} \tfrac{1}{2}\lambda A_j = \displaystyle\sum_{i=-\infty}^{i=+\infty} \int_0^\lambda dx f(x + i\lambda) \cos j\frac{2\pi x}{\lambda} = \int_{-\infty}^{+\infty} dx f(x) \cos j\frac{2\pi x}{\lambda} \\[4mm] \tfrac{1}{2}\lambda B_j = \displaystyle\sum_{i=-\infty}^{i=+\infty} \int_0^\lambda dx f(x + i\lambda) \sin j\frac{2\pi x}{\lambda} = \int_{-\infty}^{+\infty} dx f(x) \sin j\frac{2\pi x}{\lambda} \end{array} \right\} \quad (29).$$

§ 16. Take now in (29), as by (19′), (20),

$$f(x) = \phi(x, 0) - \phi(x + \frac{\lambda}{2}, 0). \quad . \quad . \quad (30).$$

This reduces all the B's to zero; reduces the A's to zero for even values of j; and for odd values of j gives, in virtue of (22),

$$\tfrac{1}{2}\lambda A_j = 2 \int_{-\infty}^{+\infty} dx \phi(x, 0) \cos j\frac{2\pi x}{\lambda} \quad . \quad . \quad (31).$$

Go back now to §§ 3, 4, (6), (12), above; and, according to the last lines of § 4, take

$$\phi(x, 0) = \left\{ \text{RS} \right\} \frac{\sqrt{2}}{\sqrt{(z + \iota x)}} = \frac{\sqrt{(\rho + z)}}{\rho} \quad . \quad (32).$$

Hence, for the harmonic expansion (24) of P(x, 0), we have

$$\mathbf{A}_j = \frac{4}{\lambda} \int_{-\infty}^{+\infty} dx \, \frac{\sqrt{(\rho + z)}}{\rho} \cos j \frac{2\pi x}{\rho} = \frac{4}{\lambda} \left\{ \text{RS} \right\} \int_{-\infty}^{+\infty} dx \, \frac{\sqrt{2}}{\sqrt{(z + \iota x)}} \cos j \frac{2\pi x}{\lambda}$$

$$\ldots \quad (33).$$

The imaginary form of the last member of this equation facilitates the evaluation of the integral. Instead of $\cos j \frac{2\pi x}{\lambda}$ in the last factor, substitute

$$\cos j \frac{2\pi x}{\lambda} + \iota \sin \frac{2\pi x}{\lambda}, \quad \text{or } \epsilon^{\iota j \frac{2\pi x}{\lambda}} \ldots \quad (34).$$

The alternative makes no difference in the summation $\int_{-\infty}^{+\infty} dx$, because the sine term disappears for the same reason that the sine terms in (29) disappear because of (30). Thus (33) becomes

$$\mathbf{A}_j = \frac{4}{\lambda} \left\{ \text{RS} \right\} \int_{-\infty}^{+\infty} dx \frac{\sqrt{2}}{\sqrt{(z + \iota x)}} \epsilon^{j \frac{2\pi \iota x}{\lambda}} \ldots \quad (35);$$

put now $\sqrt{(z + \iota x)} = \iota \sigma$;

whence $\dfrac{dx}{\sqrt{(z + \iota x)}} = 2 d\sigma$, and $\iota x = -\sigma^2 - z$ . . (36).

Using these in (35) we may omit the instruction {RS} because nothing imaginary remains in the formula: thus we find

$$\mathbf{A}_j = \frac{8\sqrt{2}}{\lambda} \int_{-\infty}^{\infty} d\sigma \epsilon^{-\frac{2\pi j}{\lambda} \sigma^2} \cdot \epsilon^{-\frac{2\pi j z}{\lambda}} = \epsilon^{-\frac{2\pi j z}{\lambda}} \cdot \frac{8\sqrt{2}}{\lambda} \cdot \sqrt{\frac{\lambda}{2\pi j}} \cdot \sqrt{\pi} \ (37),$$

$$= \epsilon^{-\frac{2\pi j z}{\lambda}} \cdot \frac{8}{\sqrt{j\lambda}} \quad \ldots \quad \ldots \quad \ldots \quad (38).$$

The transition in (37) is made in virtue of Laplace's celebrated discovery $\displaystyle\int_{-\infty}^{\infty} d\sigma \epsilon^{-m\sigma^2} = \sqrt{\frac{\pi}{m}}$.

§ 17. Equation (38) allows us readily to see how near to a curve of sines is the graph of P(x, 0), for any particular value of λ/z. It shows that

$$\mathbf{A}_1 = \frac{8}{\sqrt{\lambda}} \epsilon^{-\frac{2\pi z}{\lambda}}; \ \mathbf{A}_3/\mathbf{A}_1 = \sqrt{\tfrac{1}{3}} \cdot \epsilon^{-\frac{4\pi z}{\lambda}}; \ \mathbf{A}_5/\mathbf{A}_3 = \sqrt{\tfrac{3}{5}} \cdot \epsilon^{-\frac{4\pi z}{\lambda}}; \ (38).$$

Suppose for example $\lambda = 4z$; we have

$$\epsilon^{-\frac{4\pi z}{\lambda}} = \epsilon^{-\pi} = \cdot 043214 ; \quad A_3/A_1 = \cdot 02495 ; \quad A_5/A_3 = \cdot 03347. \quad (39).$$

Thus we see that A_3 is about $1/40$ of A_1 ; and A_5, about $\frac{1}{30}$ of A_3. This is a fair approach to sinusoidality ; but not quite near enough for our present purpose. Try next $\lambda = 2z$; we have

$$A_1 = \frac{8}{\sqrt{\lambda}} \cdot \cdot 043214 ; \quad \epsilon^{-2\pi} = \cdot 001867 ; \quad A_3/A_1 = \cdot 001078 . \quad (40).$$

Thus A_3 is about a thousandth of A_1 ; and A_5 about $1\frac{1}{3} \times 10^{-6}$ of A_1. This is a quite good enough approximation for our present purpose: A_5 is imperceptible in any of our calculations: A_3 is negligible, though perceptible if included in our calculations (which are carried out to four significant figures) : but it would be utterly imperceptible in our diagrams. Henceforth we shall occupy ourselves chiefly with the free surface, and take $z = h$, the height of O, the origin of coordinates above the undisturbed level of the water.

§ 18. To find the water-surface at any time t after being left free and at rest, displaced according to any periodic function $P(x)$ expressed Fourier-wise as in (27) ; take first, for the initial motionless surface-displacement, a simple sinusoidal form,

$$-\zeta_0 = A \cos (mx - c) . \quad . \quad . \quad . \quad . \quad (41).$$

Going back to (2), (3), and (4) above, let $w(z, x, t)$ be the downwards vertical component of displacement. We thus have, as the differential equations of the motion,

$$g \frac{dw}{dz} = \frac{d^2 w}{dt^2} \quad . \quad . \quad . \quad . \quad . \quad (42),$$

$$\frac{d^2 w}{dx^2} + \frac{d^2 w}{dz^2} = 0 . \quad . \quad . \quad . \quad . \quad (43).$$

These are satisfied by

$$w = C\epsilon^{-mz} \cos (mz - c) \cos t\sqrt{gm} . \quad . \quad . \quad (44),$$

which expresses the well-known law of two-dimensional periodic waves in infinitely deep water. And formula (44) with $C\epsilon^{-mh} = A$ and $t = 0$, agrees with (41). Hence the addition of solutions (44), with jm for m ; with A successively put equal to $A_1, A_2 \ldots, B_1, B_2 \ldots$; and with $c = 0$ for the A's, and $= \frac{1}{2}\pi$ for the B's, gives us, for time t, the vertical component-displacement at depth $z - h$ below the surface, if at time $t = 0$ the water was at rest with its surface displaced according to (27). Thus, with (38), and (24), we have $P(x, t)$.

§ 19. Looking to (44) and (27), and putting $m = 2\pi/\lambda$, we see that the component motion due to any one of the A's or B's in the initial displacement is an endless infinite row of standing waves, having wave-lengths equal to λ/j and time-periods expressed by

$$\tau_j = \frac{2\pi}{\sqrt{gm}} = \sqrt{\frac{2\pi\lambda}{jg}} \quad . \quad . \quad . \quad . \quad (45).$$

The whole motion is not periodic because the periods of the constituent motions, being inversely as $\sqrt{j}$, are not commensurable. But by taking $\lambda = 2h$ as proposed in § 17, which, according to (40), makes A_3, for the free surface, only a little more than 1/1000 of A_1, we have so near an approach to sinusoidality that in our illustrations we may regard the motion as being periodic, with period (45) for $j = 1$. This makes $\tau = \sqrt{\pi}$ when, as in § 5, we, without loss of generality (§ 10), simplify our numerical statements by taking $g = 4$; and $h = 1$, which makes the wave-length $= 2$.

§ 20. Toward our problem of "front and rear," remark now that the infinite number of parallel straight standing sinusoidal waves which we have started everywhere over an infinite plane of originally undisturbed water, may be ideally resolved into two processions of sinusoidal waves of half their height travelling in contrary directions with equal velocities $2/\sqrt{\pi}$.

Instead now of covering the whole water with standing waves, cover it only on the negative side of the line (not shown in our diagrams) Y O Y′, that is the left side of O the origin of coordinates; and leave the water plane and motionless on the right side to begin. At all *great* distances on the left side of O, there will be in the beginning, standing waves equivalent to two trains of progressive waves, of wave-length 2, travelling rightwards and leftwards with velocity $2/\sqrt{\pi}$. The smooth water on the right of O is obviously invaded by the rightward procession.

§ 21. Our investigation proves that the extreme perceptible rear of the leftward procession (marked R in fig. 10 below) does not, through the space O R on the left side of O, broadening with time, nor anywhere on the right of O, perceptibly disturb the rightward procession.

§ 22. Our investigation also proves that the surface at O has simple harmonic motion through all time. It farther shows that the rightward procession is very approximately sinusoidal, with simple harmonic motion, through a space O F (fig. 9) to the right of O, broadening with time; and that, at any particular distance rightwards from O, this approximation becomes more and more nearly perfect as time

advances. What I call the front of the rightward procession, is the wave disturbance beyond the point F, at a not strictly defined distance rightwards from O, where the approximation to sinusoidality of shape, and simple harmonic quality of motion, is only just perceptibly at fault. We shall find that beyond F the waves are, as shown in fig. 9, less and less high, and longer and longer, at greater and greater distances from O, at one and the same time ; but that the wave-height does not at any time or place come abruptly to nothing. The propagational velocity of the beginning of the disturbance is in reality infinite, because we regard the water as infinitely incompressible.

§ 23. Thus we see that the front of the rightward procession, with sinusoidal waves following it from O, is simply given by the calculation, for positive values of x, of the motion due to an initial motionless configuration of sinusoidal furrows and ridges on the left side of O. Fig. 8 represents a static initial configuration, which we denote by Q $(x, 0)$, approximately realising the condition stated in § 20. Fig. 9 represents on the same scale of ordinates the surface displacement at the time 25τ in the subsequent motion due to that initial configuration ; which, for any time t, we denote by Q (x, t) defined as follows :—

$$Q(x,t)=\tfrac{1}{2}\phi(x,t)-\phi(x+1,t)+\phi(x+2,t)-\ldots ad.\ inf.\ (46),$$

where ϕ is the function defined by (17), with $z=1$ and $g=4$.

§ 24. The wave-height, at all distances so far leftward from O that the influence of the rear of the leftward procession has not yet reached them at any particular time, t, after the beginning, is simply the P(x, t) of § 13 calculated according to §§ 18, 17; and the motion there is still merely standing waves, ideally resolvable into rightward and leftward processions. Let I, beyond the leftward range of fig. 10, be the point of the ideally extended diagram, not precisely defined, where the leftward procession at any particular time, t, becomes sensibly influenced by its own rear. Between I and R the whole motion is transitional in character, from the regular sinusoidal motion P(x, t) of the water on the left side of I, to regular sinusoidal motion of half wave-height $\tfrac{1}{2}$P(x, t), from R to O ; and on to F of fig. 9, the beginning of the front of the disturbance in the rightward procession. Hence to separate ideally the leftward procession from the whole disturbance due to the initial configuration, we have only to subtract $\tfrac{1}{2}$P(x, t) from Q(x, t) calculated for negative values of x. Thus the expression for the whole of the leftward procession is

$$Q(x,\ t)-\tfrac{1}{2}P(x,\ t) \text{ for negative values of } x \quad . \quad (47).$$

Fig. 10 represents the free surface thus found for the leftward procession alone at time $t = 25\tau$.

§ 25. The function $D(x, t)$, which appears in § 13 as an item in one of the modes of summing shown for $P(x, 0)$ in (19′), and indicated for $P(x, t)$ at the end of § 13, and which has been used in some of our summations for $Q(x, t)$; is represented in figs. 6 and 7, for $t = 0$, and $t = 25\tau$ respectively.

§ 26. Except for a few of the points of fig. 6, representing $D(x, 0)$, the calculation has been performed solely for integral values of x. It seemed at first scarcely to be expected that a fair graphic representation could be drawn from so few calculated points ; but the curves have actually been drawn by Mr. Witherington with no other knowledge than these points, except information as to all zeros (curve cutting the line of abscissas), through the whole range of each curve. The calculated points are marked on each curve ; and it seems certain that, with the knowledge of the zeros, the true curve must lie very close in each case to that drawn by Mr. Witherington.

§ 27. The calculation of $Q(x, t)$, for positive integral values of x, is greatly eased by the following arrangements for avoiding the necessity for direct summation of a sluggishly convergent infinite series shown in (46), by use of our knowledge of (Px, t). We have, by (46) and (19),

$$Q(0, t) = \tfrac{1}{2}\phi(0, t) - \phi(1, t) + \phi(2, t) - \ldots \, ad \, inf. \quad (48),$$

$$P(0, t) = \sum_{i=-\infty}^{i=+\infty} (-1)^i \phi(i, t) \quad . \quad . \quad . \quad (49).$$

Hence, in virtue of $\phi(-i, t) = \phi(i, t)$,

$$P(0, t) = 2Q(0, t) \quad . \quad . \quad . \quad . \quad (50).$$

Again going back to (46), we have

$$Q(x, t) = \tfrac{1}{2}\phi(x, t) - \phi(x+1, t) + \phi(x+2, t) - \phi(x+3, t) + \ldots$$
$$Q(x+1, t) = \qquad \tfrac{1}{2}\phi(x+1, t) - \phi(x+2, t) + \phi(x+3, t) - \ldots$$

By adding these we find

$$Q(x+1, t) + Q(x, t) = \tfrac{1}{2}[\phi(x, t) - \phi(x+1, t)] = \tfrac{1}{2}D(x, t) . (51).$$

By successive applications of this equation, we find

$$2Q(x+i, t) = (-1)^i 2Q(x, t) - (-1)^i D(x, t) \pm \ldots + D(x+i-1, t) \quad (52).$$

Hence by putting $x = 0$, and using (50), we find finally

$$2Q(i, t) = (-1)^i P(0, t) - (-1)^i D(0, t) \pm \ldots + D(i-1, t) \quad (53).$$

This is thoroughly convenient to calculate $Q(1, t)$, $Q(2, t)\ldots$ successively; for plotting the points shown in fig. 9.

Fig. 6.

D(x, 0).

Fig. 7.
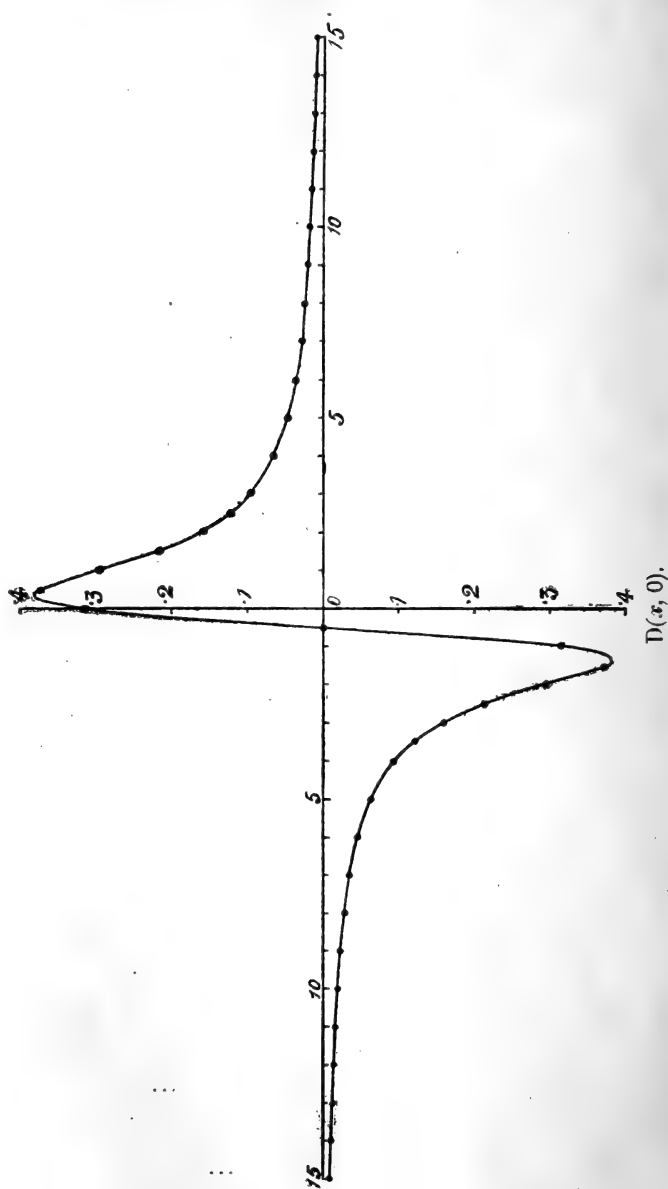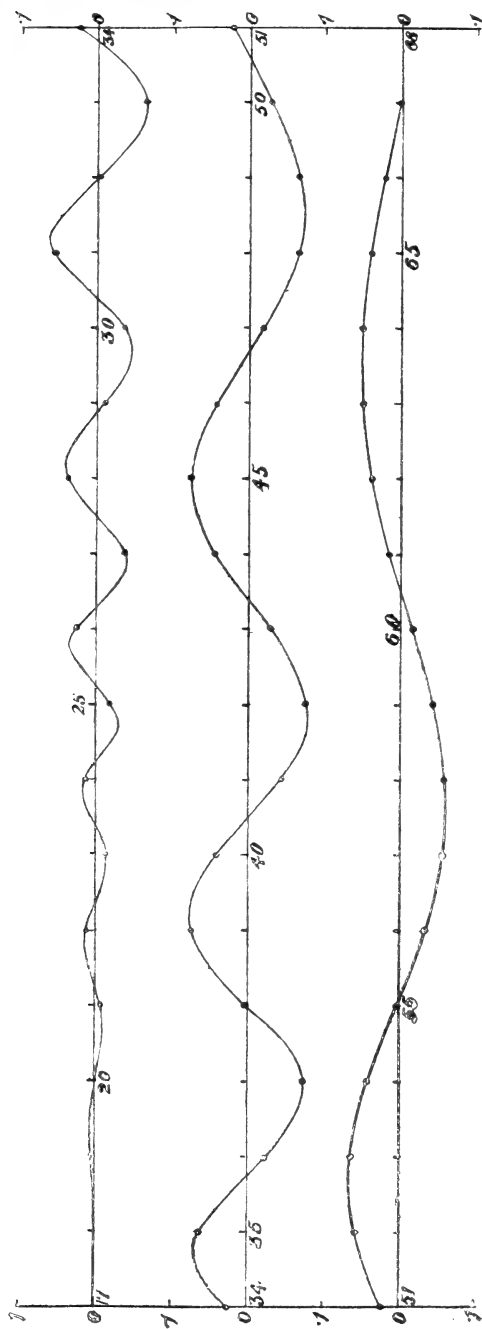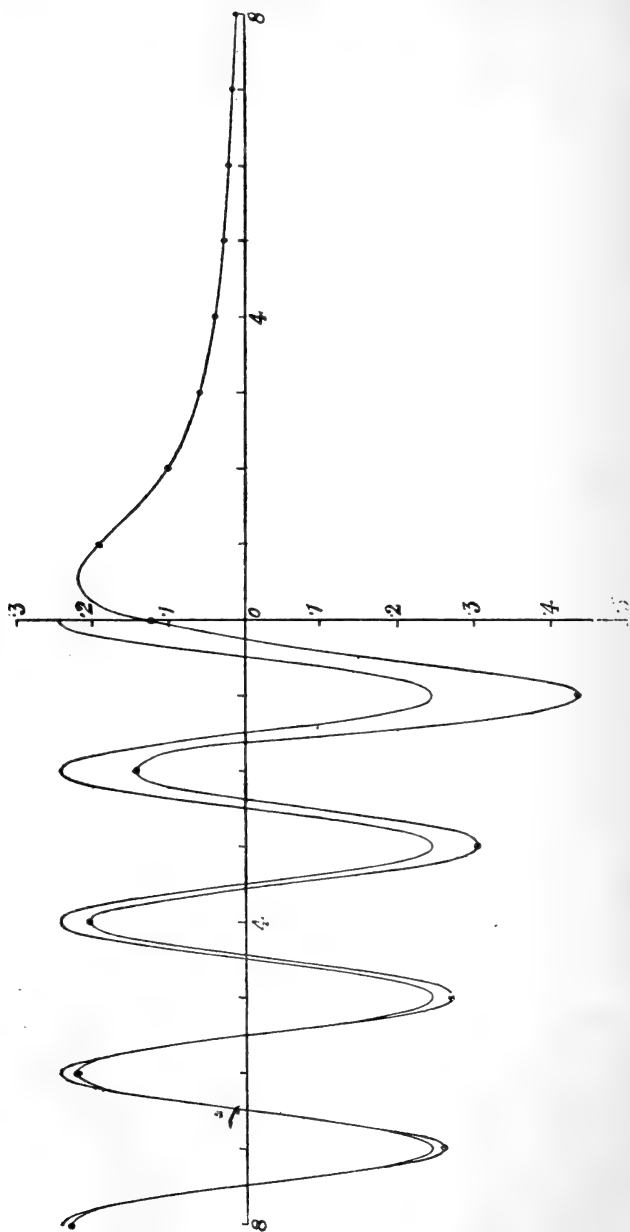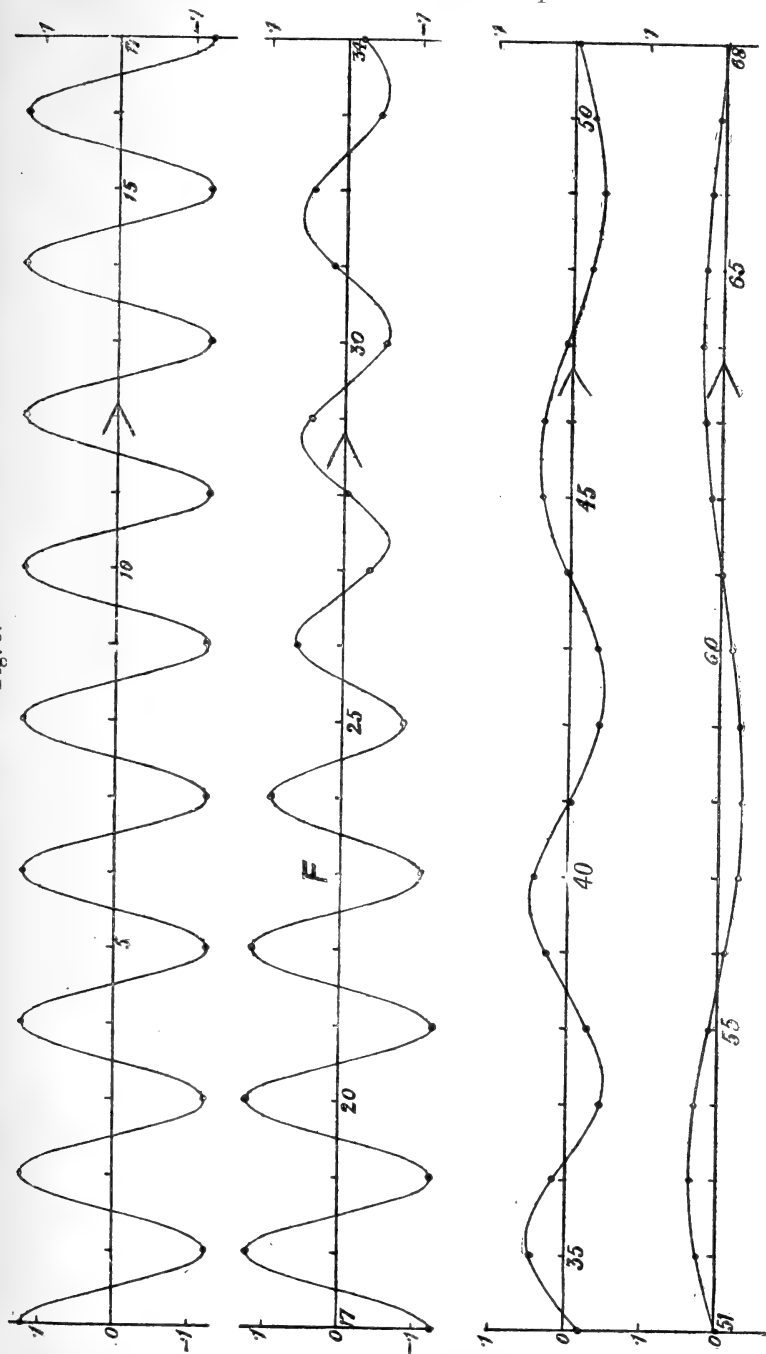
$D(s, 25\eta)$.

Fig. 8.

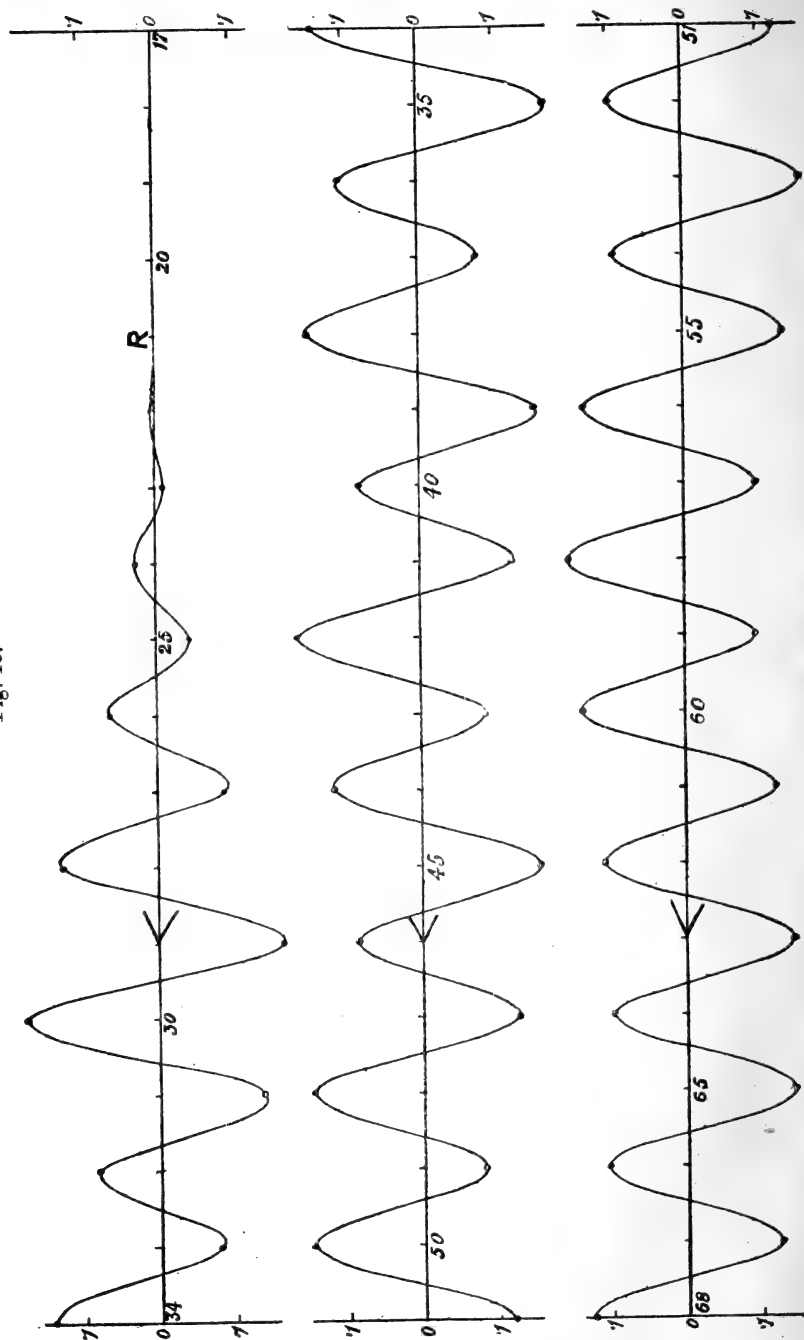
Q(x, 0), and a portion of the curve of sines which very approximately agrees with it at great leftward distances.

Fig. 9.

Head and front of rightward procession.

Fig. 10.

Tail and rear of leftward procession.

§ 28. For fig. 10, instead of assuming as in (47) the calcu-
lation of $Q(x, t)$ for negative values of x, a very troublesome
affair, we may now evaluate it thus. We have by (46)

$$Q(x, t) = \tfrac{1}{2}\phi(x, t) - \phi(x+1, t) + \phi(x+2, t) - \ldots$$

$$Q(-x, t) = \tfrac{1}{2}\phi(-x, t) - \phi(-x+1, t) + \phi(-x+2, t) - \ldots$$

Hence

$$\left.\begin{aligned}Q(x,t) + Q(-x, t) = \phi(x, t) - \phi(x+1, t) + \phi(x+2, t) - \ldots\\ - \phi(-x+1, t) + \phi(-x+2, t) - \ldots\end{aligned}\right\} \quad (54).$$

Now by the property of ϕ, used in the first term of (54),
that its value is the same for positive and negative values
of x, we have $\phi(-x+i, t) = \phi(x-i, t)$. Hence (54) may be
written

$$Q(x, t) + Q(-x, t) = \sum_{i=-\infty}^{i=+\infty} (-1)^i \phi(x+i, t) = P(x, t) \ . \ (55).$$

Hence

$$Q(-x, t) = P(x, t) - Q(x, t) \ . \quad . \quad . \quad (56).$$

Using this in (47) we find

$$\tfrac{1}{2} P(x, t) - Q(x, t) \quad . \quad . \quad . \quad . \quad (57),$$

for the elevation of the water due to the leftward procession
alone at any point at distance x from O on the left side, x
being any positive number, integral or fractional. Having
previously calculated $Q(x, t)$ for positive integral values of x,
we have found by (57) the calculated points of fig. 10 for the
leftward procession.

§ 29. The principles and working plans described in
§§ 11–28 above, afford a ready means for understanding and
working out in detail the motion, from $t=0$ to $t=\infty$, of a
given finite procession of waves, started with such displace-
ment of the surface, and such motion of the water below the
surface, as to produce, at $t=0$, a procession of a thousand or
more waves advancing into still water in front, and leaving
still water in the rear. To show the desired result gra-
phically, extend fig. 10 leftwards to as many wave-lengths as
you please beyond the point, I, described in § 24. Invert
the diagram thus drawn relatively to right and left, and fit it
on to the diagram, fig. 9, extended rightwards so far as to
show no perceptible motion ; say to $x=200$, or 300, of our
scale. The diagram thus compounded represents the water

surface at time 25τ after a commencement correspondingly compounded from fig. 8, and another similar figure drawn to represent the rear of the finite (two-ended) procession which we are now considering.

§ 30. Direct attack on the problem thus indirectly solved, gives, for the case of 1000 wave-crests in the beginning, the following explicit solution,

$$-\zeta = \sum_{i=0}^{i=2000} (-1)^{i} \psi(x-i, t) \quad . \quad . \quad . \quad (58),$$

where ψ is a function found according to the principles indicated in § 4 above, to express the same surface-displacement as our function ϕ of § 12, and the proper velocities below the surface to give, in the sum, a rightward procession of waves. Our present solution shows how rapidly the initial sinusoidality of the head and front of a one-ended infinite procession, travelling rightwards, is disturbed in virtue of the hydrokinetic circumstances of a procession invading still water. Our solution, and the item towards it represented in figs. 6 and 7, and in fig. 2 of § 6 above, show how rapidly fresh crests are formed. The whole investigation shows how very far from finding any definite "group-velocity" we are, in any initially given group of two, three, four, or any number, however great, of waves. I hope in some future communication to the Royal Society of Edinburgh to return to this subject in connexion with the energy principle set forth by Osborne Reynolds [*], and the interferential theory of Stokes [†] and Rayleigh [‡] giving an absolutely definite group-velocity in their case of an infinite number of mutually supporting groups. But my first hydrokinetic duty, the performance of which I hope may not be long deferred, is to fulfil my promises regarding ship-waves, and circular waves travelling in all directions from a place of disturbance in water.

§ 31. The following tables show some of the most important numbers which have been calculated, and which may be useful in farther prosecution of the subject of the present paper.

[*] Nature, vol. xvi. 1877, pp. 343–344.
[†] Smith's Prize Paper, Camb. Univ. Calendar, 1876.
[‡] 'Sound,' ed. 1, vol. i. 1877, pp. 246–247.

TABLE I.

$$t = 0 \; ; \quad \phi(x, 0) = \frac{\sqrt{(\rho + z)}}{\rho} \; ; \quad \mathrm{D}(x, 0) = \phi(x, 0) - \phi(x+1, 0).$$

x.	$\phi(x, 0)$.	$\dfrac{\mathrm{D}(-1, 0) = -\mathrm{D}(0, 0)}{\mathrm{D}(x, 0)}$.	x.	$\phi(x, 0)$.	$\dfrac{\mathrm{D}(-1, 0) = -\mathrm{D}(0, 0)}{\mathrm{D}(x, 0)}$.
0	1·4142	·3155	34	·1740	·0026
1	1·0987	·2942	35	·1714	·0025
2	·8045	·1593	36	·1689	·0023
3	·6452	·0962	37	·1666	·0023
4	·5490	·0647	38	·1643	·0022
5	·4843	·0468	39	·1621	·0021
6	·4375	·0357	40	·1600	·0020
7	·4018	·0284	41	·1580	·0019
8	·3734	·0232	42	·1561	·0019
9	·3502	·0194	43	·1542	·0018
10	·3308	·0166	44	·1524	·0017
11	·3142	·0143	45	·1507	·0017
12	·2999	·0125	46	·1490	·0016
13	·2874	·0111	47	·1474	·0016
14	·2763	·0100	48	·1458	·0015
15	·2663	·0089	49	·1443	·0015
16	·2574	·0081	50	·1428	·0014
17	·2493	·0073	51	·1414	·0014
18	·2420	·0068	52	·1400	·0014
19	·2352	·0062	53	·1386	·0013
20	·2290	·0058	54	·1373	·0013
21	·2232	·0053	55	·1360	·0012
22	·2179	·0050	56	·1348	·0012
23	·2129	·0047	57	·1336	·0012
24	·2082	·0043	58	·1324	·0011
25	·2039	·0041	59	·1313	·0011
26	·1998	·0039	60	·1302	·0011
27	·1959	·0036	61	·1291	·0011
28	·1923	·0035	62	·1280	·0010
29	·1888	·0033	63	·1270	·0010
30	·1855	·0031	64	·1260	·0010
31	·1824	·0029	65	·1250	·0010
32	·1795	·0028	66	·1240	·0009
33	·1767	·0027	67	·1231	·0009

TABLE II.

$$t=25\tau;\ \tau=\sqrt{\pi};\ \chi=\tan^{-1}\frac{x}{z};\ \phi=\sqrt{\frac{2}{\rho}}\cos\left[x\left(\frac{25}{\rho}\right)^2\pi-\frac{1}{2}\chi\right]\epsilon^{-\left(\frac{25}{\rho}\right)^2\pi}$$

$x.$	$\dfrac{xt^2}{\rho^2}+90°-\frac{1}{2}\chi.$	$\epsilon^{\frac{-t^2}{\rho^2}}.$	$\phi(x,25\tau).$	$D(x,25\tau).$
15	$41\pi+133°\ 43'$	·0002	·0000	+·0001
16	$39\pi+\ 30°\ 41'$	·0005	−·0001	−·0002
17	$36\pi+161°\ 31'$	·0011	+·0001	−·0002
18	$34\pi+157°\ 22'$	·0024	+·0003	+·0006
19	$33\pi+\ 11°\ \ 3'$	·0044	−·0003	+·0020
20	$31\pi+\ 77°\ 25'$	·0075	−·0023	−·0018
21	$29\pi+171°\ 23'$	·0118	−·0005	−·0055
22	$28\pi+109°\ 24'$	·0174	+·0050	+·0117
23	$27\pi+\ 68°\ 20'$	·0246	−·0067	−·0136
24	$26\pi+\ 45°\ 33'$	·0333	+·0069	+·0146
25	$25\pi+\ 39°\ \ 6'$	·0434	−·0077	−·0188
26	$24\pi+\ 46°\ 38'$	·0550	+·0111	+·0281
27	$23\pi+\ 67°\ \ 0'$	·0679	−·0170	−·0386
28	$22\pi+\ 98°\ 45'$	·0820	+·0216	+·0377
29	$21\pi+140°\ 41'$	·0917	−·0161	−·0101
30	$21\pi+\ 11°\ 47'$	·1131	−·0060	−·0372
31	$20\pi+\ 71°\ 11'$	·1299	+·0312	+·0558
32	$19\pi+138°\ \ 6'$	·1472	−·0246	−·0032
33	$19\pi+\ 31°\ 50'$	·1651	−·0214	−·0626
34	$18\pi+111°\ 49'$	·1832	+·0412	+·0267
35	$18\pi+\ 17°\ 29'$	·2016	+·0145	+·0637
36	$17\pi+108°\ 23'$	·2201	−·0492	−·0266
37	$17\pi+\ 24°\ \ 6'$	·2385	−·0226	−·0713
38	$16\pi+124°\ 14'$	·2569	+·0487	+·0021
39	$16\pi+\ 48°\ 27'$	·2752	+·0466	+·0728
40	$15\pi+156°\ 27'$	·2934	−·0262	+·0425
41	$15\pi+\ 87°\ 58'$	·3112	−·0687	−·0410
42	$15\pi+\ 22°\ 44'$	·3287	−·0277	−·0751
43	$14\pi+140°\ 32'$	·3459	+·0474	−·0290
44	$14\pi+\ 81°\ \ 8'$	·3629	+·0764	+·0434
45	$14\pi+\ 24°\ 24'$	·3794	+·0330	+·0741
46	$13\pi+150°\ \ 6'$	·3956	−·0411	+·0429
47	$13\pi+\ 98°\ \ 9'$	·4112	−·0840	−·0190
48	$13\pi+\ 48°\ 20'$	·4267	−·0650	−·0642
49	$13\pi+\ \ 0°\ 32'$	·4416	−·0008	−·0657
50	$12\pi+134°\ 40'$	·4560	+·0649	−·0282
51	$12\pi+\ 90°\ 36'$	·4702	+·0931	+·0224
52	$12\pi+\ 48°\ 10'$	·4840	+·0707	+·0582
53	$12\pi+\ \ 7°\ 25'$	·4973	+·0125	+·0643
54	$11\pi+148°\ \ 9'$	·5101	−·0518	+·0417
55	$11\pi+110°\ 18'$	·5226	−·0935	+·0035
56	$11\pi+\ 73°\ 48'$	·5348	−·0970	−·0332
57	$11\pi+\ 38°\ 35'$	·5464	−·0638	−·0556
58	$11\pi+\ \ 4°\ 34'$	·5580	−·0082	−·0578
59	$10\pi+151°\ 43'$	·5690	+·0496	−·0421
60	$10\pi+119°\ 57'$	·5797	+·0917	−·0152
61	$10\pi+\ 89°\ 14'$	·5900	+·1069	+·0141
62	$10\pi+\ 59°\ 30'$	·6001	+·0928	+·0373
63	$10\pi+\ 30°\ 43'$	·6098	+·0555	+·0501
64	$10\pi+\ \ 2°\ 50'$	·6193	+·0054	+·0506
65	$9\pi+155°\ 48'$	·6284	−·0452	+·0403
66	$9\pi+129°\ 35'$	·6372	−·0855	+·0226
67	$9\pi+104°\ \ 9'$	·6459	−·1081	+·0022
68	$9\pi+\ 79°\ 28'$	·6540	−·1103	

XLV. *The Electrical Effects produced by the Explosion of Hydrogen and Oxygen.* By C. E. HASELFOOT, *late Fellow of Hertford College, and the* Rev. P. J. KIRKBY, *Fellow of New College, Oxford* *.

THE following experiments were undertaken to investigate the electrical effects which accompany the explosive combination of hydrogen and oxygen ; it having been already shown by preliminary experiments that such effects do exist. The method adopted was to produce an explosion of the mixed gases between two cylinders, of which the inner was a thick wire. On establishing a difference of potential between the wire and the outer cylinder, a flow of electricity to the wire was observed. In order to measure this more accurately, the following apparatus was devised :—

Hydrogen and oxygen were generated by the electrolysis of pure barium hydrate, kindly supplied by Mr. H. B. Baker, in a glass tube A (fig. 1), and were admitted to a vessel B
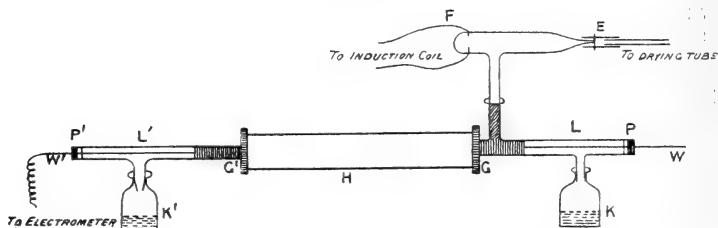
Fig. 1.
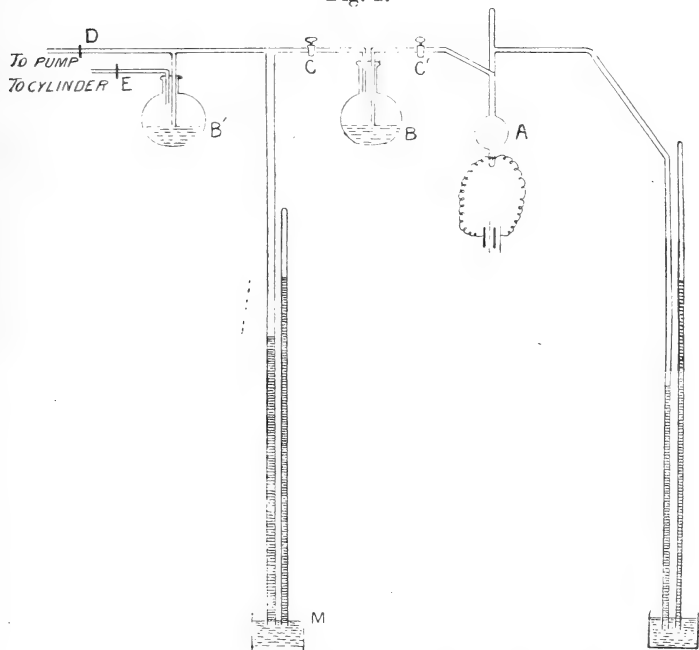
containing phosphorus pentoxide. From this they could be passed through a stopcock C, either to a Fleuss pump through

pressure-tubing, which could be pinched at D, or through a
drying vessel B′ and a piece of pressure-tubing, which could
be pinched at E, into the apparatus represented by fig. 2.

Fig. 2.

This consisted of an aluminium cylinder H, 26·5 cm. long,
41·5 mm. internal diameter, closed by ebonite ends into which
brass tubes G, G′ were fitted. To these were fastened by
means of ebonite plugs two glass tubes L, L′, each of which
formed a kind of **T**-piece, the lower branches fitting into two
removable glass bottles K, K′ containing phosphorus pent-
oxide. By this means the phosphorus pentoxide could be
easily renewed without taking the apparatus to pieces. A
brass wire, of 3·25 mm. diameter, was fastened along the
common axis of the cylinder and tubes, resting in ebonite
plugs P, P′, and was connected to one pair of quadrants of
an electrometer. The apparatus was completed by a piece
of glass-tubing, fitting into the pressure-tubing E, and fastened
by elastic glue into a short brass tube soldered into the tube
G. Platinum wires were fused into the glass-tubing at F,
and could be connected to the terminals of an induction-coil.
Thus the gas could be exploded. By alternately pumping
out the apparatus and filling it with dried hydrogen and
oxygen the pressure of air in the cylinder could be reduced
to a fraction of a millimetre.

The course of an experiment was as follows :—The hydrogen
and oxygen to be exploded were admitted to the required
pressure, the pinch-cock E was closed, and the pressure p_0
inside the cylinder was read ; G and G′, dividing the insu-
lations between the cylinder H and the wire W, were then
earthed, and the cylinder was charged to any desired potential
by connecting it to one terminal of a battery, the other
terminal of which was to earth. The field of force having
been established, the quadrants connected to the wire W,
which had previously been earthed, were insulated, and no
deflexion of the electrometer was observed. The gases were
then exploded and the quantity of electricity thrown on to

the wire was determined by an observation of the electro-
meter in the way fully described below. Finally the
pinchcock E was opened and the pressure read on the
manometer. From these observations the fall of pressure
caused by the explosion is easily deduced, when the ratio of
the volume, V, of the apparatus of fig. 2, in which the
explosion takes place, to the volume, *v*, of the apparatus in
fig. 1 between the stopcocks C, D, and E is known. This
ratio was determined as 1·4 in the apparatus used.

It was found in the preliminary experiments that the effect
of the explosions was to break down the insulations, owing,
no doubt, to water-vapour being thrown on to the insulators.
It was to obviate this that the bottles K, K′ were designed,
the phosphorus pentoxide in which served the double purpose
of protecting the insulators and of absorbing the water-
vapour. P, P′ were further protected by small circular
ebonite shields placed on the wire, as shown in the figure,
close to, but not touching, the glass tubes. These shields,
in addition to intercepting the water-vapour, screened the
insulators from fragments of phosphorus pentoxide, which
were in some cases blown into the cylinder by the explosion.

The preliminary experiments had led us to believe that
the electricity produced was a small charge, which could be
best measured by the deflexions of an electrometer connected
with a condenser of small capacity; but with the improved
apparatus it was found that a condenser of the order of a
microfarad was required. The following was the method
adopted for measuring the quantity of electricity generated.

Fig. 3.
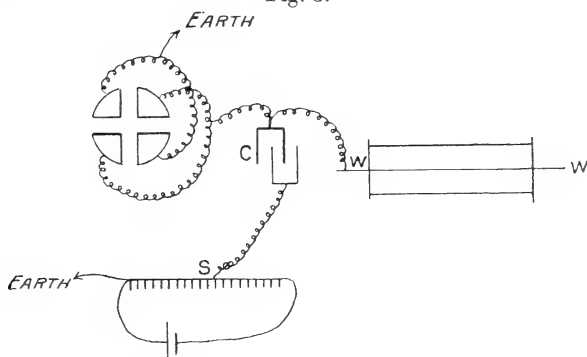
The quadrants in metallic communication with the wire W
were connected with one terminal of a condenser C, the other
terminal of which was connected with the slider of a high-
resistance potentiometer, as shown in fig. 3. The electrodes

of the latter were attached by wires to the poles of a battery of two volt accumulators, one of the poles, of course, being put to earth.   Immediately before the explosion the quadrants were insulated, and on the explosion taking place the electrometer-needle was brought back to its zero by moving the slider S of the potentiometer from the point of zero potential along the potentiometer-scale.   When this has been done the potential of one terminal of the condenser is zero, of the other, V, the potential shown by the potentiometer.   The charge on the condenser is therefore CV, where C is the capacity of the condenser, and this is the quantity of electricity thrown on to the wire W.   This method greatly extended the range of the electrometer, which was of the Dolezalek pattern, in measuring quantity.   Moreover, by thus employing a null method the leak of electricity on the electrometer was rendered negligible.   A slight leak was observed in the condenser, but it was possible to read the potential before the loss due to this cause had time to become appreciable.   It was verified that no effect was produced on the electrometer, either by the spark when no explosion took place, or by an explosion when the wire and cylinder were at the same potential.   It follows, therefore, that the charge acquired by the wire is due to the action of the electric field in collecting the ions generated by the explosion.   Under the action of the field, if the cylinder is kept at a positive potential, the positive ions are driven to the wire, the negative to the cylinder, and the wire acquires a positive charge.   If the cylinder is kept at a negative potential, the reverse effect takes place ; but the phenomenon might be further complicated owing to the generation of ions by collision, as the negative ions pass through the intense field of force around the wire.   For this reason the cylinder was kept at a positive potential.

It was found that the explosion would not take place if the pressure of the combined gases was much below 80 mm., and that explosions of the gases at a pressure above 100 mm. endangered the air-tight joints of the apparatus.   The experiments were therefore confined to this range of pressures.

The results are shown in the following table, where :—

p = the pressure in mm. of the mixed gases before the explosion,

p' = the fall of pressure in mm. resulting from the explosion and the absorption of the water-vapour formed by it,

p'' = the pressure in mm. of the unexploded gas (viz. $p'' = p - p'$),

Q = the quantity of electricity in micro-coulombs thrown on to the wire W,

q = the quantity in micro-coulombs per mm. fall of pressure ($= Q/p'$),

V = The (positive) potential in volts of the outer cylinder, that of the wire W being initially zero.

TABLE I.

p.	p'.	p''.	V.	Q.	q.
74·5	68·5	6	−41	1·84	·027
87	81	6	41	1·7	·021
77	72	5	124	1·45	·02
78·5	63	15·5	206	·46	·0073
81	74	7	206	·5	·0068
80	60	20	41	·24	·004
96	90	6	41	·45	·005
103	100	3	82	·82	·0082
87·5	82	5·5	206	·587	·0072
79·5	59·5	20	206	·231	·0039

The amount of air present was 41 mm. in the last experiment; in the others it was less than 1 mm.

The capacity of the condenser used varied from one-fifth of a microfarad to a microfarad, the potential to which it was raised lay between a quarter of a volt and two volts. The charge observed was therefore much larger than had been anticipated from the preliminary experiments, ranging, as it did, from a quarter of a microcoulomb to nearly two microcoulombs, and the electrometer was accordingly discarded in favour of a ballistic d'Arsonval galvanometer. To measure the charge the wire W was connected with one terminal of the galvanometer, the other terminal being put to earth. Thus the whole charge passed through the galvanometer and was measured ballistically. The following are the results, in which Table II. refers to a low-resistance galvanometer,

TABLE II.

p.	p'.	p''.	π.	V.	Q.	q.
86	79	7	7	240	11·4	·144
86·5	82	4·5	5	240	4·4	·054
88	84	4	7	240	3·1	·037

TABLE III.

p.	p'.	p''.	π.	V.	Q.	q.
86	83	3	·2	83	3·2	·038
84·5	80	4·5	·25	83	·61	·0076
83·5	76	7·5	2·2	83	·78	·0103
85·5	78	7·5	1	83	·53	·0068
85	82·5	3·5	1	83	1·14	·014
85	80	5	·006	83	1·11	·014
91	78·5	12·5	9	81	·97	·012
91	75	16	8	81	·7	·0093
90	73	17	17	81	2·42	·033
92	69	23	17	81	·7	·01
91	72	19	15	240	·94	·013
100	88·5	11·5	·06	240	·61	·0069
100	82·5	17·5	16	240	·88	·011
94	71·5	22·5	29	240	2·3	·032
94	65	29	44	240	·93	·014

Table III. to a more sensitive one. In the former case it was found that a microcoulomb produced a throw of 6·5 divisions, in the latter a throw of 36 divisions. The notation is as before, and π is the pressure of the air, so that the initial pressure is $p + \pi$.

Five other observations were taken, but they are not reproduced, as the deflexions ranged from only 5 to $8\frac{1}{2}$ divisions on the galvanometer-scale, and the percentage error in reading may have been considerable. They are, however, of some interest, since they are fairly consistent, and they give values of q whose mean is ·012. For comparison with previous values this may be taken as ·01, as explained later.

Ten other observations were taken. In three of these the deflexion was very small and not more than ·04 mm. of air was present; they give values of q whose mean is ·0027. In five others the amount of air was small, probably not more than a millimetre. They give a mean value of q equal to ·0057. The remaining two were made with 25 mm. and 26 mm. of air and 92 mm. of the combined gases. The first produced a deflexion beyond the limits of the scale. This corresponds to a value of q of the order obtained in the first experiment of Table II. The other gave a value of q equal to ·042.

These tables exhibit very great differences in the quantity of electricity produced by the explosion, far beyond the possible limits of experimental errors, and these are most

clearly marked, even in the cases where special care had been taken to reproduce the same conditions as far as possible. It was thought that this might be partly due to the explosion of the gas taking place to a large extent outside the field of force, for the unexploded gases were estimated to amount to between 5 and 10 per cent. of the gases originally present; and it will be seen, on referring to fig. 2, that the spark which originated the explosions was entirely outside the cylinder. It also seemed possible that moisture caused by an explosion might affect the subsequent experiments. With the object of testing these points, a new apparatus was devised. The arrangements shown in fig. 1 were precisely the same as before, but the apparatus of fig. 2 was replaced by a brass cylinder 32·2 cm. long and 3·75 cm. internal diameter, closed by ebonite ends, to which the wire constituting, as before, the inner cylinder was directly fixed. The diameter of this wire was 3·18 mm. The tubes and drying-bottles external to the cylinder were abolished, and instead new arrangements were substituted for exploding the gas well within the field of force and for drying the cylinder after explosion. The gas was exploded by means of a thin platinum wire placed within the cylinder and soldered to two brass rods which protruded through one of the ebonite ends of the cylinder, and which could be connected to the poles of a battery. The platinum wire could thus be heated to incandescence and the gas could be exploded. Practically all the gas which exploded with this arrangement lay within the field of force.

It was verified that when no explosion occurred the heating of the wire produced no electrical effect which could be measured by the ballistic galvanometer.

The cylinder was dried after an explosion by warming it and passing dry air through it. This was done by means of two glass tubes fastened by air-tight joints to the ebonite ends of the cylinder and communicating by means of pieces of pressure-tubing, which could be clipped, one with tubes containing calcium chloride and phosphorus pentoxide to dry the air as it entered, the other with a water-pump. The latter tube was disconnected from the pump before making an experiment and joined to the apparatus of fig. 1, so that by its means the cylinder was filled with air or hydrogen and oxygen as might be required in the ordinary way. It was hoped in this way, by confining the explosion within the field of force, and by drying the cylinder after each experiment, to secure agreement between experiments made under

the same conditions of pressure. This hope was not realized, as will be seen from the following table of results:—

TABLE IV.

p.	p'.	p''.	π.	V.	Q.	q.
80	60	20	3	83	2·3	·038
84	63	21		83	·55	·0087
88	68	20		83	·305	·0045

The last observation is the mean of four experiments in each of which the field of force was identical and the amount of the mixed gases varied by only 1·5 mm. The deflexions obtained were all small, giving values of q ranging from ·0034 to ·0064. Except in the first experiment of the Table, the amount of air present was less than 1 mm. It appeared, too, that a smaller percentage of the gas was exploded than with the previous apparatus. But some uncertainty arises from the difficulty of estimating the amount of water-vapour formed, since it diffused slowly through the pressure-tubing which led to the pentoxide of phosphorus.

With regard to the experiments generally, there are two quantities which it is sought to measure, the quantity of gas exploded and the amount of electricity formed. We consider that the experimental errors may be estimated at 5 per cent. in each case. The chief difficulty in measuring the former quantity was to make the proper allowance for the water-vapour formed by the explosion which is not absorbed immediately by the phosphorus pentoxide. It was observed that the pressure p diminished by as much as 3 mm. when the gases were left undisturbed for a considerable time. A neglect of this would involve an error of about 8 per cent. in the fall of pressure as calculated above. Accordingly, suitable corrections were applied to the pressures observed immediately after opening the pinchcock E, the effect of which would be that errors exceeding 4 per cent. could scarcely occur. This estimate, however, does not apply to Table IV.

In measuring the charge of electricity there was the possibility of a leak of electricity across the ebonite plug through which the wire passed, and of a misreading of the instrument. The former was obviated in the case of the electrometer-observations in the manner already described—in the case of

the galvanometer the discharge was instantaneous, and any leak sufficiently serious to affect the reading of the first throw would have given rise to a steady current which could not have escaped detection. There was, however, greater possibility of an error in reading the deflexion, especially when it consisted of but few divisions; but the observations in which there was reason to suspect a greater error than 5 per cent. have not been given in the tables.

In comparing the values of q, allowance must be made for the fact that in the first six experiments of Table III., and in those of which the mean is given after Table II., shields were placed round the tubes G, G', L, L', outside the cylinder and raised to the same potential with the cylinder, with the object of collecting a greater charge. The volume of the field of force was thus increased from 356 c.c. to 447 c.c., and consequently the values of q, had the shields not been present, would have been approximately four-fifths of the values actually given. But after making every allowance, some other cause must be sought for to explain the large discrepancies.

One fact seems to us to stand out very prominently, namely, the number of cases in which the electrical effects obtained in an experiment made when the contents of the cylinder were probably dry, has very largely exceeded the effects given by experiments repeated very soon afterwards under conditions as far as possible the same. This may mean that the presence of moisture impedes the formation of ions very seriously. Thus, in the case of the electrometer, the first three experiments give values two to three times as great as were obtained afterwards. In Tables II. and III. the deflexion first obtained is largely in excess of those which immediately follow. In Table III. the ninth experiment and the two which succeed it were made at the same time, and the charge measured in the second experiment with the last form of apparatus is notably less than that given by the first. On the other hand, prolonged attempts at drying, short of actually taking down the apparatus, failed to give more than a fraction of the maximum numbers obtained, and in some cases, notably when much air was present, greater values were found, when there was probably a considerable amount of moisture, than had been anticipated. Still, on the whole, we think that the presence of water-vapour checks the formation of ions, that when the cylinder and gases are perfectly dry the electricity generated per c.c. exploded at a pressure of 80 mm. may be as large as ·032 microcoulomb, but that when much moisture is present the amount may

be reduced to as little as one fiftieth of this value. This result is got by dividing the largest value of Q obtained, 11·4, given by the first experiment with the low-resistance galvanometer, by 356, the number of c.c. in the cylinder. It appears that an increase of electric force produces little or no effect, and that therefore, presumably, all the ions are collected when the outer cylinder is at 82 volts. This conclusion was arrived at from the experiments of Table I.

There is not sufficient evidence to show whether the presence of air produces an effect, but the experiments given in Table III., in which the quantity of air was varied, seem to point to the presence of air causing some increase in the ionisation. But, if this is so, it is far from proportional to the amount of air present. The matter is further complicated by the diminution in the completeness of the explosion when the amount of air is considerable. This is shown in Table III.

The chief interest of these results lies, perhaps, in their bearing on the question whether the production of molecules of water-vapour from atoms of hydrogen and oxygen is accompanied by the formation of a comparable number of ions. The number of molecules of water-vapour formed is the number of molecules of hydrogen which enter into combination. Hence the number formed per c.c. for each millimetre fall of pressure of the mixed gases is $\dfrac{2}{3} \cdot \dfrac{N}{760} = \dfrac{N}{1140}$, where N is the number of molecules per c.c. at 760 mm. pressure and 15° C. The number of coulombs produced per c.c. is $\dfrac{q \times 10^{-6}}{v}$, where v c.c. is the volume of the cylinder and q has the same meaning as in the tables, or $\dfrac{3000\,q}{v}$ electrostatic units. If then e is the electrostatic charge on an ion, the number of pairs of ions produced bears to the number of molecules formed the ratio

$$\frac{3000\,q}{ve} \times \frac{1140}{N} = 2 \cdot 8 \times 10^{-4}\,\frac{q}{v},$$

since $Ne = 1 \cdot 22 \times 10^{10}$ (J. S. Townsend, Phil. Trans. 1899). Taking the largest value of q given by the experiments, ·144, and $v = 356$ c.c., the ratio becomes approximately $1 \cdot 1 \times 10^{-7}$. It would appear then that about ten million molecules of water-vapour are formed for every pair of ions produced, and that, too, when nearly 10 per cent. of the mixture is air. On the other hand, it is quite possible that a more suitable apparatus would produce greater electrical

effects, as the large discrepancies seem to show that the limit has not been reached. Nevertheless, we think it highly improbable that these electrical effects are a direct result of chemical action, but believe rather that they are a secondary effect due to the enormous amount of heat set free by chemical union.

It can, in fact, be shown that the energy required for the formation of the ions is quite insignificant in comparison with that produced by the combination of hydrogen and oxygen. For the heat evolved when one gram of hydrogen enters into combination is 34,000 calories. In other words, when v c.c. of hydrogen and oxygen are exploded at p mm. pressure, $34{,}000 \times \dfrac{2p}{3 \times 760} \times v\rho \times 4 \cdot 2$ joules are formed, where ρ is the density of hydrogen at 15° C. and 760 mm. pressure. This equals $1 \cdot 06 \times 10^{-2} \times p\,v$ joules. Again, to produce Q coulombs of electricity of one sign in a gas by ionisation, $\dfrac{Q}{e'}$ pairs of ions must be generated where e' is the charge on an ion measured in coulombs, and this will not require more than $5\,Q$ joules, since the energy necessary for the formation of each pair does not exceed $5\,e'$ joules (J. S. Townsend, Phil. Mag. Feb. 1901, p. 220). In our experiments p was about 80 mm., v 356 c.c. and Q not greater than 12×10^{-6} coulombs. The heat set free was therefore approximately 300 joules, while the energy required for ionisation did not exceed 6×10^{-5} joules, which is only one five millionth of the former quantity.

We wish to express our thanks to Professor Townsend for his valuable suggestions and criticisms.

XLVI. *On the Open Organ-Pipe Problem in Two Dimensions.* By Lord RAYLEIGH, *O.M., F.R.S.*[*]

IN the usual symmetrical organ-pipe of radius R, supposed to be provided at the mouth with an infinite flange, we know that the correction (α) that must be added to the length in order that the open end may be treated as a loop, lies between $\frac{1}{4}\pi R$ and $8R/3\pi$. The wave-length of vibration is here supposed to be very great, so that in the neighbourhood of the mouth the flow follows the electrical law. If we use this analogy and regard the walls of the pipe and the flange as non-conductors, the question is one of the *resistance* of the air-space, measured from a section well inside the pipe to an infinite distance beyond the mouth. And in spite

of the extension to infinity this resistance is finite. For if r be the radius of a large sphere whose centre is at the mouth, the resistance between r and $r + dr$ is $dr/2\pi r^2$; and the part corresponding to the passage from a sufficiently great value of r outwards to infinity may be neglected.

A parallel treatment of the problem in *two dimensions,* where inside the mouth the boundary consists of two parallel planes, appears to fail. The resistance to infinity, involving now $\int^\infty r^{-1} dr$, instead of $\int^\infty r^{-2} dr$, has no finite limit; and we must conclude that when the wave-length (λ) is very great, the correction to the length becomes an infinite multiple of the width of the pipe. But it remains an open question how the correction to the length compares with λ,—whether, for example, when λ is given it would vanish when the width of the pipe is indefinitely diminished.

The following consideration suggests an affirmative answer to the last question. If we start with a pipe of circular form and suppose the section, while retaining its area, to become more and more elliptical, it would appear that the correction to the length must continually diminish. But the question has sufficient interest to justify a more detailed treatment.

In *Theory of Sound*, § 302, the problem is considered of the reaction of the air upon the vibratory motion of a circular plate forming part of an infinite and otherwise fixed plane. For our present purpose the circular plate is to be replaced by an infinite rectangular strip extending from $y = -\infty$ to $y = +\infty$, and in the other direction of width x. If $d\phi/dn$ be the given normal velocity of the element dS of the plane and $k = 2\pi/\lambda$,

$$\phi = -\frac{1}{2\pi} \iint \frac{d\phi}{dn} \frac{e^{-ikr}}{r} \, dS \quad . \quad . \quad . \quad (1)$$

gives the velocity-potential at any point P distant r from dS. In the present case $d\phi/dn$ is constant, where it differs from zero, and accordingly may be removed from under the integral sign. If a denote the velocity of sound, ϕ varies as e^{ikat}, and if σ be the density, we get for the whole variation of pressure acting upon the plate

$$\iint \delta p \, dS = -\sigma \iint \dot\phi \, dS = -ika\sigma \iint \phi \, dS.$$

Thus by (1)

$$\iint \delta p \, dS = \frac{ika\sigma}{\pi} \frac{d\phi}{dn} \Sigma\Sigma \frac{e^{-ikr}}{r} dS \, dS'. \quad . \quad . \quad (2)$$

In the double sum

$$\Sigma\Sigma \frac{e^{-ikr}}{r} dS\, dS', \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which we have now to evaluate, each pair of elements is to be taken *once* only, and the product is to be summed after multiplication by the factor $r^{-1}e^{-ikr}$, depending on their mutual distance. The best method is that suggested by Maxwell for the common potential. The quantity (3) is regarded as the work that would be consumed in the complete dissociation of the matter composing the plate, that is to say, in the removal of every element from the influence of every other, on the supposition that the potential of two elements is proportional to $r^{-1}e^{-ikr}$. The amount of work required, which depends only on the initial and final states, may be calculated by supposing the operation performed in any way that may be most convenient. For this purpose we suppose that the plate is divided into elementary strips, and that on one side external strips are removed in succession.

To carry out this method we require first an expression for the potential (V) at the edge of a strip. Here,

$$V = \int_0^x \int_{-\infty}^{+\infty} \frac{e^{-ikr}}{r} dx\, dy. \quad . \quad . \quad . \quad (4)$$

where $r = \sqrt{(x^2 + y^2)}$; and therein

$$\int_{-\infty}^{+\infty} \frac{e^{-ikr}}{r} dy = 2 \int_x^\infty \frac{e^{-ikr}}{y} dr = 2 \int_1^\infty \frac{e^{-ikcv}}{\sqrt{v^2-1}} dv, \quad . \quad (5)$$

representing the potential of a linear source at a point distant x from it. Convergent and semi-convergent series for (5), applicable respectively when x is small and when x is great, are well known.

We have

$$\int_1^\infty \frac{e^{-ikcv} dv}{\sqrt{(v^2-1)}} = \left(\frac{\pi}{2ikx}\right)^{\frac{1}{2}} e^{-ikx} \left\{ 1 - \frac{1^2}{1 \cdot 8ikx} + \frac{1^2 \cdot 3^2}{1 \cdot 2 \cdot (8ikx)^2} - \dots \right\}$$

$$= -\left(\gamma + \log \frac{ikx}{2}\right) \left\{ 1 - \frac{k^2 x^2}{2^2} + \frac{k^4 x^4}{2^2 \cdot 4^2} - \dots \right\}$$

$$- \frac{k^2 x^2}{2^2} S_1 + \frac{k^4 x^4}{2^2 \cdot 4^2} S_2 - \frac{k^6 x^6}{2^2 \cdot 4^2 \cdot 6^2} S_3 + \dots , \quad (6)$$

where

$$S_m = 1 + \tfrac{1}{2} + \tfrac{1}{3} + \dots + 1/m, \quad . \quad . \quad . \quad (7)$$

and γ is Euler's constant (·5772…). A simple method of derivation, adequate so far as the leading terms are concerned, will be found in Phil. Mag. xliii. p. 259, 1897 (Scientific Papers, iv. p. 290).

Confining ourselves for the present to the case where the total width of the strip is small compared with the wavelength, we have to integrate the second series in (6) with respect to x, for which purpose we have the formula

$$\int x^n \log x \, dx = \frac{x^{n+1}}{n+1}\left\{\log x - \frac{1}{n+1}\right\}. \quad . \quad . \quad (8)$$

In this way we obtain V, the potential at the edge of the strip of width x. Afterwards we have to integrate again with respect to y and x. The integration with regard to y introduces simply the factor y, representing the (infinite) length of the strip. The integration with respect to x is again to be taken between the limits 0 and x. Thus

$$\iint V \, dx \, dy = -yx^2\{\gamma - \tfrac{3}{2} + \log(\tfrac{1}{2}ikx)\}, \quad . \quad . \quad (9)$$

terms in x^4 being omitted. This is the equivalent of (3) in the case of an infinite strip of length y and width x. Accordingly by (2)

$$\iint \delta p \, dS = -\frac{ika\sigma}{\pi}\frac{d\phi}{dn} . yx^2\{\gamma - \tfrac{3}{2} + \log(\tfrac{1}{2}kx) + \tfrac{1}{2}i\pi\}. \quad (10)$$

The reaction of the air upon the plate may be divided into two parts, of which one is proportional to the velocity of the plate and the other to the acceleration. If ξ denote the displacement of the plate at time t, so that $d\xi/dt = d\phi/dn$, we have

$$\frac{d^2\xi}{dt^2} = ika\frac{d\xi}{dt} = ika\frac{d\phi}{dn};$$

and therefore in the equation of motion of the plate, the reaction of the air is represented by a dissipative force

$$xy . \frac{d\xi}{dt} . \frac{a\sigma}{2} . kx. \quad . \quad . \quad . \quad . \quad (11)$$

retarding the motion, and by an accession to the inertia equal to

$$xy . \frac{\sigma}{\pi} . x\left(\tfrac{3}{2} - \gamma - \log\frac{kx}{2}\right), \quad . \quad . \quad (12)$$

the first factor in each case denoting the area of the plate. The mass represented by (12) is that of a volume of air having a base equal to the area of the plate and a *thickness*

$$\frac{x}{\pi}\left(\tfrac{3}{2} - \gamma - \log\frac{kx}{2}\right). \quad . \quad . \quad . \quad (13)$$

When x is given (13) increases without limit when $\lambda(=2\pi/k)$ is made infinite, as we found before. But if we

regard λ as given, and suppose x (the width of the plate) to diminish without limit, we see that (13) also diminishes without limit.

The application of these results to the problem of the open pipe in two dimensions depends upon the imaginary introduction of a movable piston, itself without mass, at the mouth of the pipe—a variation without influence upon the behaviour of the air at a great distance outside the mouth, with which we are mainly concerned. The conclusion is that if the wave-length or pipe-length be given, the mouth or open end may be treated more and more accurately as a loop as the width is diminished without limit. Both parts of the pressure-variation, corresponding the one to inertia and the other to dissipative escape of energy, ultimately vanish.

So far we have considered the case where in (3), (4) the width of the strip is *small* in comparison with the wave-length. It remains to say something as to the other extreme case; and it may be well to introduce the discussion by a brief statement of the derivation of the semi-convergent series in (6) by the method of Lipschitz *.

Consider the integral $\int \dfrac{e^{-rw}dw}{\sqrt{(1+w^2)}}$, where w is a complex variable of the form $u+iv$. If we represent, as usual, simultaneous pairs of values of u and v by the coordinates of a point, the integral will vanish when taken round any closed circuit not including the points $w = \pm i$. The circuit at present to be considered is that enclosed by the lines $u = 0$, $v = 1$, and a quadrant at infinity. It is easy to see that along this quadrant the integral ultimately vanishes, so that the result of the integration is the same whether we integrate from $w = i$ to $w = i\infty$, or from $w = i$ to $w = \infty + i$. Accordingly

$$\int_1^\infty \frac{e^{-irv}dv}{\sqrt{(v^2-1)}} = \int_0^\infty \frac{e^{-ir}e^{-ru}du}{\sqrt{(2iu+u^2)}} = \frac{e^{-ir}}{\sqrt{(2ir)}}\int_0^\infty \frac{e^{-\beta}\beta^{-\frac{1}{2}}d\beta}{\sqrt{\left(1+\dfrac{\beta}{2ir}\right)}}$$

$$= \left(\frac{\pi}{2ir}\right)^{\frac{1}{2}}e^{-ir}\left\{1 - \frac{1^2}{1\cdot 8ir} + \frac{1^2\cdot 3^2}{1\cdot 2\cdot(8ir)^2} - \frac{1^2\cdot 3^2\cdot 5^2}{1\cdot 2\cdot 3\cdot(8ir)^3} + \dots\right\}, \quad (14)$$

on expansion and integration by a known formula. This agrees with (6), if kx be written for r.

In arriving at the value of $\iint V\,dx\,dy$, we have to integrate (5) twice with respect to x between the limits 0 and x.

* *Crelle*, Bd. lvi. (1859). See also Proc. Lond. Math. Soc. xix. p. 504 (1888); Scientific Papers, iii. p. 44.

Taking first an integration with respect to x, we have

$$\int_0^x dx \int_1^\infty \frac{e^{-ikxv}dv}{\sqrt{(v^2-1)}} = \int_1^\infty \frac{(1-e^{-ikxv})dv}{ikv\sqrt{(v^2-1)}};$$

in which

$$\int_1^\infty \frac{dv}{v\sqrt{(v^2-1)}} = \int \frac{dv}{v^2\sqrt{(1-v^{-2})}} = \frac{\pi}{2}.$$

Again, for the second integration with respect to x,

$$\int_0^x dx \int_1^\infty \frac{e^{-ikxv}dv}{ikv\sqrt{(v^2-1)}} = \int_1^\infty \frac{(1-e^{-ikxv})dv}{(ikv)^2\sqrt{(v^2-1)}},$$

in which

$$\int_1^\infty \frac{dv}{v^2\sqrt{(v^2-1)}} = \int_0^1 \frac{dz}{\sqrt{(z^{-2}-1)}} = 1.$$

Thus

$$\iint V\, dx\, dy = y \left\{ \frac{\pi x}{ik} + \frac{2}{k^2} - \frac{2}{k^2} \int_1^x \frac{e^{-ikxv}dv}{v^2\sqrt{(v^2-1)}} \right\}. \quad (15)$$

When x is great, the outstanding integral in (15) may be treated in the manner already explained. The integral

$$\int \frac{e^{-rw}dw}{w^2\sqrt{(1+w^2)}}$$

will yield the same value, whether taken from $w=i$ to $w=i+i\infty$, or from $w=i$ to $w=\infty+i$. The first gives

$$-\int_1^\infty \frac{e^{-irv}dv}{v^2\sqrt{(v^2-1)}};$$

the second gives

$$\int_0^\infty \frac{e^{-ru}e^{-ir}du}{(u+i)^2\sqrt{(2iu+u^2)}}.$$

Accordingly

$$\int_1^\infty \frac{e^{-irv}dv}{v^2\sqrt{(v^2-1)}} = \frac{e^{-ir}}{\sqrt{(2ir)}} \int_0^\infty \frac{e^{-\beta}\beta^{-\frac{1}{2}}d\beta}{\left(1+\frac{\beta}{ir}\right)^2 \cdot \sqrt{\left(1+\frac{\beta}{2ir}\right)}},$$

and the latter may be expanded in inverse powers of r and integrated as before. For the leading term we have

$$\int_0^\infty e^{-\beta}\beta^{-\frac{1}{2}}\, d\beta = \sqrt{\pi}.$$

Thus approximately when x is great

$$\iint V dx\, dy = y \left\{ \frac{\pi x}{ik} + \frac{2}{k^2} - \frac{2\sqrt{\pi} \cdot e^{-ikr}}{k^2 \sqrt{(2ikx)}} \right\}$$

$$= xy \left\{ -\frac{i\pi}{k} + \frac{2}{k^2 x} \right\}, \quad . \quad . \quad . \quad . \quad (16)$$

if we confine ourselves to the leading real and imaginary terms.

From (2) we now get

$$\iint \delta p\, dS = \frac{ik a\sigma}{\pi} \frac{d\phi}{dn} \cdot xy \cdot \left\{ -\frac{i\pi}{k} + \frac{2}{k^2 x} \right\}$$

$$= xy \cdot a\sigma \cdot \frac{d\xi}{dt} + xy \cdot \frac{2\sigma}{\pi k^2 x} \frac{d^2\xi}{dt^2} \quad . \quad . \quad (17)$$

When x is large, the inertia term ultimately vanishes in comparison with the area of the plate. The reaction is then reduced to the dissipative term, which is the same as would be obtained from the theory of plane waves of infinite extent.

XLVII. *On the Lengthening of the Spark from an Induction-Coil by the Aid of Auxiliary Wires.* By T. J. BOWLKER [*].

IF a discharge is taking place between the ends of two wires connected with the poles of an induction-coil, the ends of the wires being at such a distance that the spark just passes freely, then if an insulated wire be so placed that one end is near the positive terminal, and some other point on the

Fig. 1.
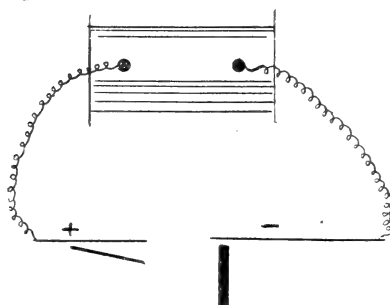
wire is almost touching the wire leading to the positive terminal, the length of the spark is greatly increased (see fig. 1).

A wire similarly placed with regard to the negative terminal has no effect, if the condenser is properly adjusted.

The ratio of the increase depends upon the shape of the terminals, but the actual length of the spark tends towards a maximum almost independent of the shape of the ends.

Also, if an insulated wire be placed at right angles to the negative wire, near its terminal, the length of the spark is similarly increased, to some extent if touching the negative wire, to a greater extent if almost touching; whereas when such a wire, if thick (in the experiments one of 3·4 mm. diameter was used), and with well-rounded ends, is similarly placed with regard to the positive wire, it has no effect on the length of the spark.

Either of these phenomena give us a ready means of determining the nature of the poles of an induction-coil.

The second effect, with the negative terminal, takes place, to a gradually diminishing extent, when the detached wire is at a distance from the terminal, vanishing when the point of application is about three times the length of the spark-gap.

The results of one experiment to show these effects are given below.

Positive terminal of wire ·45 mm. diameter.
Negative terminal of wire 1·60 mm. diameter.

	Length of Spark-gap.
Unassisted	40 mm.
Positive assisted.............	66 „
Negative assisted	65 „

Current reversed.

Unassisted	56 „
Positive assisted.............	67 „
Negative assisted	65 „

The ratio of increase of length of spark varies for different original spark-lengths. This is seen from the following series of experiments between the same terminals :—

					Ratio.
Unassisted spark-length.. 16 mm.			Positive assisted ..	25 mm.	1·56
			Negative assisted..	23	1·44
Unassisted	„	.. 23	Positive assisted ..	39	1·70
			Negative assisted..	36	1·57
Unassisted	„	.. 34	Positive assisted ..	60	1·76
			Negative assisted..	59	1·73
Unassisted	„	.. 40	Positive assisted ..	66	1·65
			Negative assisted..	65	1·62
Unassisted	„	.. 62	Positive assisted ..	89	1·43
			Negative assisted..	86	1·39
Unassisted	„	.. 98	Positive assisted ..	133	1·36
			Negative assisted..	130	1·32
Unassisted	„	..113	Positive assisted ..	135	1·19
			Negative assisted..	135	1·19

Probably the form of the wave of difference of potential between the terminals of the induction-coil has much to do with the ratio of increase.

With a certain length of spark and a certain capacity of condenser, a luminous brush is perceived at the positive terminal, and a glowing point at the negative terminal; as the capacity is increased or diminished, more or less of a luminous brush appears also at the negative terminal. This probably means that more or less of positive electrons are discharged from the negative pole, which will complicate the phenomena under observation.

Experiments were tried to show the effect of altering the capacity of the condenser :—

| Fraction of whole capacity used. | Spark-length. | | Ratio. |
	Unassisted.	Negative assisted.	
$\frac{9}{9}$	12 mm.	14 mm.	1·16
$\frac{7}{9}$	12 mm.	15 mm.	1·25
$\frac{3}{9}$	15 mm.	22 mm.	1·46
$\frac{1}{9}$	15 mm.	24 mm.	1·60
$\frac{9}{9}$	82 mm.	107 mm.	1·30
$\frac{8}{9}$	91 mm.	116 mm.	1·27
$\frac{5}{9}$	88 mm.	116 mm.	1·32

Thus the effect of the variation of the condenser capacity varies considerably with the length of the spark.

It was found that with a spark-length of 60 mm. or more, a capacity of $\frac{7}{9}$ of the full gave the best results.

Experiments were made to try and discover the causes of these phenomena ; and first with regard to the action of the positive assister. In these experiments it must be premised that the error in the measurement of the spark-length may be as much as 1 or 2 per cent., owing to slight irregularity in the vibrator. Also the effects varied somewhat on different days, apparently owing to changes in the temperature and humidity.

To determine whether the assisting effect was due to some form of radiation or ionization, the assister was so placed that

its terminal was some distance from the main positive sparking terminal ; it was then found that a large sheet of mica inter- vening between it and the spark-gap caused the effect to cease entirely.   When the mica, however, had slits cut in it parallel to the spark-gap, so as to necessitate any luminous radiation to the negative terminal or to the spark-gap passing through the mica, whilst the slits offered a good passage for the electrons discharged from the positive assister to the spark-gap—then the effect was almost as great as when the mica was withdrawn.   Thus the effect was not due to luminous or ultra-luminous radiation, or to some form of Hertzian oscil- lations, but was due to the ionization of the air by the positive discharge from the assister.   The impulsive rush of the electricity into the positive assister apparently gives the discharged ions power to ionize a longer spark-gap ; $\dfrac{dV}{dt}$ is greatly increased.

When experimenting with positive assisters, it was found that the longest spark was obtained by placing the assister in the line of the spark-gap, one end almost in contact with the positive terminal.   A short assister in this position seems just as effective as a longer one.   An assister 5 mm. in length gave about 3 per cent. longer spark than assisters of 90 mm., 60 mm., and 140 mm., and 22 per cent. longer spark than one of 400 mm. coiled into a length of 100 mm.

In order to experiment on the action of the negative assister, an arrangement was made as shown in fig. 2.

Fig 2.
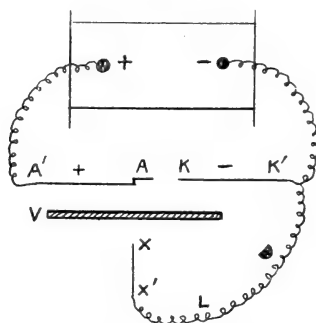
In this, A is the positive terminal ; K is the negative terminal ; XX′ is the negative assister ; L is the wire leading from the negative pole of the induction-coil. V is a vulcanite screen interposed between the assister and the spark-gap.

The term " assister attached " means X′ in contact with L.

The term "assister detached" means X' nearly in contact with L (the gap varied usually from $\frac{1}{2}$ to 1 mm.).

To start with, it was found that an assister arranged in this way gave almost as great an effect as an assister arranged to have approximate contact with K, and of course it was more convenient to experiment with.

It was found that a sheet of mica placed midway between the assister terminal and the spark-gap caused hardly any diminution of the lengthening effect of the assister. A plate of vulcanite 3 mm. thick acted in the same way as the mica.

The effect, therefore, in this case could not be due to ionization produced by electrons proceeding from the negative assister.

Experiments were tried to find how the effect of the negative assister varied when placed at different distances from the positive terminal. With assister of wire 1 mm. in diameter and 100 mm. long, the following results were obtained.

(The assister XX' was on a line from A at right angles to the spark-gap AK.)

		Spark-gap.
Unassisted		45 mm.
XA = 63 mm. and XX' detached		63
XA = 90 "		57
XA = 120 "		50

Experiments were tried to show the effect of moving XX' parallel to itself towards A.

Keeping the distance from X to AA' = 65 mm. and XX' being detached—the following results were obtained :—

			Spark-gap.
Unassisted..................................			45 mm.
X to AA' = 65 mm., A to line of X'X = 0			67
" A " X'X = 40 mm.....			65
" A " X'X = 80			58
" A " X'X = 120			54
" A " X'X = 160			47

Experiments were then tried to show the effect of the assister when "detached" and when "attached." When trying these, it was observed that when the vulcanite screen was brought very close to the point of the assister, then a decided diminution of the lengthening effect of the assister was produced when the assister was in the "detached" position.

With negative assister at distance XA = 50 mm., the vulcanite screen midway between X and A :—

	Spark-gap.
Unassisted	75 mm.
With assister attached..................	88
With assister detached	111

Showing that in this case (with the unassisted spark-gap

of 75 mm. or $1\frac{1}{2}$ times the distance XA) 64 per cent. of the whole effect is due to the assister being detached.

In another case, with negative assister distant 65 mm. from the positive terminal the results were :—

	Spark-gap.
Unassisted	68 mm.
With assister attached...................	75
With assister detached...................	97

Here 76 per cent. of the effect is due to the assister being detached.

In another case, with negative assister distant 120 mm. from A, the results were :—

	Spark-gap.
Unassisted	75 mm.
With assister attached...................	76
With assister detached	93

Here nearly all the effect is due to the assister being detached.

The further off the assister the greater is the part of the whole effect produced by detachment.

Additional experiments showing the " detached " effect and the effect of the screen being placed near the assister point are as follows :—

(*a*) Negative assister 100 mm. long, distant 110 mm. from the positive terminal.

	Spark-gap.
Unassisted.............................	86 mm.
Assister attached	91
Assister detached......................	110
Assister detached and screen near point	98

(*b*) Negative assister distant 120 mm.

Unassisted	75
Assister attached	76
Assister detached	93
Assister detached and screen near point	76
Assister detached and screen 10 mm. from point.	88
Assister detached and screen 20 mm. from point.	93

(*c*) Negative assister distant 65 mm.

Unassisted	68
With detached assister (screen midway)	97
With detached assister (screen near point) ..	86

Here 37 per cent. of the effect is stopped by having the screen near the point of the assister.

(*d*) With negative assister 280 mm. long at 96 mm. distance.

	Spark-gap.
Unassisted	68 mm.
Assister detached	96
Assister detached and screen near point	80

Experiments were tried with an assister 160 mm. long in a closed glass tube 6 mm. in diameter with bulb 10 mm. in diameter. Assister distant (XA) 100 mm.

	Spark-gap.
Unassisted	79 mm.
Detached assister in glass tube with point 30 mm. from the closed end	93
Do. with screen near closed end	93
Detached assister in same glass tube with point 5 mm. from the end	89
Detached assister removed from the glass tube.	105

An assister 100 mm. long was coated with solid paraffin for 5 cm. of its length, and gave the following results :—

	Spark-gap.
Unassisted	81 mm.
With assister detached.	90
With assister detached, and microscopic puncture in the paraffin at the point	112
With assister detached and puncture 12 mm. from point of assister	98

An experiment on another day with assister of 100 mm. covered with paraffin over 7 cm.; distant 100 mm. from A, gave :—

		Spark-gap.
Unassisted		78 mm.
Assister attached		82
Assister detached (no screen)	End punctured	103
Assister detached (screen midway). . . .		100
Assister detached (screen near point) . .		94
Assister detached (no screen)	Puncture closed.	90
Assister detached (screen near point). .		90

From these experiments it is evident that placing the vulcanite screen near the point has almost the same effect as completely enclosing one-half of the assister in paraffin.

An assister of 160 mm. was taken, and 110 mm. of its length was placed in a glass bottle 60 mm. in diameter, the point of the assister being 20 mm. from the end of the bottle and 100 mm. from the spark anode. The results were :—

	Spark-gap.
Unassisted	74 mm.
With detached assister in the bottle	103
With detached assister (bottle removed)	108

The same assister embedded in solid paraffin contained in a glass tube gave the following results :—

		Spark-gap.
Unassisted		87 mm.
With assister detached	Distance from anode 60 mm.	98
" attached		90
" detached	Distance from anode 40 mm.	98
" attached		88
" detached	Distance from anode 27 mm.	99
" attached		89

Compare these results with those from a bare assister separated from the anode by a vulcanite screen.

	Spark-gap.
Unassisted	45 mm.
Assister distant 63	63
Assister distant 12 (detached)	79
Assister distant 12 (attached)	72

Experiments were tried to find the effect of a metal screen in addition to the vulcanite screen.

A tin plate about 30 cm. square was placed touching the vulcanite screen and on the same side of it as the assister; the assister used was 160 mm. long, with 100 mm. of its length in a glass bottle.

Assister distance XA = 85 mm.   The results were :—

	Spark-gap.	
	Tin insulated.	Tin removed.
Unassisted..............	78 mm.	77 mm.
Assister attached	78 mm.	78 mm.
Assister detached......	87 mm.	97 mm.

Next a plate of lead, 200 mm. × 140 mm. × 1·5 mm. was used—in some of the experiments the lead was put to earth.

Results:—

	Spark-gap.		
	Without lead.	Lead insulated.	Lead to earth.
Unassisted..............	82 mm.	82 mm.	95 mm.
Assister attached	87 mm.	82 mm.	95 mm.
Assister detached......	109 mm.	97 mm.	98 mm.

When the lead is insulated it stops the whole of the effect of the attached assister, and when put to earth almost the whole of the effect of the detached assister. (Note the increased length of spark due to the presence of the earthed lead plate—its distance from the anode was 48 mm.)

The effect of altering the length of the assisters was tried. Assisters of wire 1·6 mm. in diameter.

Assister-length.	Spark-length.
2 cm.	105 mm.
4 cm.	105
16 cm.	107
26 cm.	106
32 cm.	106

Showing that the length of the assister makes no appreciable difference in the length of spark.

In the course of the experiments it was found that if the end of the assister towards the positive terminal was pointed the effect was somewhat greater than if blunt; also that an assister whose end was made up of a brush of widely separated wires was more effective than a single wire.

The most effective negative assister was a semicircle of wire almost touching the negative wire close to its terminal and having the positive terminal as centre.

In the experiments with the negative assister arranged as in fig. 2, the increase of sparking distance depends upon the length of the spark passing between the assister and the negative wire; when this spark is extremely small there is very little increase in the main spark-length, but as the assister gap is increased there are two values of it which give maxima values for the main spark-gap. This is shown in the following experiments with an assister 100 mm. long.

Negative assister gap.	Spark-length.
·2 to 1·0 mm.	107 mm.
3·0	96
11 to 15·0	108
,, 17·0	102
,, 19·0	84
Unassisted	79

A similar lengthening of the spark is obtained when the assister, instead of being separated by a short gap from the negative wire, is separated by a short gap from a wire put to earth. Thus in one case :—
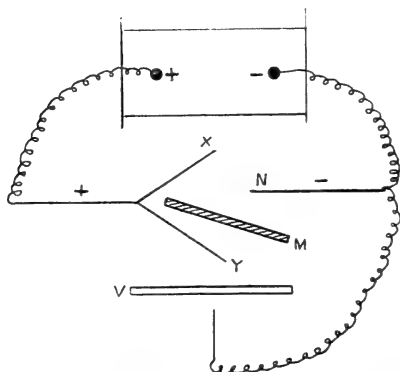
	Spark-length.
Unassisted...........................	77 mm.
With assister to earth.................	82
Assister gap to earthed wire = ·1 mm.	102

That the increased length of spark produced by a negative assister is due to the electrons discharged from the positive terminal is shown by an experiment in which the positive wire bifurcates giving two terminals; then a negative assister acting on one of these causes sparking between the other positive terminal and the negative terminal, but if a sheet of

mica be interposed between this spark-gap and the assisted positive terminal the sparking distance is reduced to its original value.

Thus in an experiment where X and Y are the positive terminals and N the negative terminal (see fig. 3), the

Fig. 3.

unassisted sparking distance between X and N was 44 mm., but with Y assisted this became 65 mm. ; with a mica screen interposed between Y and XN it fell again to 44 mm. ; showing that the lengthening of the spark-gap XN was due to the electrons proceeding from the assisted point Y. In the figure, M shows the position of the mica screen.

The above experiments lead me to suggest the following explanation of the phenomenon under observation.

The action of the negative assister in lengthening the spark may be due to the attraction of the negative electrons in the assister and discharged from the assister on the positive electrons.

When the negative assister is impulsively charged across its spark-gap this causes an induced impulsive rush in the positive wire, causing the positive electrons to leave the terminal with greater velocity. The effect of the negative assister is probably added to by a discharge of negative electrons from its point when it is impulsively charged. This negative discharge from the point is in an effective position to act attractively upon the positive electrons, and endures long enough to produce its full effect. When the negative assister is surrounded by an insulator or when a

sheet of vulcanite is brought up close to its point, the negative discharge is either entirely prevented or is quickly reabsorbed.

A somewhat similar explanation probably accounts for the fact that when an imperfect conductor is brought near the positive terminal of an induction-coil, the spark is lengthened ; the induced negative charge and the discharge of negative electrons from this conductor giving increased energy, by their attraction, to the discharged positive electrons.

It will be found that the frayed end of a strip of asbestos cloth acts as an excellent assister when brought close to the positive terminal, being most effective when put to earth.

A striking experiment illustrating this subject which brings out the effect of the positive electrons in causing a spark-discharge, as compared with the negative electrons, is made by placing the two similar terminals of wires from an induction-coil in a glass tube ; arranging that the spark remains of the same length when the commutator is reversed. Now when a few turns of thin wire are wound round the glass tube, almost touching it and not too near to the positive terminal, the length of the spark is greatly increased when one end of this wire almost touches the negative end of the induction-coil, but the length is hardly altered when the commutator is reversed. In one experiment it was found :

Unassisted, sparking distance.................. = 77 mm.
With wire coil to negative end, sparking distance. =125
With commutator reversed, sparking distance .. = 74

In order to ascertain whether the effect of the negative assister was cumulative, it was endeavoured to find evidence of reflexion of the impulse action of the assister, but the experiments failed to give any results.

It would be interesting to find what increase of spark-length could be obtained from two points, maintained at a steady difference of potential, by the rapid rotation of assisters in very close proximity to the terminals ; various rates of rotation being employed giving various values of $\frac{dV}{dt}$. The author had not a sufficiently steady supply of high-tension electricity to try this experiment.

XLVIII. *A Radioactive Gas from Crude Petroleum.*
By E. F. BURTON, *B.A.**

IN the course of their investigations on the radioactivity of the atmosphere, Elster and Geitel † have shown that the soil and rock-masses constituting the surface-layers of the earth are the source of an emanation, or gas, which gradually escapes into the air, and there exhibits properties analogous to the radioactive emanations from thorium and radium. In a conjoint paper by Professor McLennan and myself ‡ on the conductivity of air confined in receivers of different metals, some observations are cited which indicate that metals generally are, to a slight degree, the source of a similar emanation. This result has since been confirmed by Strutt §, who found that air drawn through a glass tube, heated just below redness, and containing scrap copper, acquired a conductivity three or four times its normal value. Strutt § has also shown that a highly radioactive emanation can be obtained by bubbling air through mercury heated to about 300° C.

More recently, Professor J. J. Thomson ‖ established the existence of a radioactive gas in the Cambridge tap-water as well as in the water from a number of wells in different parts of England. Similar results have been obtained by Himstedt ¶ at Freiburg, and by Lord Blythswood and H. S. Allen ** with the mineral waters of Bath. Later still Adams †† made a careful study of the radioactive gas in Cambridge tap-water, and his results, as well as those of Strutt on the emanation from mercury, go to show that the activity in all these cases is due to the presence of a substance very similar to, if not identical with, the emanation from radium.

In the following paper an account is given of some experiments with a highly radioactive gas obtained from crude petroleum, which, both in the rate at which its activity decays and in the nature of the induced radioactivity it produces, very closely resembles the emanations dealt with by the investigators just mentioned.

Apparatus.—The petroleum used in the experiments was

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* Communicated by Prof. J. J. Thomson, F.R.S.
† *Phys. Zeit.* iii. Jahr. 24, p. 574; *Denksch. d. Kommission für luft-electrische Forschungen,* München, 1903.
‡ Phil. Mag. vol. v. (June 1903) p. 699.
§ Phil. Mag. vol. vi. (July 1903) p. 113.
‖ Proc. Camb. Phil. Soc. xii. 3 (1903) p. 172.
¶ *Berichte der Naturf. Ges. von Freiburg i. B.,* 1903, xiii. p. 101.
** Nature, Jan. 14, 1904, p. 247.
†† Phil. Mag. vol. vi. (November 1903) p. 563.

obtained from one of the wells belonging to Mr. A. C. Edward, of Petrolia, Ontario, to whom my most sincere thanks are due for many samples of oil supplied during the course of the investigation. The petroleum from this locality is drawn directly from the Corniferous limestone, which lies at a depth of 465 feet below the surface, and, while it may possibly originate in these rocks, there are reasons for concluding that the oil has its source in a deeper stratum, very probably in the underlying Trenton formation.

Fig. 1.
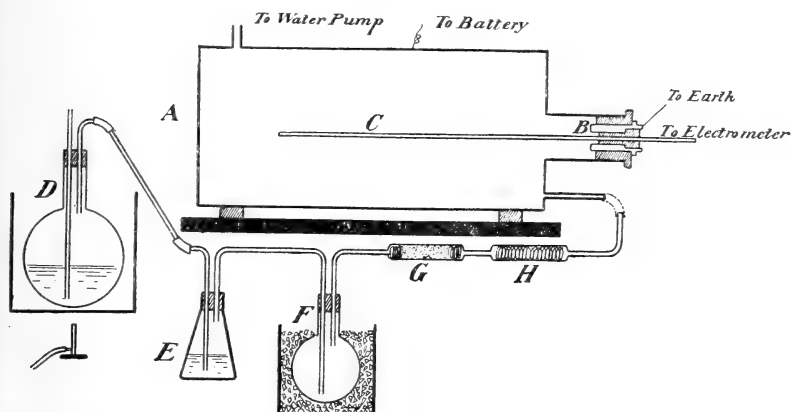
The petroleum to be tested was contained in a large three-litre flask D (fig. 1), supported in a water-bath. This flask was connected to a wash-bottle E, partly filled with concentrated sulphuric acid, and to a second flask F, embedded in ice for the purpose of condensing any vapours from the heated oil. The tube G was filled with phosphoric pentoxide, and the tube H tightly packed with glass-wool. The vessel A, made of thin galvanized iron, 62 cms. long and 25 cms. in diameter, was provided with an exploring electrode C, which was supported by an ebonite plug carrying a guard tube B. The rod C was connected to one of the pairs of quadrants of a quadrant electrometer of the Dolezalek type, whose sensitiveness was such that a potential-difference of one volt between the quadrants gave a deflexion of 1100 mms. on a scale at a distance of one metre. Throughout the experiments, the cylinder A was maintained at a potential of 168 volts by a battery of small storage-cells, and the conductivity of the gas which it contained was determined by measuring the saturation current to the exploring electrode. This saturation current, when the cylinder A was filled with ordinary dry

air, was about 16·5 scale-divisions per minute. After heating
the water in the bath to the boiling-point, air was bubbled
for fifteen minutes through the oil and drawn into the cylinder
A by means of a water-pump. The cylinder was then dis-
connected from the tube H and hermetically sealed, after
which measurements were made, from time to time, on the
conductivity of the gas which it contained. The density of
this gas was determined in every case and found to be about
1·05, air being taken as unity.

A Radioactive Emanation.—On first introducing into the
cylinder the air which had passed through the oil, it was
found to have an initial conductivity very greatly in excess
of that of normal air. Its conductivity steadily increased,
after the cylinder was closed, for about three hours, when it
reached a maximum value, after which it slowly decreased
approximately in a geometrical progression with the time.
Fresh air passed through different samples of petroleum into
the cylinder under exactly similar conditions was found to
possess different initial conductivities, but, in every case, the
conductivity of the confined air steadily rose in about three
hours to a maximum about 40 per cent. in excess of the initial
value. It then decayed according to an exponential law,
always dropping to one-half value in about 3·125 days. A
typical set of observations on the conductivity of air bubbled
through one of the samples of oil is given in Table I., the
time being reckoned from the moment when the cylinder was
closed.

TABLE I.

Time.		Current. Arbitrary Scale.	Time.		Current. Arbitrary Scale.
hrs.	min.		hrs.	min.	
...	10	92	27	...	92
...	30	95·6	41	30	83·5
1	4	103	50	...	77·8
1	35	111·7	67	...	71
2	8	116·5	73	30	67·7
2	43	119·7	95	...	60·3
9	30	111·6	116	30	55·5
20	...	101	128	...	50·8
23	...	95·7	138	30	48·6

These results are shown graphically in fig. 2, where the
ordinates of the curve represent the conductivity of the gas
and the abscissæ the times in hours.

As in the experiments of Professor Thomson with the
Cambridge tap-water and those of Strutt with mercury, all

of the observed phenomena lead to the conclusion that the air, in passing through the petroleum, becomes mixed with some radioactive gas or emanation. The initial portion of the curve leading up to the maximum corresponds exactly to that

Fig. 2.
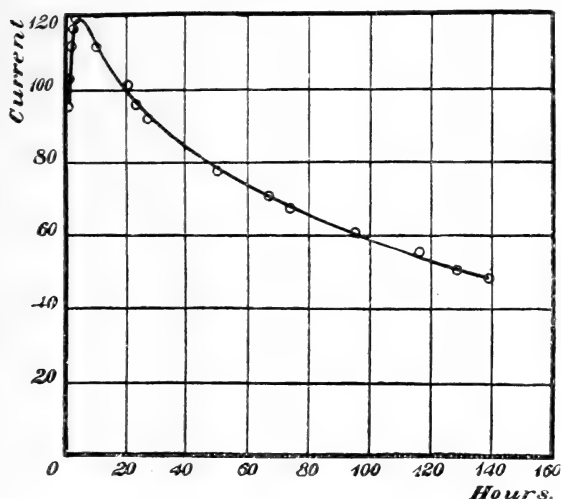
of the curve given by Rutherford * for the emanation from radium, and also to that of the curve given by Strutt for the radioactive gas obtained by bubbling air through mercury and may be explained in the same way. The value of the conductivity immediately after the cylinder has been sealed measures the ionization due to the emanation itself. But, according to the disintegration theory proposed by Rutherford, the emanation is continuously producing by its decay the matter which causes excited radioactivity, and the ionizing power added by this latter material more than neutralizes, for a time, the decrease due to the decay of the emanation. Thus the conductivity of air freshly charged with this emanation gradually increases to a maximum state, which is reached when the loss in the ionizing power due to the decay of the emanation is just equalled by the gain contributed by the excited radioactivity produced in this process of decay.

From this time the rate of change indicated gives the rate of decay of the emanation. The law which the rate of decay

* Phil. Mag. vol. v. (April 1903) p. 445.

TABLE II.

Column 1. BURTON.

Time in hours.	Current. Arbitrary Scale.	$1/\lambda$.
0	119·7	
17	101	360,000
47	77·8	414,000
64	71	669,000
92	60·3	617,000
135·6	48·6	726,000

$1/\lambda = 557,000.$

Half value in 3·125 days.

Column 2. STRUTT.

Time in hours.	Current. Arbitrary Scale.	$1/\lambda$.
0	140	
18	118	379,000
42	94·5	389,000
66	78·7	472,000
90	66·3	504,000
140·5	40·6	371,000

$1/\lambda = 423,000.$

Half value in 3·18 days.

Column 3. ADAMS.

Time in hours.	Current. Arbitrary Scale.	$1/\lambda$.
0	188	
16·7	160	366,000
40·4	129	401,000
64·8	108	494,000
88·9	86	381,000
139·6	53	372,000
160·8	46	573,000

$1/\lambda = 425,000.$

Half value in 3 days.

of the emanation from radium follows may be expressed by
the equation

$$I_t = I_0 e^{-\lambda t},$$

where I_0 is the value of the conductivity at any given time,
I_t the value after an interval of t seconds, e the base of natural
logarithms, and λ a constant. By using this equation the
values of $1/\lambda$ have been determined for a number of pairs of
the readings given above, and the results are tabulated in
column 1 of Table II. These values of $1/\lambda$, which give a
mean of 557,000, show a marked increase with the time,
and consequently indicate that the rate of decay is slower
than that required by the law given above. This departure
from the law of decay is probably due to a slight trace of a
more persistent radioactive substance in the gas than the
emanation, and will be referred to later.

In column 2 of Table II. is given a set of Strutt's readings
for the ionization due to the radioactive gas in mercury, and
in column 3 the values obtained by Adams with the active
emanation in Cambridge tap-water. The calculated values
of $1/\lambda$ are inserted in both cases, but do not show the increase
exhibited by the numbers in column 1. The averages of the
three series of values of $1/\lambda$ given in Table II., together
with the mean values of the same constant obtained by
Mme. Curie * and by Rutherford † for the decay of the
emanation from radium, as well as the mean value calculated
from Himstedt's results for the radioactive gas in water, are
collected in Table III. The values show a very close agree-
ment, and lead to the conclusion that the active gases from
petroleum, spring-water, and mercury are very probably
identical with the emanation from radium.

TABLE III.

Experimenter.	Source of Emanation.	Value of $1/\lambda$.
Mme. Curie	Radium.	497,000
Rutherford	Radium.	463,000
Strutt	Mercury.	423,000
Adams	Tap-water.	425,000
Himstedt	Water.	491,000
Burton	Petroleum.	557,000

In his experiments with the water from the Cambridge
mains, Professor J. J. Thomson found that when the water

* Thèses prés. à la Faculté des Sci. de Paris, 1903.
† Phil. Mag. vol. v. (April 1903) p. 445.

had once been well boiled, the gas expelled on any subsequent re-boiling was not appreciably radioactive. In the present investigation air was drawn through a selected sample of oil into the cylinder on three consecutive days, and again on the sixth day, the first measurement being made about 24 hours after the petroleum had been pumped from the well. Each time the oil was used the bath was brought up to the boiling-point and the air bubbled through it for 15 minutes, when observations on the conductivity of the air in the cylinder were commenced and continued at intervals over a period of about 20 hours.
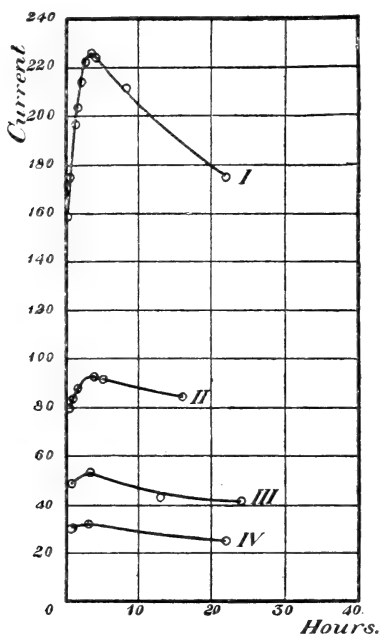
TABLE IV.

Curve 1.		Curve 2.		Curve 3.		Curve 4.	
Time.	Current. Arb. Sc.	Time.	Current. Arb. Sc.	Time.	Current. Arb. Sc.	Time.	Current. Arb. Sc.
h m		h m		h m		h m	
0 10	158·7	0 30	80·0	0 40	49	0 35	29·8
0 30	174·2	1 0	83·4	3 10	53	0 55	30·3
1 5	196·7	1 30	87·2	13 0	43	3 0	31·6
1 30	203·7	3 40	92·6	24 0	41·5	22 0	25·2
1 50	214·2	5 0	92·0				
2 30	222·5	16 0	84·3				
3 30	226·0						
4 0	224·2						
8 15	211·1						
22 0	176·0						

The results which are embodied in Table IV., and illustrated by the curves in fig. 3, show that the activity acquired by fresh air, when drawn through the oil, gradually decreased from day to day. The curves corresponding to the different tests exhibit the same characteristics as that in fig. 2. In each case the conductivity rose to a maximum in about three hours, and then gradually decreased. The maximum currents in the four trials were respectively 13·9, 5·6, 3·2, and 1·9 times the conductivity of the ordinary air, thus showing that the oil at the end of a week still possessed in a marked degree the power to impart radioactivity to air drawn through it. Experiments made with a sample of oil, which had been used in some preliminary tests and had been placed aside in a tightly-corked glass vessel, for over a month, gave values almost identical with those represented by curve 4, fig. 3, the maximum conductivity impressed in this case being 1·6 times that of the normal air. From these results it would

appear that there is present in crude petroleum an active substance more persistent than the emanation from radium, perhaps a minute quantity of radium itself. If this be so, the air drawn through the oil might possibly carry with it

Fig. 3.

into the cylinder a slight trace of this substance. Such a condition would explain the departure from the law of decay $I_t = I_0 e^{-\lambda t}$ exhibited by the increasing values of $1/\lambda$ in column 1 of Table II.

Induced Radioactivity.—Each time the gas containing the emanation was blown from the cylinder, the conductivity of the ordinary air admitted from the room was found to be still very high. Repeated tests showed that the initial conductivity of this fresh air was about 35 per cent. of that of the displaced gas, but in every case it quickly fell, until after about two hours the conductivity reached the normal value of 16·5. In expelling the emanation, a blast of air was sent through the cylinder continuously for five minutes by means of a small foot-pump, after which the receiver was again sealed.

Measurements were then made on the conductivity at short

intervals, and in Table V. are given the results of one of these tests, the time being taken from the closing of the

<div align="center">TABLE V.</div>

Time in Minutes.	Current. Arbitrary Scale.
5	73·8
15	58·2
25	50·6
35	47·2
46	41·0
56	35·6
65	35·4
75	32·0
91	26·0
200	16·7

cylinder. In this particular case, the cylinder while filled with the air containing the emanation was maintained at a negative potential of 168 volts for 22 hours, and during this time the conductivity rose from its initial value of 158·7 to

<div align="center">Fig. 4.</div>

Minutes.
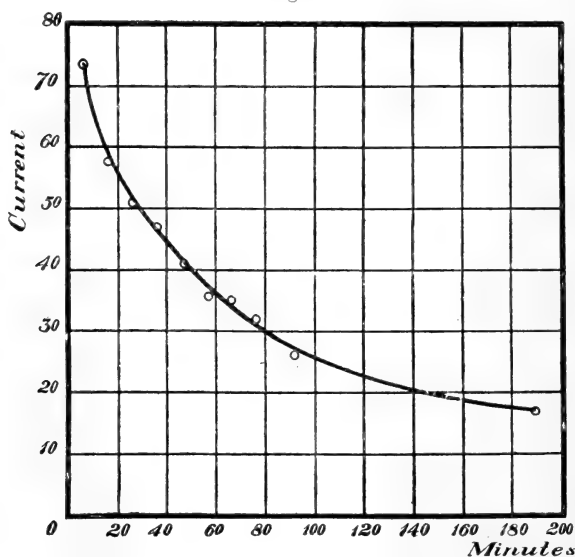
a maximum of 226, and then fell to 176·3 before the expulsion took place.

The curve given in fig. 4, in which the ordinates represent

currents and the abscissæ times, illustrates the results in this table. From this curve it is seen that the conductivity decreases in a geometrical progression with the time, falling to one-half value in about 35 minutes. This phenomenon is exactly analogous to that which other investigators have found in working with the radioactive emanations from thorium and radium, and which has been explained on the assumption that these emanations have but a transitory existence, and are gradually transmuted to a new substance which has a definite rate of decay and which is the cause of the so-called induced or excited radioactivity. On this view it is clear that, from the observations above, the active emanation from petroleum also produces the substance which is responsible for induced radioactivity, and that the presence of this substance in the cylinder is the cause of the high conductivity of the fresh air which replaced that blown out.

An experiment, giving similar results, was conducted under the same conditions as that just described, except that the cylinder was maintained for 22 hours before the emanation was expelled at a positive potential of 168 volts. This would show that the substance responsible for excited radioactivity was left in the cylinder in both cases when the air was blown out, and, as it is known that negatively-charged conductors in the presence of radioactive emanations become more active than those positively electrified, it is very probable that in the first experiment the excited radioactivity was deposited on the walls of the receiver, while in the second case it was concentrated upon the electrode C.

A confirmation of this conclusion was obtained by exposing a conductor under negative electrification, and then under positive, to the petroleum emanation. The exploring electrode C was taken from the cylinder A and suspended in a large glass tube, through which air containing the radioactive emanation was drawn. It was connected for half an hour to the negative terminal of an electrical machine giving a potential of about 10,000 volts, and on being replaced in the receiver it increased the conductivity of the air to about three times its normal value. The conductivity in this case fell to a half value in the same time as before. When the exploring electrode was suspended under a positive electrification of 10,000 volts, for the same time, in the current of air containing the emanation, it did not acquire any appreciable activity.

It has been shown by Mme. Curie, Rutherford, and others, that the induced radioactivity from the radium emanation

decays to one-half value in about 30 minutes, and Adams has found that the induced radioactivity from the gas in Cambridge tap-water falls to half value in about 35 minutes. These values are practically the same as that determined in the present investigation, and confirm the conclusion already arrived at, that the active gas from crude petroleum is very probably identical with the emanation from radium.

Conclusion.

Summarizing the results given in the foregoing paper we have the following :—

1. Fresh crude petroleum has been found to contain a strongly radioactive gas which is similar in its rate of decay and also in the rate of decay of the induced radioactivity which it produces to the emanation from radium and to the emanations obtained by a number of experimenters from mercury and from certain waters fresh from the earth.

2. This radioactive gas decays approximately according to an exponential law, falling to half value in 3·125 days.

3. It produces an induced radioactivity whose rate of decay is such that it falls to a half value in about 35 minutes.

4. There are indications of the existence in crude petroleum of slight traces of a radioactive substance more persistent than the radium emanation.

In a paper published during the progress of the experiment by Elster and Geitel* reference is made to a recent investigation by Himstedt on the radioactivity of petroleum, but up to the present time this communication has not been received, so that a comparison with his results cannot be made here.

In conclusion I desire to express my thanks to Professor J. C. McLennan for suggesting the research and for his invaluable aid and advice at all times. I also wish to acknowledge my indebtedness to Mr. L. Gilchrist and to Mr. S. Dushman for kindly aiding me in making some of the observations.

Physical Laboratory,
      University of Toronto,
            March 31, 1904.

* *Archives des Sci. Phys. et Nat.* sér. 4, t. xvii. (Jan. 1904) pp. 5–22.

XLIX. *On Clausius' Theorem for Irreversible Cycles, and on the Increase of Entropy.* By Prof. W. McF. Orr, *M.A.**

Introduction.

1. **I** BELIEVE that many persons interested in the exposition of Thermodynamics are dissatisfied with the discussions which are accorded to the theorem generally known as Clausius' theorem for irreversible cycles and to equivalent theorems relating to increase of entropy by most writers on the subject. I venture to put forward some criticisms on such discussions, and to suggest a slight modification of the statement of the Second Law of Thermodynamics, which I believe is necessary if any such theorem is to be logically deduced therefrom.

Planck's Discussion. His peculiar Definition of "Irreversibility."

2. Planck's treatment of the subject takes the form of a series of propositions relating to entropy, and is of peculiar interest on account of the unusual sense in which he uses the words "reversible" and "irreversible." An irreversible process is defined † not merely as one such that it cannot be directly reversed, but one such that it is impossible, starting with the final state of the system (which we may denote by B) and using appliances of any kind whatsoever, mechanical, thermal, chemical, &c., to obtain the initial state (which may be denoted by A) and leave all the materials and machines used in the same condition exactly (at A) as before their application (at B). A process which is "reversible" in this sense only is described as "reversibel;" one which can be directly reversed as "umkehrbar."

He begins by considering perfect gases. The properties postulated for such bodies are not stated as clearly as is desirable ‡; and the entropy ϕ of a mass M of a perfect gas of molecular weight m is defined by means of the equation :—
$$\phi = M(c_v \log_e \theta + R/m \cdot \log_e v).$$
He demonstrates the theorem that if a system of perfect gases has its total entropy the same in two different states, it may be transformed from one

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* Communicated by the Author.

† *Vorlesungen über Thermodynamik*; or Ogg's translation, Arts. 109, 112.

‡ The reviewer of Ogg's translation in 'Nature,' Dec. 31, 1903, remarks with justice that a student may be thus led into the fallacy of reasoning in a cycle, and also that he is likely to be confused by being given two different definitions of entropy, firstly one which is applicable to perfect gases only, and afterwards another to be applied to all bodies.

state to the other by a "reversible" ("reversibel") process without leaving changes in other bodies*. This is proved substantially as follows :—Let the initial state of the system be given by the temperatures $\theta_1, \theta_2, \ldots \theta_n$, and the entropies $\phi_1, \phi_2, \ldots \phi_n$, and the final state by the corresponding dashed letters. Bring the first gas and the nth to a common temperature by a reversible adiabatic change (the necessary work in this and following expansions or compressions being done by or on bodies external to the system †) ; then place them in thermal communication, and by indefinitely slow compression or expansion compel heat to pass between them until the entropy of the first gas is changed from ϕ_1 to ϕ_1' ; let the second, third, and remaining gases by similar interchanges of heat with the nth change their entropies from $\phi_2, \phi_3, \ldots$ &c., to $\phi_2', \phi_3', \ldots$ &c. Then, since in each interchange the sum of the entropies of the two gases concerned is unaltered, and since by supposition $\phi_1 + \phi_2 + \ldots \phi_n = \phi_1' + \phi_2' + \ldots \phi_n'$ it follows that the entropy of the nth gas is now ϕ_n'. Finally, let each gas be expanded or compressed adiabatically until they attain the temperatures $\theta_1', \theta_2', \ldots \theta_n'$, respectively, and they are then in the given final state since each has the assigned temperature and entropy.

3. It is assumed as a result of experience that the expansion of a perfect gas without performing any external work is "irreversible" ("irreversibel") and also, as usual, that in such a case when the gas after expansion comes into a state of equilibrium its final temperature is the same as the initial provided it does not interchange heat with any external bodies, so that the entropy, from its definition, is increased by such an expansion. From these postulates it follows that it is impossible to diminish the entropy of a perfect gas without leaving changes in other bodies‡.

The last deduction is then extended from the case of a single gas to that of a system of gases : it is proved that it is impossible to diminish the total entropy of such a system without leaving changes in other bodies§. The demonstration is as follows :—Suppose, if possible, that the total entropy could be diminished without leaving changes in other bodies ; let the initial entropies be $\phi_1, \phi_2, \ldots \phi_n$, and the final $\phi_1', \phi_2', \ldots \phi_n'$, where $\Sigma \phi' < \Sigma \phi$. Then it is possible, according to the proposition of Art. 2 above, to bring the system, without leaving changes in any other bodies, from the final

state into any other in which the sum of the entropies is $\Sigma\phi'$, and accordingly into a state in which the first $n-1$ gases have respectively the entropies $\phi_1, \phi_2, \ldots \phi_{n-1}$, and the nth in consequence the entropy $\phi_1' + \phi_2' + \ldots \phi_n' - \phi_1 - \phi_2 - \ldots \phi_{n-1}$, which by supposition is less than ϕ_n. The first $n-1$ gases may now be restored to their original states by reversible adiabatic processes. The entropy of the nth gas has, therefore, been diminished without leaving changes in other bodies, which has been shown to be impossible. The proposition thus follows.

4. From these theorems it then follows that if a gas-system undergoes an " irreversible " (" irreversibel ") process in which no change is left in other bodies the sum of the entropies is increased [*] ; for it cannot be decreased according to the last proposition, nor can it be unaltered, as then, according to the proposition of Art. 2 above, the process could be " reversed " (in Planck's sense).

The definition of entropy and the above propositions relating to it are then extended to bodies other than perfect gases.

Planck gives one Definition of " Reversibility," but uses another.

5. Now if the phrase " without leaving changes in other bodies " is to be interpreted literally, it is obvious that the proofs of the above propositions break down and that the propositions themselves are as a matter of fact untrue. In the case of the theorem of Art. 2 above, for instance, changes of volume are described in which work is done on or by bodies external to the system ; and it may be necessary to allow the aggregate volume of the system to increase in order that it may pass from one state to the other, thereby altering the volume, density, and possibly the temperature, of some other body. This has been noticed by the English translator ; he indicates [†] that the work to be done in such an expansion may be performed by raising weights, and that changes of position of such weights are not to be considered but that of course changes of density are. Under ordinary circumstances, however, no body can expand without producing a change of density in some other body. It appears, then, that the enunciations of the propositions should be amended by changing the phrase " without leaving changes in other bodies " into " without interchanging heat with other bodies," and that there should be a corresponding change in

[*] This is in substance part of the theorem of Art. 126, *loc. cit.*
[†] P. 91.

the definition of "reversibility." The definition which is *used* by Planck appears in fact to be this, that a process is "reversible" ("reversibel") if it is possible to pass the system back from the final state to the initial without interchanging heat with external bodies.

Planck conceives of the Entropy of a Gas when rushing into a vacuum.

6. Planck proceeds :—"It must not be supposed that the entropy of a gas has a meaning only for states of equilibrium. We may assume each sufficiently small particle, even of a gas in turmoil, to be homogeneous and at a definite temperature, and must, therefore, according to (52)"—(the equation in § 2 above by which the entropy of a gas is defined)—"assign to it a definite value of the entropy. . . . . A summation extending over all the particles of the mass—within which the values of v and θ may vary from particle to particle—gives the entropy of the whole mass of the gas in the particular state. The proposition still holds, that the entropy of the whole gas must continually increase during any process which does not give rise to changes in other bodies, *e. g.* when a gas flows from a vessel into a vacuum"[*].

Bertrand's Objection. The terms "Temperature," "Pressure," "Entropy," meaningless in many irreversible processes.

7. This view is in direct opposition to that held by Bertrand, who objected[†] that in many irreversible processes such terms as "temperature," "pressure," "entropy," have no definite meaning. "What," he asked, "is the pressure of a gas when it is rushing in a state of tumult into a vacuum? It is neither the same at each point nor the same in all directions. . . ." He consequently regarded such discussions as for the most part meaningless and futile.

Ordinary definitions of "Temperature" refer only to states of equilibrium.

8. The defining of "temperature," and of "equality of temperature," presents indeed a great fundamental difficulty in the Theory of Heat ; the proper elucidation of this matter is one of the most formidable tasks with which the writer of a treatise on Thermodynamics is confronted, and one to which a chapter, rather than as is usual merely a few lines,

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* Ogg's translation, Art. 127.
† *Thermodynamique,* Chap. XII.

might well be devoted. Any definition which can be regarded as at all satisfactory refers, in the first instance at least, to a body whose parts are without relative motion and which has attained an invariable state ; the matter is more difficult when such motions exist, or when its state is otherwise varying. The definition, or rather the *test*, of equality of temperature, which apparently finds most favour is that two bodies have, in general, the same temperature if, when placed in contact, there occurs in either no change of volume. This test is, however, unduly restricted, as it refers to one particular method alone of measurement ; if for " no change of volume " we substitute " no change whatever such as is, within the limited range of hotness in which we can apply a test, associated with changes in the heat sensations caused by that body," we obtain a definition which allows temperature to be tested by other means, as, for instance, by the electric resistance of platinum or by the electromotive force of a metallic circuit, methods which are actually in use. Two bodies in which, when placed in contact, no such change occurs are said to be in thermal equilibrium with one another.

Any such definition becomes obviously self-contradictory if applied to a body whose density and volume are changing sensibly. Many, indeed, if not all definitions of terms as measurable quantities, are only provisional ; definitions and laws are necessarily interwoven, and the statements of each require to be continually amended as knowledge increases. Probably the most striking instance of this in the whole domain of physical science is furnished by the fundamental laws and definitions of dynamics. The proper understanding of fundamental terms employed in any science is of the utmost importance : this is, however, only half a truth ; it is an *aim* of science to formulate a system of definitions and laws based on each other by the aid of which natural events may be described as simply as possible. To prohibit the use of terms whose meaning cannot always be stated with precision, or of statements which it is impossible to put directly to the test, would bar almost all scientific progress ; every increase in exactness in such matters requires, constitutes, and causes an addition to knowledge. It is *necessary*, however, that definitions and laws should not contradict themselves and each other. And the recognition of difficulties in such matters constitutes a step towards their removal; this fact will, I hope, justify the remarks in the present paper.

Kirchhoff's more general Definition of " Temperature."
Objections to it.

9. I am not aware that any attempt has been made to give a definition of temperature which may be applied to non-equilibrium states in any treatise on Thermodynamics, with the sole exception of Kirchhoff's (posthumous) *Theorie der Wärme*, edited by Planck, wherein (p. 114) the temperature of a fluid whose parts are in relative motion is defined by the statement that the total energy of any indefinitely small portion exceeds its kinetic energy by the energy which it would possess if it were at rest at the same temperature and density.

This definition appears indeed applicable to a fluid.   It could not, however, be applied to a solid in general : in a solid at rest the stress across a plane is not necessarily normal, nor of the same intensity for all planes through a given point : and the state of a solid cannot, like that of a fluid, be specified by two coordinates alone, such as, for example, temperature and a single pressure : consequently there is in general no equation connecting the energy of a solid at rest with its temperature and density.   This objection may fairly be made, I think ; for although the bodies discussed in Thermodynamics are chiefly fluids, or solids subjected to such stresses as could occur in fluids, they are not so exclusively or of necessity.

Kirchhoff's definition appears, however, to be open to the more serious objection that it is inappropriate as having no reference to any measurements which are usually made or which it is practicable to make.   When the portion of fluid considered is taken small enough, its kinetic energy becomes as nearly as we please the same as if it moved without distortion, and with the velocity which its mass-centre has, and its determination consequently involves only the determinations of that velocity and of the mass ; to measure its total energy, however, it must be *isolated* from external influences, a process which even in the case of a fluid is attended with considerable difficulty, and which in the case of a solid may be practically impossible, and after it has come into a state of relative equilibrium its temperature must be measured in the ordinary way.

Another Definition suggested.

10. A better attempt to define in such a case the temperature at any instant might be stated, I think, somewhat as follows :—
Suppose that into the centre of the very small portion of the body considered there is introduced a much smaller body constituting a thermometer of any kind by whose aid the

temperature might be measured if the portion considered were in an invariable state, the thermometer being moved with the body; and *assume* that if the thermometer is sufficiently small, any further diminution of its size makes no perceptible difference in its "reading" when the circumstances are repeated; then the temperature of the portion of the body means the temperature indicated at the instant by the thermometer.

This definition has at least the merit of referring to measurements which observers actually try to make.

The difficulty, however, cannot be surmounted simply by altering the words in which a definition is expressed. A definition is not merely useless but harmful, as leading to logical fallacy, unless there is some fact in Nature corresponding to it. The ordinary definition of temperature is based on the experimental fact that the definition is self-consistent; *i. e.*, that bodies which are in thermal equilibrium with the same body are in thermal equilibrium with one another. In a similar way the definition suggested above depends for its validity on the correctness of the supposition that if, on different occasions when the same circumstances recur, a number of sufficiently small thermometers are used, they would all at the corresponding instants indicate one and the same temperature, irrespective of their size (provided it is sufficiently small), of the substances of which they are made, and of the property which is taken as a basis of measurement: that is, they would, when each is placed in contact with the portion of the body considered, assume states which would be states of thermal equilibrium if in contact with each other. There is reason to believe that these suppositions are to a considerable extent justified if the body is changing its state in respect of density or other property; but it is, I think, open to question whether they are legitimate in such an extreme case as that of a gas rushing into a vacuum.

Although further emphasis of the interdependence of fact and definition in this matter is probably unnecessary, one may draw attention to the mistake which would be committed if the definition suggested above were altered so as to permit sensible relative motion between the thermometer and the parts of the body in contact with it, or if, for example, in the case of a stationary atmosphere and a body moved through it with a given constant velocity, one were to define equality of temperature as the steady state (*i. e.* in respect of changes such as are accompanied by changes in the heat sensations we perceive) which would ultimately be attained. It is known in fact that the steady state of the moving body is hotter than

its steady state at rest in contact with the same atmosphere at rest, so much so that meteorites moving through the atmosphere are frequently heated to incandescence, and that if bodies composed of different substances, or the same substance but of different sizes, are so moved through the air, their steady states would not in general be such as could be maintained in contact with each other. We usually express these facts by stating that the temperature of the moving body is raised above that of the atmosphere to such an extent that the rate at which heat is generated in it by friction against the atmosphere is equal to the rate at which it loses heat by radiation &c.; the amount of this excess depends on the velocity, size, and nature of the body.

Yet occasionally observers attempt to measure the temperature of the air by whirling a thermometer through it *.

It is then, I think, doubtful whether in such an extreme case as that of a gas rushing into a vacuum the temperature of any portion of it, however small, has at all times a meaning which is definite and in accordance with the laws which experience shows to hold for bodies at rest. (Bertrand's objection to the use of the word " temperature " appears, however, not to be that in a state of extreme agitation the " temperature " of a small part has not a clear meaning, but merely that its value differs from point to point.) In many cases of less turbulent motion Planck's view might be justified, and his *definition* of the entropy of a perfect gas therefore be allowed as valid.

Bertrand's objection to the term " Pressure." The Nature of the Stresses in a Viscous Fluid.

11. But in establishing the theorem that the entropy of a system *continually* increases, Planck supposes, in the argument of Arts. 2–4 above, that the system can at any time be divided into a number of small particles, each of which can be restored to its original state by a series of infinitely slow adiabatic and isothermal changes, constituting in fact a succession of equilibrium states in each of which the portion considered is subject to pressures wholly normal. Bertrand's objection to the validity of such an argument seems to have great force. It may be pardonable here to recall some of our fundamental ideas as to the nature of the stress in fluids, as the bodies discussed in thermodynamics are usually either fluids or, if solids, are subjected only to such stresses as could exist in fluids. Defining a fluid as a body having the property that

* *E. g.* Nansen, 'Farthest North,'

when not undergoing changes of form the stress at any point across a plane is wholly normal we deduce that in a fluid at rest the stresses across all planes through a given point are at that point of the same intensity, and accordingly to the value of this stress we give, and are justified in giving, the name " pressure at the given point."   But in the case of an actual fluid whose parts are in relative motion, it is no longer the case that the stress across every plane is normal: this is only true for the fictitious ideal "perfect fluid," which we *define* as having this property; (a " perfect gas " as usually defined is not, and indeed according to the kinetic theory a gas cannot be, a " perfect fluid ").   According to any of the various theories of stress in a fluid whose parts are in relative motion which have been constructed by O. E. Meyer, Navier, Poisson, St. Venant, Stokes, and others, through a given point there can be drawn three, and in general only three, planes such that at that point the stress across each is normal; the arithmetical mean of these three stresses, which are in general unequal, is what in such cases is conventionally called " the pressure at the given point."   Furthermore, whether this " pressure " is the same as if the fluid in the neighbourhood of the point were in a state of equilibrium at the same temperature and density, is a question whose answer, in the case of a gas at all events, is doubtful [*], and even whose meaning, owing to the difficulty of defining temperature in such cases, is not absolutely clear.   At all events Planck apparently requires each small portion of a fluid to be passed, by means of a process at every stage of which the stresses exerted on it across its surface are wholly perpendicular to that surface, out of a state in which some of those stresses are oblique, and in the case of a highly viscous fluid may be very much so. The argument seems to involve an absolute contradiction in terms, and its validity can scarcely be admitted without further justification.   The difficulty of course cannot be got over by the consideration of a more general stress-system such as exists in a perfectly elastic solid.

[*] The question is the same as whether in a fluid expanding uniformly in all directions, this being the only case in which the stresses across all planes are normal, the pressure depends only on the density and temperature at the instant, and not on the rate of expansion. Stokes (" On the Friction of Fluids in Motion, &c.," Camb. Phil. Trans. viii.; Math. and Phys. Papers, vol. i.) answers this question in the affirmative ; Meyer (*Kinetische Theorie der Gase*, or Baynes' translation) in the negative.   Lord Rayleigh, while adopting the view of Stokes in his ' Theory of Sound,' vol. ii. chap. xix., appears more recently to doubt its correctness, in a paper " On the Cooling of Air by Radiation and Conduction, &c." (Phil. Mag. vol. xlvii.; Collected Papers, vol. iv.).

The latter stresses depend on the *amounts* of the distortions; viscous fluid stresses depend on the *rate* at which distortion is taking place.

Planck's Definition of Entropy of a Body other than a perfect gas not in a state of equilibrium.

12. Planck defines the entropy ϕ of a body other than a perfect gas by the equation: $\phi = \int \dfrac{d\mathrm{U} + p\,d\mathrm{V}}{\theta}$; according to the views put forward here even this definition is meaningless except for equilibrium states.

Values of Entropy can be compared only in states of equilibrium.

13. I submit then, following Bertrand, that no definition of entropy has yet been given (with the doubtful exception of Planck's for a perfect gas) which is applicable to any states but those of relative equilibrium, and accordingly that any statement that entropy increases should be restricted to a comparison between its values in such states.

A shorter proof of the principle of Increase of Entropy if the substance of Planck's definition of " Irreversibility " be adopted.

14. Again, the proof of the general theorem that when a system of bodies passes by means of an "irreversible" ("irreversibel") process from one state, A, to another, B*, without exchanging heat with any external body, the entropy in the final state is greater than in the initial, could apparently be put more briefly, and without recourse to the perfect-gas system, as follows :—Let each small portion of the system be restored from the state B to the state A by an isothermal adiabatic process, in which every transference of heat which takes place is to or from the working substance of a perfectly reversible engine working between the temperature of the particle considered and an external body at a fixed temperature, θ_0 (on Lord Kelvin's absolute scale). The heat received by this external body is evidently the product of θ_0 and the excess of the value of the united entropies of the system in the state B over that in the state A. The system has now passed through a cycle starting from, and ending in, the state A. The heat received by the external body therefore cannot be

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* It is supposed that for each of these states it is admitted that entropy has a meaning.

negative, as this would involve a contradiction of Lord Kelvin's version of the Second Law ; nor can it be zero, as if so the process by which the system passed from the state A to the state B would be " reversible " (" reversibel ") by definition ; it must therefore be positive and hence the result follows.

Clausius' Theorem : an Enunciation.

15. In discussions which take the course of demonstrating the proposition known as " Clausius' theorem," in some cases not only is the argument obscure or fallacious, but even the statement of the theorem is wanting in clearness. At least two slightly different forms of the proposition have been discussed : for the sake of definiteness I consider the following :—If a body or system of bodies undergo any irreversible cycle of operations in which, in addition to interchanges of heat which may take place between different parts of the system, it receives heat from (or gives heat to) any external bodies, then for the cycle $\iint \frac{\delta H}{\theta}$ is negative, where δH denotes the small quantity of heat absorbed by any small portion of the system when at the absolute temperature θ, whether this heat be supplied by conduction or by radiation, and whether by external bodies or by any other part of the system, the doubled sign of integration being used to indicate that integration is to be performed over the whole mass as well as throughout the cycle.

Preliminary Objections : in some cases the statement is meaningless.

16. The difficulty, alluded to above, of defining " temperature " in extreme cases of irreversibility, of course presents itself here too ; and there appears to be at least one further difficulty. What is meant by the " quantity of heat absorbed " and how is it measurable? The justification of the phrase " quantity of heat " and the elucidation of its meaning is another of the fundamental difficulties which a treatise on Thermodynamics must overcome. Its measurableness, like that of temperature, is complicated in the present instance by the existence of relative motions within each small part of the system. Moreover, heat is or may be generated by " friction " (to use the word somewhat loosely) within each small portion, and, as I understand the statement of the theorem, such heat is not to be included in the quantity " absorbed." If it were so included the truth of the theorem as a matter of fact, leaving aside the question of proof, would

depend solely on the irreversible transference of heat, and
the theorem would then have no reference to the generation
of heat by "friction." How is heat thus generated by
"friction" to be distinguished from heat received from out-
side? These difficulties would apparently be of less
importance in another mode of stating the theorem in which
δH and θ refer to an external system, though in that case
there are other points of difficulty to be considered.

I proceed to comment on some proofs of the theorem in
cases in which it (the theorem) may be admitted to be
intelligible.

Clausius' Proof.

17. Clausius * himself stated the proposition in the forms :—
"The algebraical sum of the Transformations occurring in a
cyclical process can only be positive or as a limiting case
zero ;" and "Uncompensated Transformations can only be
positive."

He did not explicitly state that the limiting case referred
to in the first form can only occur if the process is reversible.
He has, however, usually been interpreted in this sense, and
apparently without objecting to it. As regards proof, he
merely pointed out that the argument by which he had shown
that in a reversible cycle the algebraical sum of the Trans-
formations is zero, holds so far as to prove that in an
irreversible cycle the sum cannot be negative, but fails to
show that it cannot be positive, and that on the contrary
there may very well be a balance left over of positive trans-
formations. Not only the argument, but also the statement
seems too obscure to have any claim to general validity. One
would prefer nowadays to modify somewhat in the direction
of greater precision the language of Clausius ; in endeavouring
to make clear, however, one runs the risk of misinterpreting.
Taking, however, the most favourable view, one cannot regard
Clausius as having proved more than that in an irreversible

cycle $\iint \frac{\delta H}{\theta}$ cannot be positive and may be negative.

Parker's proof.

18. Of the discussions with which I am acquainted, the
one which appears least unsatisfactory is that given by
Parker †. Here all the heat absorbed (algebraically) by each
small portion of the system is supposed to be imparted by a

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* *Mechanische Wärmetheorie*, i. chap. x., or Browne's translation.
† 'Elementary Thermodynamics,' 1891, Art. 61.

perfectly reversible Carnot engine which continually brings it from a source whose temperature is uniform and constantly equal to θ_0. In a complete cycle the total quantity of heat taken from the source is $\theta_0 \iint \dfrac{\delta H}{\theta}$. Hence, since a system which undergoes a complete cycle cannot take heat from a body whose temperature is uniform and constant unless some other body of different temperature be also present, it is inferred that the integral $\iint \dfrac{\delta H}{\theta}$ is zero for a "non-frictional" cycle, and that for all other cycles it is negative. That the integral cannot be positive for any cycle, and hence that it is zero for a reversible cycle, is a legitimate inference from the premises; but the grounds of the deduction that it is negative for a frictional cycle are not, I maintain, stated with sufficient definiteness, especially as the precise meaning which the author attaches to the word "non-frictional" is not clear. He has previously [*] stated that "in much that follows the term 'friction' is used in a general way to include every kind of irreversibility as well as friction proper;" but, on the other hand, that a certain process [†] (the slow heating of a saturated solution of sulphate of magnesium along with an excess of salt) is "not reversible, even though it is a non-frictional process."

The author, indeed, is evidently himself dissatisfied with this discussion, as in a later one [‡] he states that "the case of an irreversible cycle cannot be discussed in its generality; but on taking some typical cases of irreversibility, as in Art. 37, it is found that $\dfrac{Q}{\theta} + \dfrac{Q'}{\theta'} + \dfrac{Q''}{\theta''} + \dots$ is negative. We therefore infer that we may put $\dfrac{Q}{\theta} + \dfrac{Q'}{\theta'} + \dfrac{Q''}{\theta''} + \dots \lessgtr 0$, according as the cycle is reversible or irreversible."

Proofs by Poincaré, Kirchhoff, Voigt, and Buckingham.

19. Poincaré proves [§] that in any cycle $\iint \dfrac{\delta H}{\theta}$ cannot be positive, not that in an irreversible cycle it must be negative.

[*] *Loc. cit.* footnote, p. 118.
[†] *Loc. cit.* p. 108.
[‡] 'Thermodynamics treated with Elementary Mathematics,' 1894, p. 102.
[§] *Thermodynamique,* Chap. xii.

His argument, which is specially designed to meet the objections of Bertrand mentioned above and alluded to by him, is much the same as Parker's but presented at tedious length.

He afterwards supposes each term of the integral divided into two, one corresponding to heat received from external bodies, the other to heat received from other portions of the system, and endeavours to establish a similar theorem for the integral of the set of terms relating to the former alone. If θ is the temperature of a body which receives heat, I do not see how the source of the heat is to be identified, unless processes of radiation are excluded ; such a distinction thus seems untenable.

Kirchhoff's proof [*] also is somewhat similar to that given by Parker, except that Clausius' version of the Second Law is adopted instead of Lord Kelvin's. It is proved only, however, that the integral cannot be positive. And it is stated that the idea of entropy applies to reversible ("umkehrbare") processes only.

Voigt adopts [†] the proof of Kirchhoff.

In the case of the above writers and continental authors generally, it appears doubtful whether they intend to assert that the integral is negative or merely that it cannot be positive.

Buckingham [‡] states that all so-called proofs which have been given of the theorem appear to contain an unproven assumption, and regards the theorem as a new experimental principle forming a pendant to Carnot's principle.

No proof that Entropy is increased by an Irreversible Process can be deduced from the Laws of Thermodynamics as usually stated.

20. It is here maintained then, in the first place, that any statement of the kind that in an irreversible process the entropy *continually* increases is meaningless ; and in the second, that in none of the discussions alluded to is the theorem that when a system passes in an irreversible way from one equilibrium state to another, without interchanging heat with external bodies, the entropy in the final state is greater than in the initial logically deduced from clearly stated premises. From the latter contention, Planck's proof must of course be excepted : it is valid only, however, because of the unusual

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[*] *Theorie der Wärme,* S. 58.
[†] *Thermodynamik,* Band i. Art. 124.
[‡] 'Thermodynamics,' p. 153.

definition of irreversibility, and, as has been pointed out above, the words in which this definition is expressed are not happily chosen. It is further contended, along with Parker and with Buckingham, that if an irreversible process be defined merely as one such that the successive states cannot occur in the reverse order in point of time, with all the motions, heat exchanges, chemical, electrical, and other changes, exactly reversed, and the mechanical forces unaltered *, it is impossible to deduce the theorem from the Laws of Thermodynamics in the form in which they are usually stated. It may be established for a few simple cases which are as much ideal as the reversible cycles usually discussed. Yet no one doubts its truth. Our belief in it seems based either on an induction from these simple cases in which it may be proved deductively, or on a deduction from premisses which include some property of irreversible (or natural) processes not stated explicitly but merely implied.

Suggested modification of the Statement of the Second Law.

21. Such a property may be stated explicitly in various forms. One form would constitute a slight amendment of Lord Kelvin's version of the Second Law. Besides stating, as that version does, that if a system in a cycle interchange heat with external bodies it is impossible that in every interchange the system should receive heat, we might add a further statement to the effect that if any process in the cycle is one which is actually possible with natural bodies, the system must at some stage give out heat. It is not of course suggested that such an addition embodies any new principle ; it is, in fact, merely another mode of stating that all actually possible processes are " irreversible " in Planck's sense (" irreversibel "), which Planck himself appears to regard as axiomatic †. For the statement that if an isolated system passes from one state to another by a process which is actually possible, it cannot, even by the aid of ideal reversible (" umkehrbare ") processes, be passed back to its original state without interchanging heat with some external body, is equivalent to the statement that if a system undergoes a

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* I do not think that there is anything in the idea of reversibility *per se* which requires us to restrict its application to a mere succession of equilibrium states. The motions of rigid frictionless bodies, or the vibrations of perfectly elastic bodies, as treated in dynamics, are instances to the contrary.

† " .... Irreversible processes, which in fact are the only real processes in Nature. ...." Electrician, Feb. 13, 1903.

cycle any part of which is actually possible it *must* interchange heat with external bodies; and by Lord Kelvin's version of the Second Law it is impossible that in all such interchanges the system should absorb heat ; it must therefore at some stage give out heat.

It is, however, contended that with a view to a proper logical development of the subject, it is absolutely necessary either to state clearly some such additional premiss, or definitely to abandon all attempts to give a deductive proof of the theorem under discussion.

Of course, the necessary emendation might be made to Clausius' version of the Second Law instead of to Lord Kelvin's.

The statement is frequently made that all operations in Nature are frictional*. In the endeavour to attach any precise meaning to this statement or to explain how its truth or falsehood could be tested, we should, I think, fall back on some such statement as that suggested above.

An Irreversible Process in which effects of Inertia and of Irreversibility may be confounded.

22. Attention may here also be drawn to a point in the description of a certain class of irreversible processes in which the student might be misled by some writers. In discussing the expansion of gas in a cylinder it is frequently said that the external pressure is less than the internal. The phraseology in some cases might even suggest to the reader that " action " and " reaction " are unequal. In such processes the effects of inertia and of irreversibility are necessarily blended together, but some care should be taken to endeavour to distinguish between them, otherwise the student is likely to attribute to the former results which are really due to the latter. To illustrate the point, consider, for example, the definite statement that if steam expands with more or less suddenness in a cylinder whose walls are impermeable to heat, the work done by the steam on the piston is less than that which it would perform in an indefinitely slow adiabatic expansion starting with the same initial pressure, volume, and temperature, and ending with the same final volume. In so far as this statement is based not on direct experience but on any general theoretical considerations, it cannot, I conceive,

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* *E. g.* Planck, *l. c.* Art. 115 :—" Since there exists in nature no process entirely free from friction or heat conduction...." Parker, ' Elementary Thermodynamics,' 1891, p. 142 :—"As no process in nature can exactly be a non-frictional process...."

be deduced from the principles of mechanics. For the force
which the steam exerts on the moving piston is not necessarily
always less in the former expansion than that at the corre-
sponding stage in the latter. In the first expansion, the laws
of mechanics tell us indeed that when the velocity of the
steam is being accelerated, the force which the steam exerts
on the piston is less than that which it exerts on the fixed end
of the cylinder, and the fact that the sides of the cylinder exert
friction on the steam shows that the difference is greater than
the mass of the steam multiplied by its average acceleration ;
the same laws tell us also, however, that towards the end of the
expansion, when the velocity is diminishing, the force on
the piston may exceed that on the fixed end, and would do so
by the mass of the steam multiplied by its average retardation
were it not that the friction between the sides of the cylinder
and the steam now tends to equalize these forces. Mechanical
principles may thus enable us at any stage in the former
expansion to compare the force which the steam exerts on the
moving piston with the force which it exerts on the fixed end of
the cylinder ; but they do not enable us to compare either of
these forces with the force exerted on the piston in the other
expansion. It is thus quite possible that towards the end of
the former expansion the driving-force on the piston may
exceed that in the latter, especially since, as a matter of fact,
the steam in the final stages is on the average hotter in the
former. Mere mechanical considerations thus do not enable
us to say in which case more work is done on the piston, even
if we do take account of the friction of the cylinder-sides.
To settle the question we must invoke the principles of
Thermodynamics. Let us suppose that the steam firstly
undergoes the sudden expansion, secondly, settles into
equilibrium at the end of it, thirdly, is then brought by con-
tinuous heating or cooling at constant volume into the state
which it would have had at the end of the indefinitely slow
adiabatic expansion, and, finally, is restored to its original
state by an indefinitely slow adiabatic compression, the reverse
of the slow expansion. The steam has now undergone a cycle
and must therefore, by the principle of irreversibility [*], have
actually at some stage given out heat ; that is to say, the third
process is actually one of cooling : hence, from the First Law,
work must have been done on the steam in the cycle as a whole ;
that is, the work which it does in the sudden expansion is
less than that done on it in the indefinitely slow compression,

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[*] *I. e.* the principle stated in the preceding section.

which is of course the same as that which it would do in the indefinitely slow expansion.

In some instances, however, is there such an obscurity and inversion of ideas, as it seems to me, on this point, that Verdet *, followed by Jamin and Bouty †, in the endeavour to prove Clausius' theorem for irreversible cycles, classifies such cycles in several types, one of which consists of "those in which the body expands without developing a quantity of sensible energy equal to the work of its elastic force, that is to say, where the pressure which it has to overcome is sensibly inferior to its own pressure and virtually reverses the preceding argument."

Objections to treating some problems of Chemical Equilibrium by Method of Entropy, Thermodynamic Potential, or Available Energy.

23. The view held by Bertrand and urged here as to the conception of entropy constitutes to some extent an objection to the application of the principle of the Increase of Entropy, whether by Gibbs' method of Thermodynamic Potential or von Helmholtz's of Free, or Available, Energy, to a certain class of chemical problems, viz., those which deal with the internal equilibrium of a phase. The conception, as a measurable quantity, of the Thermodynamic Potential or of the Available Energy of a given state requires us to suppose that that state can be obtained in a reversible way from a certain standard state of equilibrium. The method then implies that we can conceive the possibility of passing by reversible processes to states wherein the bodies concerned are not in chemical equilibrium : as a specific instance, the usual treatment of the question of equilibrium in a gas system requires us to suppose that the various gases can diffuse into each other through semipermeable membranes in proportions other than those appropriate to the equilibrium state, for the reversible way in which the gases are supposed to be mixed or to have their relative concentrations altered is by being passed through such membranes.

It may be held that this method is only competent to decide which of two or more given possible states of equilibrium is the most stable.

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* *Théorie Mécanique de Chaleur*, Œuvres, tome 7, Art. 164, p. 188.
† *Cours de Physique*, ii. 2ᵉ fascicule, p. 139.

*Moreover, such methods, even if valid, are unnecessarily
complicated in principle.*

24. Again, as regards any useful deductions, Thermo-
dynamics appears as yet to have gone no further than to
compare one state of equilibrium with another, the two states
corresponding to different values of the independent co-
ordinates of the system, such as pressure, temperature,
quantity of each ingredient, &c. The methods referred to
institute such comparisons not directly, but by the devious
process of comparing each equilibrium state with all the
non-equilibrium states possible for the same values of these
independent coordinates.

*Such methods mathematically difficult. Carnot's cycles
preferable.*

25. I venture here to put in a plea for the treatment, at
least in text-books, of such problems as those alluded to by
the aid of Carnot's cycles as applied, for example, by Van
t'Hoff *. I do so not so much on the ground of the above
objections, as on that of the greater simplicity and directness
of the latter method. Many readers who could readily follow
reasoning based on reversible cycles are quite unable to
understand the other methods. Elementary methods are
conducive to clear thinking on the part of all, teacher as
well as student, the expert mathematician no less than the
tyro. And in no other branch of mathematical physics are the
difficulties which necessarily have to be faced, so greatly those
which are incidental to clear thinking and so little those of
mathematical analysis. The method of Thermodynamic
Potential or of Available Energy appears as unnecessary for
the discussion of many such problems as does the dynamical
principle of Least Action for the investigation of the motion
of a body sliding down an inclined plane. And the use of
the more elementary method would obviate the pitfalls into
which writers who are not trained mathematicians are
occasionally betrayed in handling the other methods.

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* For an application of this method to the question of equilibrium in
a gas-system, see Van t'Hoff, *Vorlesungen über die Theoretische Chemie*,
or Lehfeldt's translation, p. 105. The investigation of the effect of the
change of temperature on the state of equilibrium, *l. c.* p. 141, is,
however, wanting in clearness. And the corresponding results in cases
of solutions, in so far as that writer bases them on thermodynamic
principles at all, appear to be merely written down from analogy, though
it is stated that they can be obtained by similar methods, as is of course
the case,

L. *Plan of a Combination of Atoms to have the Properties of Polonium or Radium.* *By* Lord Kelvin *.

§ 1. THE properties to be explained are :

    (1) To store a large finite amount of energy in a combination having very narrow stability.

    (2) To expend this energy in shooting off with very great velocity, vitreously and resinously electrified particles.

§ 2. In the title of the present communication, Polonium means a substance which shoots off vitreously electrified particles abundantly and with very great velocities ; but few or no resinously electrified particles. Radium means a substance that shoots off in extraordinary abundance both vitreously and resinously electrified particles. From the kinetic theory of gases, it seems certain that every kind of matter has some radioactivity : that is to say, shoots off both vitreously and resinously electrified particles. Hence it is only in their extraordinarily great abundance and great velocities of shooting, that Polonium and Radium differ from ordinary matter.

§ 3. In the present communication I use the word electrion to signify an atom of resinous electricity, according to a suggestion given in a communication to ' Nature,' May 27, 1897 : and I use the suggestions regarding atoms of ponderable matter and electrions, which I first proposed in an article under the title " Aepinus Atomized " † in the jubilee volume, presented to Professor Boscha in November 1901.

§ 4. A plan for molecular structure of Polonium is represented in fig. 1, and may be shortly described as two void atoms held together against their mutual repulsion by a bond consisting of one electrion. A plan of molecular structure for emission of the β rays of Radium is represented in fig. 2, and may be shortly described as two electrions held together against their mutual repulsion by a bond consisting of one void atom.

§ 5. In fig. 1 the quantity of vitreous electricity belonging to each of the void atoms is four times the quantity of resinous electricity, e, belonging to an electrion. The quantity of vitreous electricity belonging to the single void atom of fig. 2 is $\frac{1}{4}e$.

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* Communicated by the Author.

† Reproduced Phil. Mag. for March 1902, and Appendix E of my recently published volume of Baltimore Lectures. This article will be referred to in the text as " Aep." for brevity.

§ 6. Alter fig. 1 slightly to make the two circles touch one another with the centre of the electrion, *e*, at the point of contact. The electrion still experiences equal attractions leftwards and rightwards and is therefore in equilibrium.
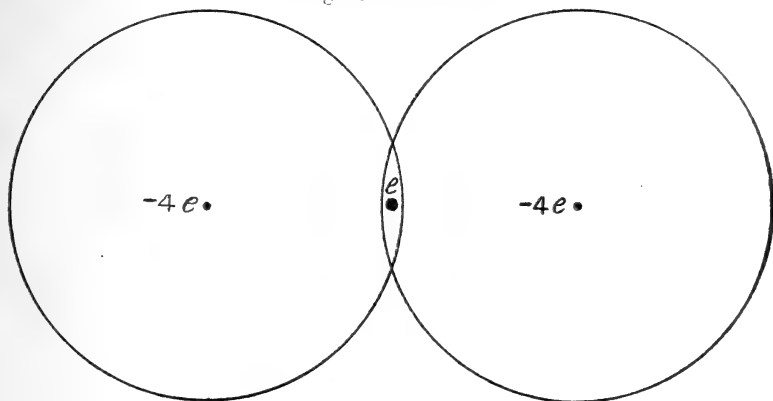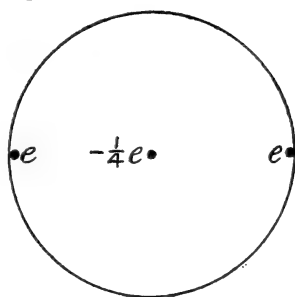
Fig. 1.—Polonium.

Fig. 2.—For *β* rays of Radium.

Each atom experiences repulsion from its neighbour, which, if *r* denotes the radius of the atom, is equal to $\dfrac{4e \cdot 4e}{4r^2} = \dfrac{4e^2}{r^2}$;

and attraction by the electrion equal to $\dfrac{4e \cdot e}{r^2} = \dfrac{4e^2}{r^2}$. These forces being equal, each atom is in equilibrium. But the equilibrium is unstable : to prove this; separate one atom by a slight distance from the other, leaving the electrion in the other, free to move while the two atoms are held fixed. The electrion will be left vibrating, through a small range, wholly within the last-mentioned atom ; and, by sending out waves through ether, will come to rest at a small distance

within this atom.   The void atom will now experience
diminished repulsion from the other atom, and an attraction
towards the electrion diminished by a greater difference.
Hence repulsion will predominate, and if the system is left
free, the two atoms will separate to an infinite distance, the
electrion remaining always within one of the two.   The
whole work done by the excess of repulsion above attraction
will be spent in the generation of etherial waves, and uniform
motion through ether, of the void atom and of the other atom
with the electrion settled at its centre.   For brevity, and to
keep as nearly as possible in harmony with the language
of J. J. Thomson, Rutherford, and other writers on the
dynamics of radioactivity, I shall call this action, by which
two atoms are sent flying asunder with very great velocity,
an explosion.

§ 7. To find the work done in this particular kind of ex-
plosion : first separate the two atoms, leaving the electrion
in the middle between them.   The attraction of the electrion
on each atom $\left(\dfrac{4e^2}{r^2}\right)$, will exactly balance the repulsion $\left(\dfrac{16e^2}{4r^2}\right)$
on it by the other atom ; and therefore no work is done.
When the two atoms are at a very great distance, bring the
electrion slightly nearer to one atom than to the other and
leave all free.   The electrion will be drawn towards the
nearer atom and will ultimately settle at its centre.   The
work done in this action (Aep., Table * of § 20) will be

$$\frac{3}{2} \cdot \frac{4e^2}{r} = \frac{6e^2}{r}.$$

Hence this is equal to the work done in the explosion of § 6,
because the initial and final configurations of atoms and
electrion are the same in the two cases.   We may make r as
small as we please, and so make the energy of the explosion
as great as we please.

§ 8. Similar considerations show that if e and e are placed
on the circumference of the circle in fig. 2 instead of slightly
within it, the configuration is unstable and is liable to an
explosion in which one of the electrions e is shot off to an
infinite distance, while the other settles at the centre of the
atom.   And just as in § 7, we find that the work done in this
explosion is equal to the work required to extract the elec-
trion from the centre of the atom and carry it off to an

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* In this table a denotes the radius of the atom instead of r as at
present : and e is the quantity of vitreous electricity belonging to the
atom instead of 4s as at present.   Thus instead of e^2 we have 4e^2.

infinite distance : which is

$$\frac{3}{2} \cdot \frac{\frac{1}{4}e \cdot e}{r} = \frac{3}{8} \cdot \frac{e^2}{r}$$

(Aep., Table * of § 20).

§ 9. In fig. 1 the total quantity of the two electricities is $8e$ of vitreous and e of resinous. Hence to make a neutral or unelectrified combination of atoms and electrions we must add a combination electrically equivalent to 7 electrions. If we simply placed seven electrions in the neighbourhood of the combination shown in fig. 1, they would instantly explode into the atoms : and the thus augmented combination might ultimately settle in two tetraelectrionic atoms moving from one another with some finite velocity, and each having its quartet in one of the stable configurations of equilibrium of four electrions within it, (Aep. § 17). Or it might settle into any of a great number of possible configurations of two overlapping tetraelectrionic atoms with 8 electrions in some configuration of stable equilibrium within them. In any of these results the explosive energy for which we are planning is lost. We must therefore find another plan for supplying the $7e$ of resinous electricity. Any such plan involves essentially the addition of 8 or more electrions. We might try one atom containing vitreous electricity equal in amount to one electrion, and try to charge it with 8 electrions : which we should almost certainly find impossible. The simplest plan really is to take fourteen atoms each possessing vitreous electricity equal to $\frac{1}{2}e$, and place within it one electrion. This would add to our vitreously electrified explosive combination represented in fig. 1, $14e$ of resinous electricity and $7e$ of vitreous ; and would so add an electrical equivalent of the required $7e$ of resinous electricity to make up a non-electrified explosive combination.

§ 10. These fourteen atoms may be first put together in two groups of seven as shown in fig. 3 (p. 532), and then applied symmetrically on the right-hand and the left-hand sides of fig. 1, in planes perpendicular to the axis. By making these atoms very large in comparison with the two atoms of fig. 1, we avoid any great interference with the forces described in §§ 6, 7, 8 ; and by placing them so as to overlap one another slightly and to make the central atoms of the two groups of seven overlap slightly the atoms of fig. 1, we can, according to the last four lines of Aep. § 4, give any mutual forces we

* In this table the quantity of vitreous electricity belonging to the atom is e instead of $\frac{1}{4}e$ as at present. Thus instead of e^2 we have $\frac{1}{4}e^2$.

please between the atoms in order to secure stability of the group of sixteen.   The fourteen electrions will find places of stable equilibrium within them, not disturbing at all the electrion shown in fig. 1 : because they repel it equally in opposite directions, and with such small forces that they do not render its equilibrium unstable.   Thus we have a beautifully symmetrical explosive group of fourteen large atoms and two small atoms containing in all fifteen electrions in positions of stable equilibrium within them.

Fig. 3.—Neutralizing preservative guard for Polonium molecules.
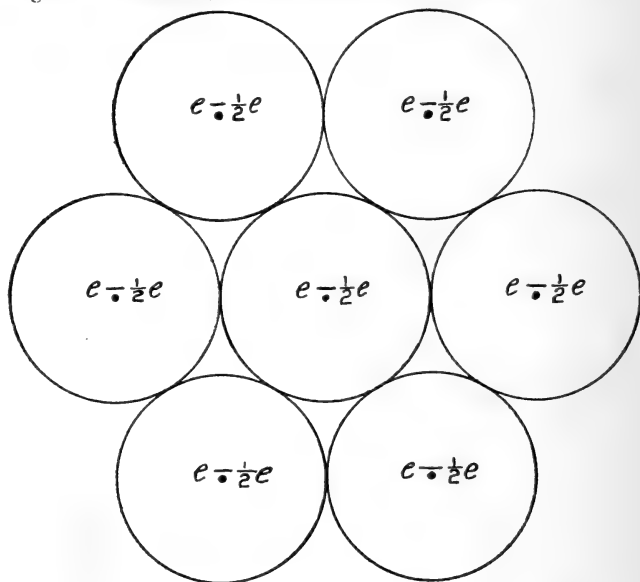
§ 11. The limits of stability of the equilibrium of the central electrion and the two small overlapping atoms which contain it as shown in fig. 1, are so narrow that a shock of a very slight but suitable kind, will produce an explosion shooting out these two atoms in opposite directions with prodigious velocities, one of them carrying the central electrion with it.   Each of them will probably shoot through the neighbouring guard atom, without carrying its electrion away.   Thus both of the atoms shot away will be found vitreously electrified : one with a quantity $4e$ of vitreous electricity, the other only $3e$, because it carries an electrion (e of resinous electricity) with it.   This agrees perfectly with the behaviour which experiment has proved for Polonium.

§ 12. Going back to fig. 1, alter to make the two circles touch one another in *e*; and for simplicity suppose the two atoms held fixed in this position; *e* is unstable in the central position, but if disturbed ever so little to either side it will vibrate between the two; and by giving out etherial waves will come to rest in either of the atoms at the point in the line joining the two centres at which the attractions on the electrion are equal and opposite. The distance *x* of this point from either centre is given by the equation

$$\frac{x}{r} \cdot \frac{m}{r^2} = \frac{m}{(2r - x)^2},$$

where *m* denotes the quantity of vitreous electricity belonging to each atom. This is a cubic equation of which one root is $x = r$: its greatest root does not belong to this problem: and its least root is $x = r(3 - \sqrt{5})/2 = \cdot 382r$, which is the required distance of the position of stable equilibrium from the centre of atom in which the electrion rests.

§ 13. Considering now the β rays of Radium, look to fig. 2. We have 2*e* of resinous electricity and $\frac{1}{4}e$ of vitreous: requiring $\frac{7}{4}e$ of vitreous for neutralization. The simplest way of applying this, which has also the advantage of converting into stability the instability of the electrions of fig. 2, is to take an atom of very large radius endowed with vitreous electricity to an amount $\frac{7}{4}e$, and place it concentric with the smaller atom shown in fig. 2. This addition gives us a non-electrified combination of two atoms and two electrions all in stable positions with the electrions slightly inside the boundary of the smaller atom as shown in fig. 2. For brevity denote by A the smaller atom and by B the larger: and by R the radius of the larger.

§ 14. The larger is R/*r*, the smaller are the distances of the two electrions from the boundary of A inwards; and the smaller and the narrower is the range of their stability; and the more nearly correct is the estimate $\frac{3e^2}{8r}$, in § 8 for the energy of the explosion, when an explosion takes place. By making *r* small enough we may make the explosive energy as great as observation shows it to be.

§ 15. Thus in §§ 10, 13 we have two un-electrified molecules, which, if put together in any substance, would give it the α, β properties of Radium. There are many other plans, some no doubt very much simpler than the combination of these two now suggested, for a combination of atoms to give the properties of Radium. It is indeed easy enough to design a single atom possessing vitreous electricity in a fixed

distribution of equal density at equal distances from the centre, into which a neutralizing quantum of electrions could be shot and come to rest in such a configuration that, in the presence of other atoms or disturbing electrions, it would act as Radium does. This is in fact done for the β rays of Radium in § 13 above : because the two atoms A, B there put together concentrically may be supposed fixed relatively to one another and called one atom.

LI. *On Electric Insulation in "Vacuum"* *.
By Lord Kelvin †.

§ 1. IT has long been well known that difference of electric potential between conductors in a high vacuum is maintained without appreciable current, even when the distance between them is a small fraction of a millimetre. Fifty or sixty years ago, when we had no experimental knowledge of what is now called a high vacuum, it was a vexed question whether vacuum is an insulator or a conductor. In a Royal Institution Friday evening lecture of May 18th, 1860 ‡, I find that I made the following statement :—" It has been supposed, indeed, that outside the " earth's recognised atmosphere there exists something or " nothing in space which constitutes a perfect insulator ; but " this supposition seems to have no other foundation than a " strange idea that electric conductivity is a strength or a power " of matter, rather than a mere non-resistance."

§ 2. The labours of many experimenters during the last fifty years, and the comparatively modern atomic theory of electricity, have thoroughly confirmed the view that the space of our best modern vacuum, and interstellar and interplanetary space, and generally, space occupied only by the all-pervading luminiferous ether, is a *very perfect non-resister* of electricity passing through it.

§ 3. Hence we see that the insulation of electricity in " vacuum " is to be explained, not by any resistance of vacant space or of ether, but by a resistance of glass or metal or other solid or liquid against the extraction of electrions from it, or against the tearing away of electrified fragments of its own substance. The kathode torrent of resinously electrified particles, discovered in 1871 by Varley, rediscovered eight years later by Crookes, and generally accepted as a truth

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* By " vacuum " I mean space occupied only by the luminiferous ether.
† Communicated by the Author.
‡ Sir William Thomson's ' Electrostatics and Magnetism,' § 281.

some eight years later still, has in many discussions and speculations been attributed to the tearing off of portions of the solid metallic kathode. But I believe the most modern and best experiments * tend rather to show that it consists solely or chiefly of atoms of resinous electricity (electrions as I call them).

§ 4. However this may be, it is quite certain (if we accept the atomic theory of electricity as true) that the extraction of an electrion from the atom is opposed by a definite permanent force which must be overcome before the electrions can be drawn out. But it may be true, and probably is true in many cases of the loss of resinous electricity from a solid, that the forces called into play may be great enough to tear away the atom, with or without its electrion or electrions, out of its place in the solid. This, however, would not contribute to the transference of electricity from the solid : in other words, Varley's torrent may contain non-electrified particles, or vitreously electrified particles, along with his negatively electrified particles which we now believe to be atoms of electricity.

§ 5. It is conceivable also that an atom may, by electrostatic force, be extracted from a solid metallic anode : and its electrion or electrions left behind in the anode. In this case the electric current would consist partially, if not wholly, of vitreously electrified particles : but I believe there is no experimental evidence in support of this supposition. Perhaps there is decisive experimental evidence against it. In the case, however, of a liquid anode, or kathode, whether of a non-conductive substance such as oil, or conductive such as liquid mercury or other melted metal, there is a lifting of spray or spindrift from the liquid surface, if the electrostatic force is strong enough : and this gives something of an electric current of vitreously electrified particles from the anode.

§ 6. To form some idea of the force required to pluck an electrion out of an atom of the metal of the kathode in a very high vacuum : suppose the vacuum so high that no current, nor torrent of sparks, passes between two blunt-ended electrodes of thick straight wire, $\frac{1}{48}$ mm. asunder, when the difference of potential between them is raised to 200,000 volts. The electrostatic force between the middles of their ends will be approximately uniform through the intervening space : and will amount to 96,000,000 volts per cm. ;

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* J. J. Thomson, 'Conduction of Electricity through Gases,' §§ 50, 279 ; and 'Electricity and Matter,' pp. 86, 87.

or 320,000 C.G.S. electrostatic. Taking with this **J. J.** Thomson's most recent estimate * $e = 3.4 . 10^{-10}$ C.G.S. electrostatic, for the quantity of resinous electricity in an electrion, we find $109 . 10^{-6}$ dyne as the force which a single electrion would experience in the electrostatic field between the electrodes in these circumstances.

§ 7. Consider now a single mono-electrionic atom having a single electrion within it, in equilibrium in the centre of the field. Let r be the radius of the atom, and x the distance from its centre, at which the electrion rests. The electro-static force at distance x from the centre is $\dfrac{x}{r} . \dfrac{e}{r^2}$, and therefore if the force of the external field is just sufficient to make $\dfrac{x}{r} = \dfrac{1}{9}$, we have $\dfrac{3.4 . 10^{-10}}{9 r^2} = 320{,}000$. This gives $r = 1.1 . 10^{-8}$ †.

§ 8. Consider next an equal and similar atom in the extreme front of the kathode. Its electrion will certainly be drawn to a considerably greater distance from its centre than $\frac{1}{9} r$; because it is backed by atoms behind it with their electrions pulled forward : it is probable, however, it could not be quite extracted from the atom without a greater electrostatic force than that considered in § 6. But it seems to me certain, from some imperfect mathematical reckonings which I have made, that from two to four or five times that force would suffice to do so. We shall guess it as 1,280,000, being four times that force : though the actual amount required is calculable and would certainly be different for different possible crystalline con-figurations of the molecules in the kathode. Thus, merely as an illustration of the orders of the magnitudes concerned, we shall assume that, with $2.2 . 10^{-8}$ for the diameter of the atom and $3.4 . 10^{-10}$ C.G.S. for the quantity of vitreous elec-tricity in the atom and of resinous in the electrion, an electrostatic force of 1,280,000 C.G.S. in the ether in front of the kathode would break down the insulation by drawing off electrions from the outlying atoms of the kathode.

§ 9. Leaving atomic considerations for a moment, remark that, per unit area, the outward attraction experienced by a metallic surface under the influence of electrostatic force **R** in the air, or the ether, outside is $R^2/8\pi$. This with $R = 1{,}280{,}000$ gives 6.519×10^7 dynes or approximately 66.4 tons weight per square centimetre. The breaking weight of the strongest steel wire scarcely amounts to 20 tons

* 'Electricity and Matter,' p. 78 (1904).
† See Baltimore Lectures, Lect. xvii. § 80.

per square centimetre.   Hence the thick straight wire of § 6 would be broken or would have its electrified end shattered and pulled away in fragments by the electrostatic force suggested at the end of § 8.   It would, however, bear without breaking, and possibly without any disintegration of its electrified end, the 320,000 C.G.S. of § 6, which would only strain the wire with a force of 16·6 tons weight per square centimetre.

§ 10. Moderate permissible changes in our guess-work assumptions regarding sizes and electric quality of atoms (mono-electrionic or poly-electrionic) might no doubt readily be devised to make the discharge of electrions take place with increasing electrostatic force, before disintegration of either kathode or anode is produced.   We have as yet no sure experimental evidence as to what would take place in the perfect vacuum (only ether, no ponderable atoms, in the space between the electrodes) which is our present subject.   What has been observed in respect to the highest of modern vacuums (from one one-millionth to one two-hundred-millionth of an atmosphere by the Macleod gauge), shows that a much greater difference of potential than 100,000 volts (which is so far as I know the highest hitherto measured electrostatically) may be maintained between two metallic electrodes without producing a manifest discharge through the "vacuum," even when the electrodes are brought within less than 1 mm. of contact.   And when a discharge does take place it is I believe not generally direct between the nearest points of the ends of the electrodes, but in wildly erratic lateral courses, attributable to residual gaseous molecules, according to J. J. Thomson's experimental and theoretical investigations on the passage of electricity through gases.

§ 11. In the experiments by which Varley discovered the kathode torrent of resinously electrified particles, the differences of potential used were those of a Daniell's battery of from 307 to 380 Daniell's elements. The fact that such small electrostatic forces produced luminous discharge, proves that his vacuum was very far from being what is now called a high vacuum : and proves that the molecules of the residual air were largely concerned in all his results. It is exceedingly interesting to learn from J. J. Thomson's experiments of 1897, described in § 50 of his 'Conduction of Electricity through Gases' that, great though the influence of the residual gas (air or hydrogen or carbonic acid gas) was in respect to the results, the virtual mass of the resinously electrified particle in the kathode torrent is the same for the different gases ; and is about 1/770 of that of the hydrogen

atom ; and is so small that he was led to believe it to be an
atom of resinous electricity unloaded with ponderable matter.
It is also very interesting to know from J. J. Thomson's
experiments and from a continuation of them by H. A.
Wilson *, that the virtual mass of the particles of the torrent
from the kathode, was the same whether the metal of the
kathode was aluminium, platinum, copper, iron, lead, silver,
tin, or zinc.   This strongly corroborates Thomson's original
conclusion that Varley's † "*attenuated particles of matter*
"*projected from the negative pole* by electricity in all directions,"
are atoms of resinous electricity.

§ 12. It is very much to be desired that careful experiments
with the very highest obtainable vacuum should be made to
ascertain the greatest steady, measured, difference of poten-
tials ; that can be maintained with or without any measurable
electric current between two metals separated by a very short
length of vacuous space.

LII. *On the Stretching and Torsion of Lead Wire beyond*
the Elastic Limit.   By Prof. TROUTON, *F.R.S., and*
A. O. RANKINE ‡.

[Plates XIV. & XV.]

THE behaviour of wires or rods when stressed longitu-
dinally or torsionally beyond the elastic limit is of a
complicated nature.   The general character of the effects
observed on the application of such a stress is as follows.
First there is the immediate effect followed by an increase
with time.   The latter initially may be considerable, but
gradually diminishes with time to what appears to be a small
constant amount.   It is convenient to refer to the first as the
primary strain, to the final constant rate as the viscous flow,
and the remaining intermediate effect as the secondary strain.
What follows refers mostly to the secondary strain.

The method of procedure in investigating this question
has apparently always been to apply a constant stress and to
observe the rate of change in the resulting strain.   Or after-
wards to remove the stress and examine the rate of recovery.
This method has been adopted for instance in the investigation
of torsional strains in wires.   We also have used it, but in
addition we have used the method in which a constant *strain*

is produced and the rate of change in the stress required for preserving it observed. We have not so far made experiments on the rate of disappearance of the stress after the obliteration or forcible removal of the strain, the case corresponding to the recovery from strain on the removal of the stress referred to above.

The investigation of the changes in the stress of an overstretched or over-twisted wire when the strain is kept constant would appear to be fundamental. Here the movements which have to be considered can only be of the nature of internal rearrangements of the particles, the material on the whole never changing its position.

The experiments which we have made may be divided into three parts. First, a determination of the rate of decrease of the restoring couple on a wire kept twisted through a constant angle. Second, a determination of the decrease of longitudinal stress in a wire kept at constant length. Third, a determination of the rate of stretching under a constant force and the rate of recovery on removing the force.

1. *Rate of Decrease of Couple in a Wire kept twisted through a Constant Angle.*

A copper rod was soldered to each of the ends of a lead wire about 1 m. long and 1 mm. in diameter. It was then suspended, the top copper rod being fixed. The lower one was made T-shaped for the application of a couple. It was necessary to have some means of keeping the wire accurately twisted through a fixed angle. The angle of twist was for this purpose observed by aid of a lamp and scale and a mirror attached to the lower rod. The couple was applied by means of the apparatus A A (fig. 1, p. 540), which simply consisted of weights hung from the points CC, which could be pulled aside by horizontal fibres fastened to the arms DD of the rod at the end of the wire under test. The couple is very approximately proportional to the deflexion of the points BB on the scale. As the restoring couple in the wire diminished further twisting began to take place, and the angle of twist was kept constant by moving the stands AA inwards, thus reducing the applied couple. The rod E was kept in a fixed position by means of the wire F passing through a small circular hole (but not touching the sides), and by always moving the stands AA in such a manner as to keep the wire F always central in this hole.

Owing to the necessity of fastening the fibres on to DD after twisting, it was impossible to read the value of the couple immediately after twisting, and the first reading was
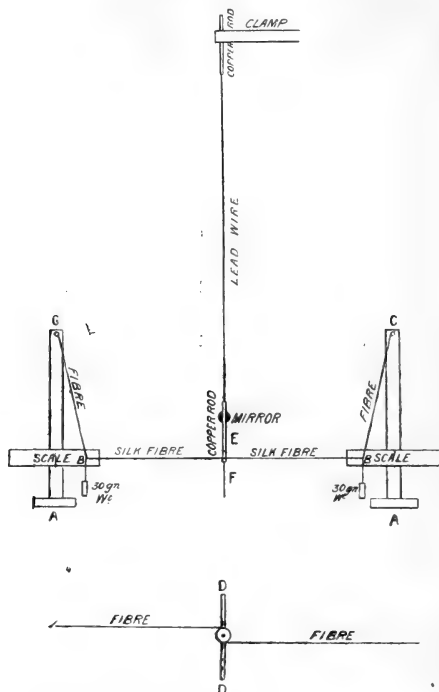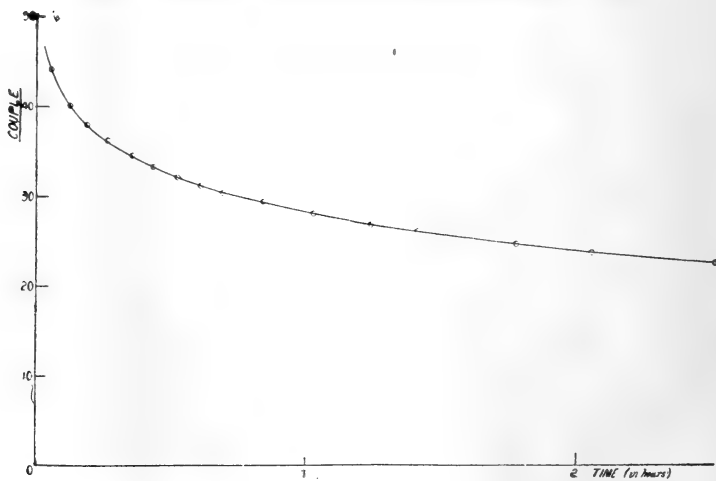
Fig. 1.

Fig. 2.—Decrease of Couple when kept at Constant Twist.

taken 3·3 minutes later. The amount of the twist was 360°, and the couple was measured as the mean of the two deflexions on AA. The curve obtained is shown in fig. 2, and to it was fitted the equation

$$G = 28 \cdot 3 - 12 \cdot 9 \log t,$$

where t is measured in hours and G in divisions of scale. The values obtained from this equation are given in the last column.

		DEFLEXIONS.					Value from Equation.
Time.	t (in hours).	I. (zero = +0·5).		II. (zero = +0·5).		Mean.	
		Reading.	Corrected.	Reading.	Corrected.		
h m							
1 15	0						
1 18·3	0·055	45·0	44·5	44·0	43·5	44·0	44·8
1 22·5	0·125	41·2	40·7	40·0	39·5	40·1	40·0
1 26·4	0·190	39·1	38·6	37·7	37·2	37·9	37·6
1 31·0	0·267	37·2	36·7	36·0	35·5	36·1	35·7
1 36·7	0·361	35·6	35·1	34·2	33·7	34·4	34·2
1 41·1	0·435	34·4	33·9	33·0	32·5	33·2	33·0
1 46·5	0·525	33·2	32·7	31·8	31·3	32·0	31·9
1 51·7	0·611	32·2	31·7	31·0	30·5	31·1	31·1
1 56·5	0·691	31·4	30·9	30·2	29·7	30·3	30·5
2 5·6	0·843	30·3	29·8	29·3	28·8	29·3	29·2
2 16·8	1·03	29·2	28·7	28·0	27·5	28·1	28·1
2 29·7	1·24	28·0	27·5	26·8	26·3	26·9	27·1
2 39·8	1·41	27·2	26·7	26·0	25·5	26·1	26·4
3 1·8	1·78	25·7	25·2	24·7	24·2	24·7	25·1
3 18·2	2·05	24·9	24·4	23·5	23·0	23·7	24·3
3 46·7	2·53	23·5	23·0	22·3	21·8	22·4	23·1
4 20·5	3·09	22·5	22·0	21·3	20·8	21·4	22·0
Next day							
9 44	20·48	9·7	9·2	8·5	8·0	8·6	
1 53	24·6	11·4	10·9	10·0	9·5	10·2	10·4
4 6	26·8	10·7	10·2	9·5	9·0	9·6	9·8

With the exception of the reading at 9·44 all these points lie very approximately on the curve of the equation given. This particular reading is faulty owing to the fact that overnight the wire was left under the action of a couple 21·4, and became twisted some eight or nine degrees more than 360° in consequence. On the sudden release to 360° the restoring couple appears to be less than would be the case at the same time had the twist been kept constant throughout the night. The two further readings on the second day, during which the wire was again kept at constant twist, are

very nearly those that would be expected from the equation

$$G = 28 \cdot 3 - 12 \cdot 9 \log t.$$

If this equation truly represents the rate of decrease, then it would mean that the couple immediately after the twist is produced is infinite. It is therefore more probable that the curve, if exponential at all, is so to an axis $t = -$ (a small quantity) instead of $t = 0$. It would be difficult to find the actual couple at $t = 0$ as the rate of change is then so great. Even at the time of the first reading the rate of change was very large, so that it was difficult to keep the spot of light on the scale stationary. This would tend to make the reading rather less than the actual couple at the time (as in the case of the first reading on the second day).

As far as the observations themselves go the equation appears to very fairly represent the curve, the variation being at no point more than 3 per cent.

2. *Rate of Decrease of Stress in a Wire kept stretched longitudinally at a Constant Length.*

An investigation was made of the rate at which the stress falls off in a wire kept stretched at a constant length. The
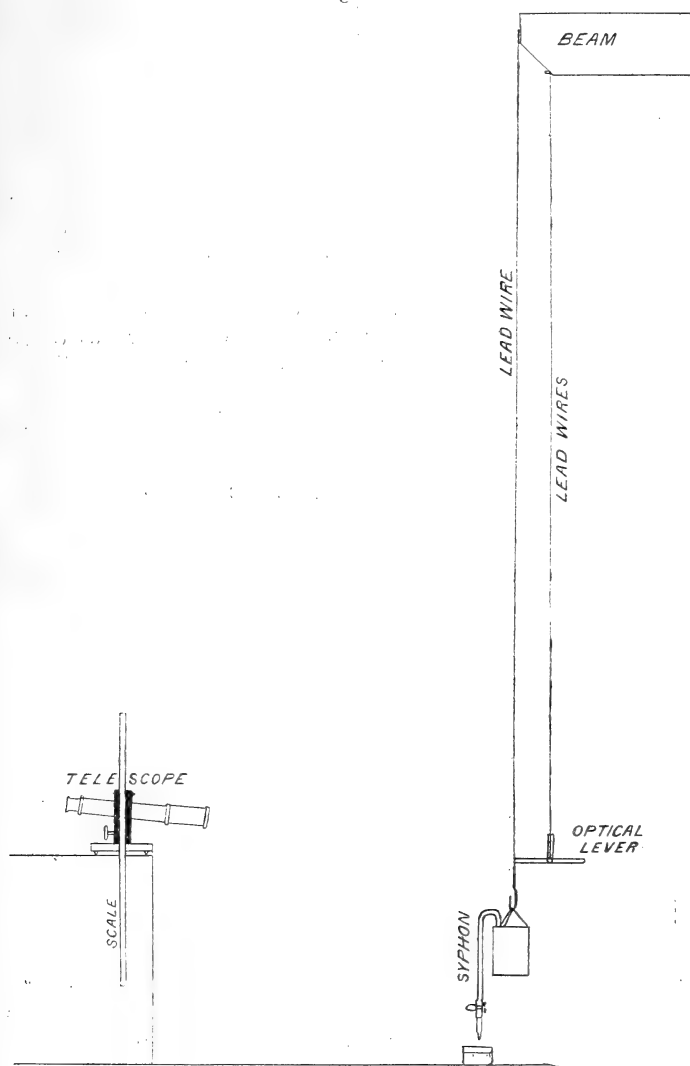

Fig. 3.
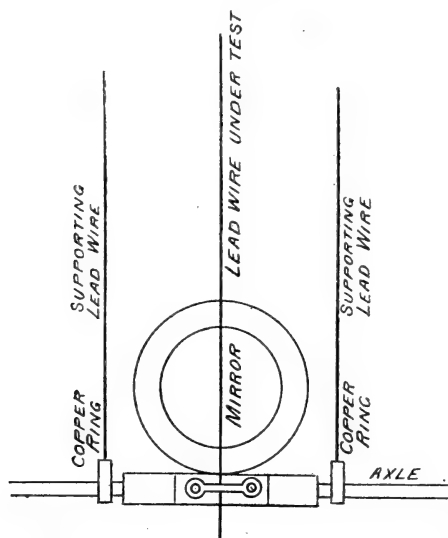
OPTICAL LEVER (ON LARGER SCALE)

small rate of change of the length necessitated some means of magnifying it. A lead wire 240 cms. in length and 1 mm.

in diameter only stretches one millimetre in the first 15 min. under the action of a 700 grams weight. The use of an optical lever was resorted to, and by having the telescope

Fig. 4.

two metres from it, it was possible to observe changes of length amounting to one-thousandth of a centimetre. It was therefore possible to keep the wire at a fixed length

very accurately by the continual removal of weight.   It was also necessary to have some means of removing weight without disturbing the mirror of the optical lever.   The removal of weights by hand was found to make the image of the scale quite unreadable.   To avoid this a tin pot containing water was used as the weight and the latter removed as required by means of a syphon.   This worked very well, as there was no visible disturbance of the image when it was used.

In order to eliminate temperature variations the optical lever was itself supported by similar lead wires hanging from the same beam.   The legs of the lever were removed and an axle put through its centre, and it was suspended as shown in figs. 3 and 4 (pp. 542, 543).   This arrangement also had the effect of eliminating any yielding there may have been in the beam from which the wires hung.   The weight of the optical lever was only 60 grams, and this was supported by two wires, so that even supposing the rate of stretching was proportional to the weight to such a low limit, it would in any case be very small compared with the rate of stretching of one wire under a stress of 700 grams.

Fig. 5.—Decrease of Stress when kept at Constant Length.
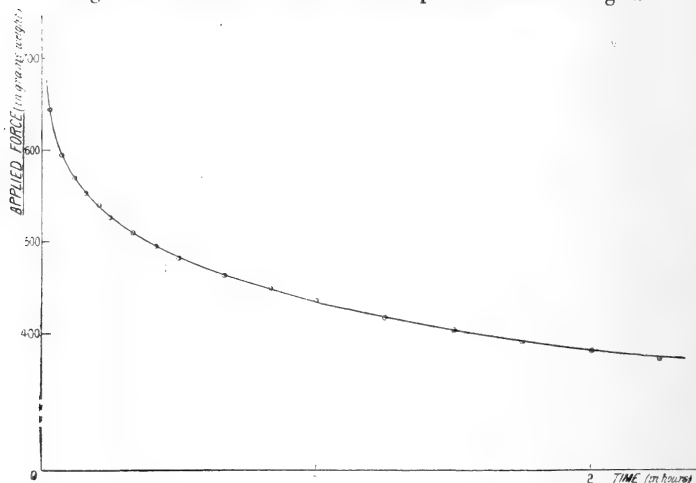
The curve obtained is shown in fig. 5, and here, even more closely than in the first case, an equation of the form

$$W = a + k \log (t + c)$$

fits.   The actual equation is

$$W = 713 - 159 \cdot 1 \log (t + 1),$$

where W is measured in grams weight and t in minutes. The values obtained from this curve are given in the last column.

Time.	t (in minutes).	Weight removed.	W.	Value from Equation.
h m				
1 45·0	0	0	700	713
1 46·8	1·8	65·3	644·7	642
1 49·3	4·3	105·6	594·4	598
1 52·1	7·1	130·0	570·0	567
1 54·7	9·7	148·0	552·0	549
1 57·5	12·5	161·7	538·3	533
2 0·0	15·0	173·8	526·2	522
2 5·0	20·0	190·9	509·1	503
2 10·0	25·0	205·6	494·4	488
2 15·0	30·0	217·3	482·7	476
2 25·0	40·0	236·8	463·2	457
2 35·0	50·0	252·0	448·0	441
2 45·0	60·0	265·0	435·0	429
3 0·0	75·0	285·0	415·0	414
3 15·0	90·0	298·6	401·4	401
3 30·0	105·0	312·0	388·0	391
3 45·0	120·0	320·8	379·2	382
4 0·0	135·0	329·8	371·2	374

Here the variation from the equation nowhere amounts to 2 per cent. Owing to reasons given before the first reading is again faulty, and with this exception the variation is little more than 1 per cent. In this case the curve is exponential to an axis $t = -1$ and cuts the axis $t = 0$ in the point $W = 713$. If the curves had these equations then the couple and the stress would vanish after some days, since both the equations represent curves cutting the t axis at a finite distance.

This experiment was afterwards extended to see what relation existed between the initial weight a and the constant k in the equation
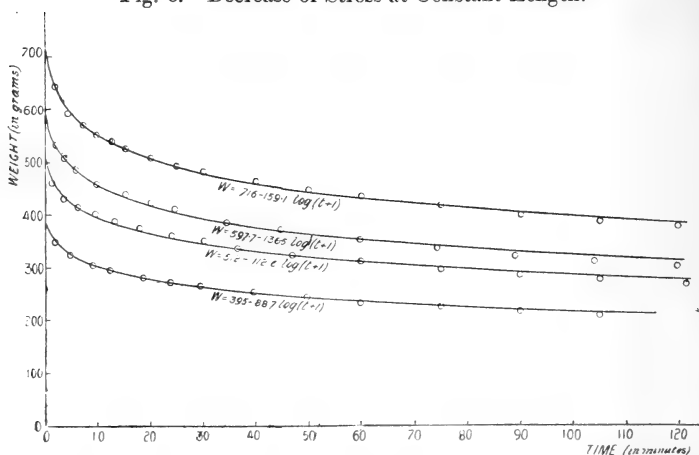
$$W = a - k \log (t + 1),$$

or in the derived equation

$$\frac{dW}{dt} = -\frac{k}{t + 1}.$$

Initial weights of 600, 500, and 400 grams were used, and in each case an equation of the above form approximately fits the experimental curve. In fig. 6 the curves are those plotted from the equations.

Fig. 6.—Decrease of Stress at Constant Length.

If in each case we divide a by k we get the following result :—

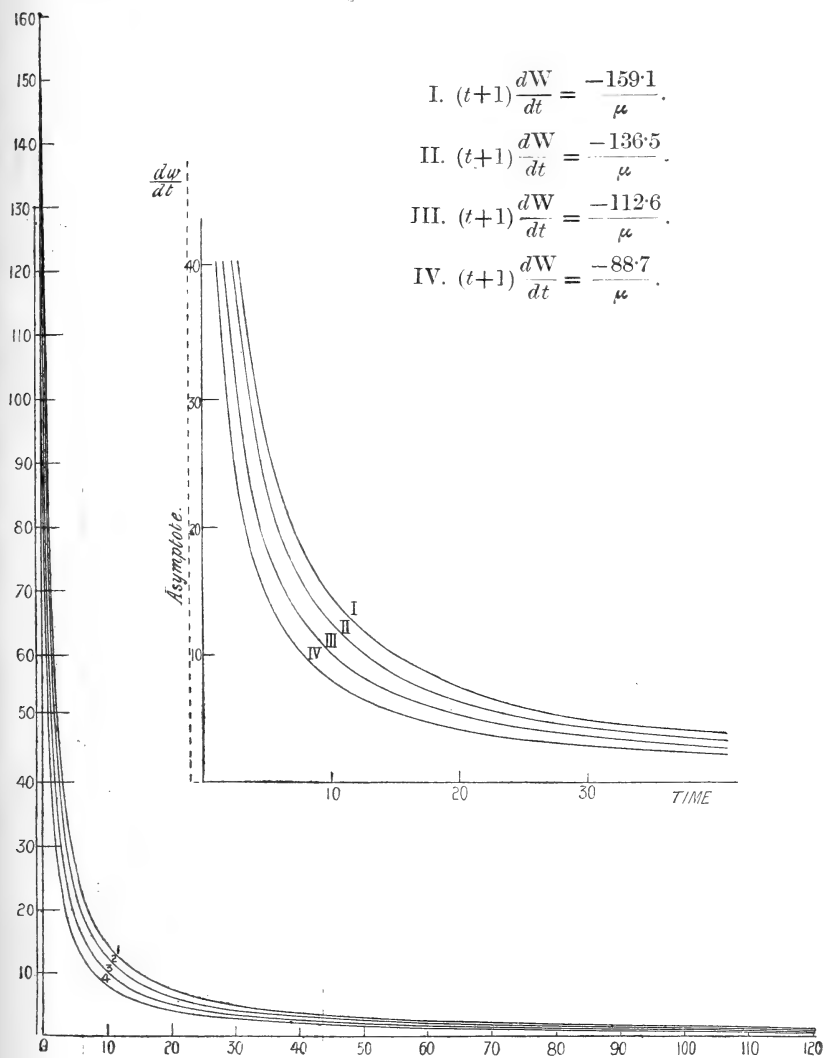| Number.	a.	k.	$\dfrac{a}{k}$.
1	716	−159·1	−4·49
2	597·7	−136·5	−4·38
3	512	−112·6	−4·55
4	395	− 88·7	−4·46

It will be seen that throughout this range the rate of decrease of weight is nearly proportional to the initial weight, at equal times from the beginning of the experiment. Fig. 7 represents $\dfrac{d\mathrm{W}}{dt}$ plotted against time. The curves are rectangular hyperbolas with $\dfrac{d\mathrm{W}}{dt} = 0$, and $t = -1$ as asymptotes. The ordinates at any value of t are in the proportion 7 : 6 : 5 : 4 approximately.

3. *Rate of Stretching under Constant Stress, and Rate of Recovery on Release.*

The apparatus used was the same as in Experiment 2, so that the temperature, and yielding of support, effects were eliminated. Three determinations were made, the stretching weight being 700 grams applied for 1, 3, and 21 hours respectively in the three cases. The curves (I.–III.) obtained are given in Plate XIV.

Fig. 7.

$$\text{I. } (t+1)\frac{dW}{dt} = \frac{-159 \cdot 1}{\mu}.$$

$$\text{II. } (t+1)\frac{dW}{dt} = \frac{-136 \cdot 5}{\mu}.$$

$$\text{III. } (t+1)\frac{dW}{dt} = \frac{-112 \cdot 6}{\mu}.$$

$$\text{IV. } (t+1)\frac{dW}{dt} = \frac{-88 \cdot 7}{\mu}.$$

2 P 2

Results.

I. During Extension.   After Removing Load.

Time.	t (in minutes).	Reading on Scale.	Time.	t (in minutes).	Reading on Scale.
h m			h m		
2 21	0	2·70	3 22	61	2·30
2 22	1	2·98	3 23	62	2·09
2 23	2	3·12	3 24	63	2·01
2 24	3	3·21	3 25	64	1·94
2 25	4	3·29	3 26	65	1·90
2 26	5	3·37	3 28	67	1·83
2 27	6	3·43	3 30	69	1·78
2 28	7	3·48	3 35	74	1·70
2 29	8	3·53	3 40	79	1·64
2 30	9	3·57	3 50	89	1·56
2 31	10	3·61	4 00	99	1·50
2 33	12	3·69	4 20	119	1·43
2 35	14	3·78			
2 37	16	3·85			
2 39	18	3·93			
2 41	20	3·99			
2 46	25	4·14			
2 51	30	4·27			
3 1	40	4·52			
3 13	52	4·80			
3 21	60	4·97			

II.   Stretching.   Recovering.

h m	h m		h m	h m	
11 10	0 0	2·74	2 10	3 0	3·92
11 11	0 1	2·99	2 11	3 1	3·72
11 12	0 2	3·12	2 12	3 2	3·62
11 14	0 4	3·29	2 14	3 4	3·50
11 16	0 6	3·41	2 16	3 6	3·43
11 18	0 8	3·51	2 19	3 9	3·35
11 20	0 10	3·60	2 22	3 12	3·30
11 25	0 15	3·78	2 28	3 18	3·21
11 30	0 20	3·93	2 34	3 24	3·14
11 35	0 25	4·07	2 46	3 36	3·05
11 40	0 30	4·19	3 00	3 50	2·99
11 46	0 36	4·31			
11 52	0 42	4·44			
11 58	0 48	4·56			
12 4	0 54	4·68			
12 10	1 0	4·80			
12 22	1 12	5·00			
12 34	1 24	5·20			
12 46	1 36	5·39			
1 10	2 0	5·75			
1 34	2 24	6·08			
1 50	2 40	6·29			
2 10	3 0	6·56			

III. Stretching.   Recovering.

Time.	t.	Reading.	Time.	t.	Reading.
h m	h m		h m	h m	
3 25	0 0	2·90	12 25	21 0	16·80
3 26	0 1	3·22	12 26	21 1	16·57
3 27	0 2	3·38	12 27	21 2	16·48
3 29	0 4	3·57	12 28	21 3	16·41
3 31	0 6	3·69	12 31	21 6	16·29
3 33	0 8	3·81	12 34	21 9	16·20
3 35	0 10	3·91	12 37	21 12	16·14
3 37	0 12	4·01	12 43	21 18	16·05
3 43	0 18	4·17	12 49	21 24	15·98
3 49	0 24	4·33	1 1	21 36	15·87
3 55	0 30	4·52	1 25	22 0	15·74
Next day			1 46	22 21	15·67
9 36	18 11	17·56	2 37	23 12	15·50
12 12	20 47	19·36			
12 25	21 0	19·50			

In all three cases the wires were stretched from zero on the scale, and the variation in immediate extension (2·70, 2·74, 2·90) may probably be accounted for by the wires being in slightly different states of extension at the beginning of the determinations.

It will be noticed from the curves:—

(1) That the immediate or primary recovery is in each case practically equal to the immediate or primary stretch. (I. 2·70, 2·67 ; II. 2·74, 2·64; III. 2·90, 2·70.)

(2) That after the first hour the curve of extension with time is practically a straight line, and points to a purely viscous flow.

(3) That after release the curves in all three cases are of the same shape, and practically coincide if superposed.

It therefore appears that either all the energy is stored within the first hour, or that after that time the rate of loss is equal to the rate of gain of energy. The immediate or primary stretch is apparently purely elastic, since the wire, if stretched and released, practically returns to its original length. This elastic property is retained throughout, since the primary recovery is constant and independent of the time. Then follows what appears to be a stretch partly elastic and partly viscous, and after a certain time the stretch is purely a viscous flow, indicated by the curve being a straight line. The gradual or secondary recovery which takes place on removal of the load is apparently in correspondence with that part of the gradual stretch which is non-viscous—the secondary strain.

Rate of Disappearance of Primary Recovery under Constant Strain.

Fig. 8 shows the primary recovery at different times, the wire in the intervals being maintained at constant length.

Fig. 8.—Constant Length.
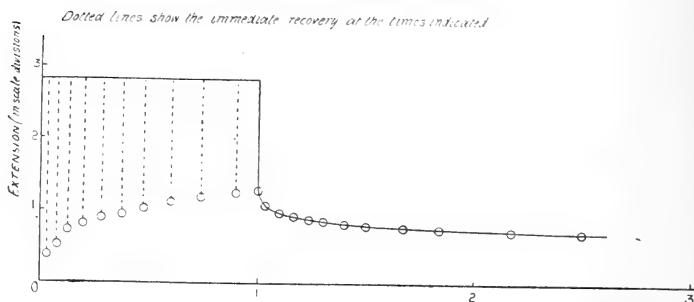
No great accuracy could be expected since the loading and unloading could not be performed very quickly, for a reading had to be taken in the meantime.   It will be seen, however, that the curve of recoveries is of the same type as the decrease of load curve, indicating that the behaviour of the wire follows a modified Hooke's Law, *i. e.* the primary recovery at any time is proportional to the weight then on. The second part of the curve shows the rate of recovery on the final removal of the whole load.

Arrangement for automatically keeping an Overstressed Wire at Constant Length.

In order to discover whether it is necessary to remove the whole load from a wire kept at constant length within a finite time (which would be the case if the empirical equation given before were true), and at the same time to remove the weight more accurately than could be done by hand, a method was devised for automatically performing the experiment.   The essence of the method is that contact between the wire under test and a plate kept in a constant position (except when necessary to compensate for temperature effect) electrically opens a valve which removes a small portion of the load ; this removal causes the wire to break contact by receding a very small distance.   The load on the wire gradually stretches it again until contact is once more made, and this process then repeats itself.

Fig. 9 is a diagram of the apparatus. As before, water is used as the load, but in this case it is necessary that the whole weight on the wire should ultimately be removed.

Fig. 9.
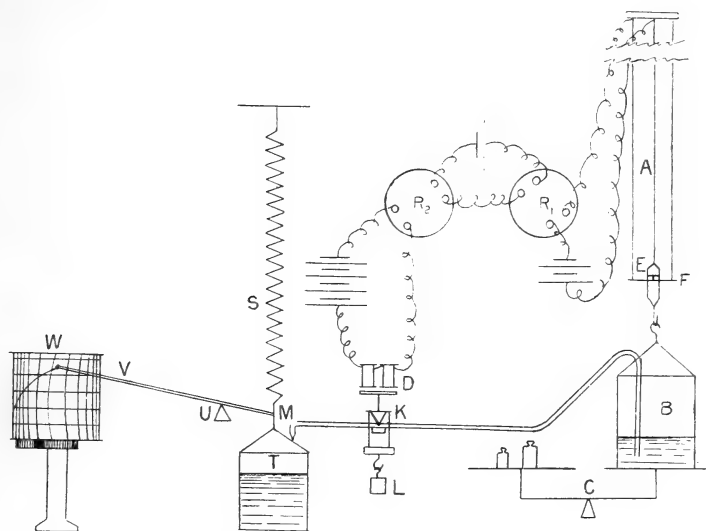
The vessel B which is suspended from A (the wire under test) is therefore placed on one pan of the balance C, and weights rather more than the weight of the vessel are placed in the other pan. The pull on the wire A at any time is the difference between the weight of B and the water it contains and the weights on the other pan of the balance. The whole of the weight on A can therefore be removed if necessary by means of the syphon.

F is a brass plate (shown on a larger scale in fig. 10), suspended at its corners *b* by three lead wires. Its weight is 10 grams, so that the tension in each wire is 3·3 grams. The piece P is not fixed, but can be unscrewed at *c c* leaving the hole Q open. E, a piece of brass soldered to the end of the wire under test, passes through the hole Q, and then P is placed through O underneath the point *a*. The point is of platinum, and P is a flat piece of brass covered with platinum foil. These two form the contacts, and it will be seen that the point of contact is on the axis of A (fig. 9).

As it requires 0·2 ampere to work the electric valve DKL, it is necessary to introduce relays R_1 and R_2 to reduce the current between E and F to ·002 ampere, so that making and

breaking the contact may occur for very small changes of length. The temperature effect was eliminated by the use of the plate suspended by lead wires, as in the previous experiments.

Fig. 10.
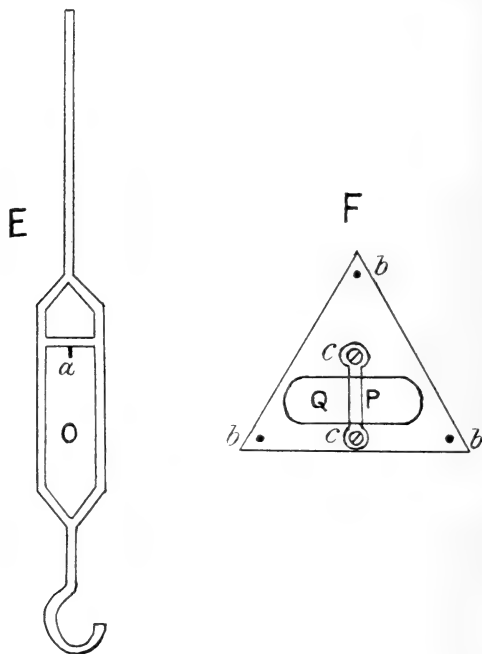
Now let E and F be not in contact, the pull on the wire stretches A until they come into contact. As soon as this is the case, the first relay R_1 is brought into action, and this in turn causes relay R_2 to act. A current flows in the electro-magnet D, and the knife-edge K is raised one or two milli-metres. Now K has been pressing on a piece of rubber tubing and preventing water from syphoning from B. When it is raised, water runs off from B, and E rises, breaking contact. Immediately the weight L pulls K down into its former position, and water ceases to flow. The intermittent action of the valve K causes E to be always just on the point of contact with F. In other words, the wire A except for temperature effects remains the same length throughout.

To record the way in which the water comes off, it is run into a second vessel T, which is suspended from the spiral spring S. A lever is fixed to the spring at M and turns over

a fulcrum U. At the other end of the lever is a pen V, which draws the curve on the drum W which revolves once in 24 hours. The length WU is about three times UM.

On the surface of the water both in B and T, a layer of oil is placed to prevent evaporation.

The curves drawn by the pen were found to be smooth during the first 24 hours or so, but after that, when the pull on the wire became very small, the test of accuracy was too severe and the curve is not to be depended upon further than to give the character of the phenomenon. Curves VIII. and IX. (Pl. XV.) are traced from the actual drawings of the pen. In the first case the original weight was 500 grams, and in the second 250 grams. The first experiment proceeded five days, when all but about 18 grams had been removed and more was still being removed. In the case of the initial weight of 250 grams, after ten days all but 5 grams had been removed and the process had stopped. It is possible therefore that the whole is not removed within a finite time, and the equation before given fails when the time is greater than a few hours. One would expect the curve to be asymptotic to the final weight removed. A curve of the type

$$W = a \log (pt + 1),$$

where W is the weight removed, fits VIII. very well during the first few hours, but there is an increasing departure from it as the time increases. The same applies to curve IX. But if the equation be slightly altered to the form

$$W = a \log \left\{ \frac{pt + 1}{qt + 1} \right\},$$

where q is so small that it has little effect during the first few hours but increasing effect with time, then it fits through the whole range and satisfies the end condition, namely, that at an infinite time the slope shall be zero and the value of W somewhere near 500 grams.

The equation

$$W = 205 \log \left\{ \frac{6 \cdot 5t + 1}{0 \cdot 0237t + 1} \right\}$$

fits curve VIII. reasonably well.

When $t = 0$, $W = 0$.

When $t = \infty$, $W = 205 \log \left\{ \frac{6 \cdot 5}{0 \cdot 0237} \right\}$
$$= 500.$$

W.

Time.	Equation.	Experimental.
0	0	0
1	177	176
2	251	231
3	263	263
4	285	287
6	316	318
8	338	341
10	355	358
12	367	370
14	376	382
16	385	390
20	399	404
24	410	414
∞	500	

Curve IX. $W = 140 \log \left\{ \dfrac{1 \cdot 5t + 1}{0 \cdot 0245t + 1} \right\}.$

0	0	0
1	54	53
2	81	80
3	99	97
4	112	112
5	123	122
6	132	133
7	139	139
8	144	146
9	151	150
10	155	155
12	163	164
14	170	173
16	175	180
18	180	185
20	185	190
∞	250	

The time of stretching was not the same in the two cases; in the first about 45 minutes and in the second 135, so that no definite correspondence between the constants can be expected. In the first experiments (those done by hand) the removal of the load was proceeded with immediately the load had been placed on the wire. In the later experiments the object was to allow the wire to reach the state of steady flow before removing any weight. The wire used in the second series of experiments was of rather less diameter than that used in the first.

No doubt with more care in the determination of the constants a, p and q, a closer fitting equation could be obtained. There seems, however, to be no convenient method of determining them except by trial and error.

The type of law which fits in with our experiments is surprisingly at variance with what one would from analogy expect, and indeed with what has actually been deduced from theoretical grounds. Professor Natanson [*], who has thrown so much light on the question of overstrain, arrives at the expression $\Pi = \Pi_0 \epsilon^{\frac{-t}{T}}$ for the stress required to preserve constant strain, where Π_0 is the initial pressure and T, following Maxwell, is the time of relaxation [†].

Our experiments with lead on the contrary, as we have seen, agree so long as the time is small with an expression $\epsilon^{\frac{w}{a}} = t + c$, or to remove that restriction $\epsilon^{\frac{w}{a}} = \dfrac{pt + 1}{qt + 1}$.

All the more obvious considerations which suggested themselves have been found to lead to an expression of the wrong type. For instance, one may picture the material as being composed partly of viscous and partly of elastic components interdependent on each other. These may be arranged in a variety of ways, which all ultimately lead, however, to the wrong form of expression. A model was actually constructed on such lines before the true law was appreciated. Two thin rubber bands hanging from above helped to support a crossbar to which a weight could be attached. Between these in the centre, fixed upright on the crossbar, was a long cylinder filled with a thick oil. A piston hung from above by a strong rubber band worked in this cylinder [‡].

The action on attaching a weight below is to immediately pull out the thick rubber band which carries the piston an amount corresponding to the weight put on, the piston for instantaneous movements being fixed in the oil, and the thin bands hardly affording any help at this stage. As the piston slowly draws out, more and more weight is thrown on the thin bands, which accordingly continue to stretch. Ultimately they carry the whole weight.

[*] *Bulletin de l'Académie des Sciences de Cracow,* Oct. 1902, p. 512.

[†] Experiments made with pitch show that the above form for the law is inadmissible for this substance also. Phil. Mag. vol. vii. p. 347, 1904.

[‡] The weight of the cylinder and oil was eliminated by means of balance weights acting by cords over pulleys.

These conditions give for the rate of elongation $P\dfrac{dx}{dt} = qx - W$, where $P = \mu q\left(\dfrac{1}{p} + \dfrac{1}{q}\right)$, μ being a viscous coefficient, p and q elastic coefficients, one for the thick and one for the thin band. We thus get for the elongation at any moment an expression $x = a + b\epsilon^{-\frac{t}{A}}$.

This model then gives us an immediate or primary strain followed by a slow movement in time—the secondary strain. To represent the final and uniform viscous flow, in order to make the model more complete, one could add say a piston, working in a cylinder of oil attached below, the weight itself being hung from beneath the cylinder.

By supposing the bands to be able to support compression as well as tension, such a model can give, except in the one particular, a complete representation not only for the elongation curve under constant stress, but the curve of change of stress with time under constant strain. The one particular, as we have seen, is that the curve is an exponential of the wrong type.

From these considerations we are forced to conclude that the explanation of the phenomenon must be sought for on altogether different lines from those based on a mixed viscous and elastic action in the way commonly attempted.

LIII. *On the Conductivity of Gases contained in Small Vessels.*
By George Jaffé, *Trinity College, Cambridge* [*].

IT was shown by C. T. R. Wilson [†] that the spontaneous ionization of gases is approximately proportional to the pressure, except at high pressures. At the same time the relative ionization of different gases is nearly proportional to the density of the gas, except in the case of hydrogen.

As it seemed of interest to try whether this proportionality still holds for a gas of very high specific gravity and complicated structure, like nickel-carbonyl, I undertook, at the suggestion of Prof. J. J. Thomson, to measure the spontaneous ionization of this gas. It was also to be hoped that the behaviour of the leak through $Ni(CO)_4$ at different pressures in vessels of various sizes would throw some light on the part played by the walls of the vessel in ionising the gas.

The apparatus used was a gold-leaf electroscope, and differed only in minor details from that used and described

by C. T. R. Wilson[*]. Most of the experiments were executed in glass bulbs of various sizes, thinly silvered internally, some, however, in brass vessels of cylindrical shape. A quartz rod, attached by sealing-wax, was substituted for the sulphur bead used by Wilson on account of the action of nickel-carbonyl on sulphur, and the supporting rod was kept at a high potential by a set of very constant storage-cells.

An initial potential of 80 volts was used in most experiments, and the leakage observed over a period varying from about 2 to 48 hours. The eyepiece micrometer-scales were calibrated by means of Clark cells; the value of one division varied in the different electroscopes between 0·16 and 0·4 of a volt; the capacities were compared with that of a sphere of known radius[†].

The insulation was tested from time to time, and found to be so satisfactory that the leak varied by less than 0·2 of a division per hour, whether the supporting rod was to earth or to 80 volts; at low pressures, however, where the leak is **very** small, an equal number of observations was always taken either way, to eliminate any error due to want of insulation.

I. *Experiments with Air at Atmospheric Pressure.*

As the experiments were always extended over so long a range of time that the error of the reading only amounted to a few per cent., and as the sensitiveness of the scale and the capacity of the leaking system were practically constant, individual readings with the same electroscope ought to have agreed to about 5 to 10 per cent., supposing the quantity under consideration to be a constant. But it was soon found out that the accuracy attainable was far less, and that different readings sometimes disagreed to 30 per cent. and even more. It was therefore concluded that the spontaneous ionization undergoes variations, and a great number of experiments were undertaken in order to find out the cause of this variability. It was thought that light, temperature, moisture, and weather-conditions might account for the observed changes; a careful examination, however, showed this not to be the case. The experiments leading to this conclusion are therefore not recorded in detail; but to show the order of magnitude of the variations involved, the maximum, minimum, and mean values of observations taken with five different vessels during several months are given in Table I.

[*] *Ibid.*
[†] *Cf.* C. T. R. Wilson, *loc. cit.*

TABLE I.

No. of experiment.	Shape of vessel.	Material of walls.	Volume in c.cm.	In the day.				In the night.				Mean $\frac{M_I+M_{II}}{2}$	Greatest deviation from the mean in per cent.
				Number of obs.	Maximum.	Minimum.	Mean M_I.	Number of obs.	Maximum.	Minimum.	Mean M_{II}.		
I. a ..	Bulb I.	Glass silvered	97·3	8	148	108	119	4	130	105	117	118	25
I. b ..	"	"	"	41	53	41	46	34	54	45	50	48	16
I. c ..	"	"	"	18	55	44	51	14	53	44	48	49	15
II. ..	Bulb II.	"	59·3	29	68	45	58	27	63	52	55	57	27
III...	Bulb III.	"	55·7	8	53	38	48	9	59	41	49	49	28
IV...	Cylinder	Brass	180	20	11·3	9·1	9·7	16	12·4	9·1	9·8	9·8	27
V. ..	"	"	100	56	4·8	2·7	3·4	47	4·3	2·6	3·5	3·5	37

Every experiment mentioned in this table gives the average leak for 6 to 16 hours. The ionization during the day was by no means constant, and the readings in single hours disagreed often even by more than the above averages ; but as an observation over a short range of time is subject to a much greater error, only averages are included in the table.

The numbers in the last two rows of Table I. are given in different units (scale-divisions per hour) for each of the two cylinders, and cannot therefore be compared. All the other numbers, however, represent the number of ions of either sign set free per second in each c.cm., calculated from the experimental data by taking 3.1×10^{-10} E.U. as the charge on one ion.

C. T. R. Wilson's determination of the same quantity is 39 ions per c.cm. per second*. The value obtained in the first series (I. *a*) is much higher ; the bulb was therefore resilvered, and the number of ions thereby reduced to the smaller value given under I. *b* and I. *c*. The first coating may have contained some radioactive impurity ; the fact, however, affords another instance for the observation of Strutt †, that different samples of the same metal can give very different values to the spontaneous leak.

The smaller values I. *b* and I. *c*, as well as those obtained for bulbs II. and III. are still appreciably bigger than 39 ; this may, however, be due to the fact that C. T. R. Wilson observed the leak in a vessel of 163 c.cm. volume. If the ionization is entirely or partly due to the influence of the walls, a bigger production of ions per c.cm. is to be expected in smaller vessels.

It will further be seen from Table I. that the values for the ionization in the day and in the night show the same variability, and that the means for either are closer to each other than one would expect, showing that neither small changes of temperature nor light affect the leak. In the case I. *b* it seems as if the leak in the night was a little too high. But this is certainly no genuine effect, as shown by I. *c*, which gives observations with exactly the same apparatus only in a different room.

This result suggested the idea that different local conditions affect the leak. In order to examine this, two vessels of about equal volume (100 c.cm.) one of silvered glass and one of brass, were at first placed close to each other and,

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* This value has been calculated from C. T. R. Wilson's observations employing the most recent determination of the charge *e* of an ion by H. A. Wilson (Phil. Mag. [6] v. p. 429, 1903) ; C. T. R. Wilson takes $e = 6.5 \cdot 10^{-10}$ E.U., and therefore gets 19 ions per sec. and c.cm.

† 'Nature,' Feb. 19, 1903.

then in different parts of the building. In the latter case
the variations of the natural leak showed no agreement,
except in a few instances of great changes; but when the
two electroscopes were kept close together there was an
exact parallelism between them. The changes occurring
were not proportional, and as long as they were within the
errors of experiment (less than 10 per cent.) they did not
even always show the same direction; but whenever any
larger variations took place, a close agreement in the be-
haviour of the two vessels could be observed. These
experiments were carried on for about two months; the
analogy was not always equally good, but on the whole
unmistakable. Table II. contains the results for ten days of
observation when the resemblance was particularly good.
The amount of ionization is given in arbitrary units, but
chosen in such a way that the averages for the two cases
coincide. The numbers for the leak in the day are means of
8 hours, those for the leak in the night means of 16 hours.
It will be noticed that for this particular period the leak at
night is always bigger than in the daytime, except on
March 12th: but it has already been mentioned that this is
not always the case, and seems due to special local conditions.

TABLE II.

Date.	Leak in the silvered glass vessel.		Leak in the brass vessel.	
	In the day.	In the night.	In the day.	In the night.
March 10	...	6·7	...	7·0
,, 11	5·9	6·7	5·2	6·2
,, 12	7·0	...	7·9	
,, 14	6·0	6·8	5·8	6·6
,, 15	6·0	6·4	5·8	6·4
,, 16	6·0	6·9	5·2	6·8
,, 17	6·0	6·8	5·6	6·5
,, 18	5·7	6·6	5·4	6·5
,, 19	5·6	...	5·0	
,, 21	6·3	7·1	6·2	7·3
,, 22	6·2	...	6·2	

The parallelism between the two vessels is not limited to
the average values for day and night, but was also observed
for the variations occurring during the day, though to a less
extent, as might be expected from the less accuracy of read-
ings over a short range of time. There was also a distinct
indication of a slight periodical diurnal variation with the
minimum between noon and 2 P.M., but this could not always

be observed, as bigger irregular variations were superposed and obscured the periodicity. In Table III. only one typical example is quoted, where obviously some disturbance took place. Such disturbances, however, and the whole resemblance in the behaviour of two vessels close to each other seem to be due to some local uncontrolled circumstances, as the agreement disappears when the electroscopes are far apart.

TABLE III.

Range of time over which the average is taken.	Leak in the silvered glass vessel.	Leak in the brass vessel.
Jan. 21, 6 P.M.—Jan. 22, 9 A.M. ...	7·6	7·6
Jan. 22. 10 A.M.— ,, 1 P.M. ...	7·3	6·6
Jan. 22, 1 P.M.— ,, 3 P.M. ...	8·4	7·6
Jan. 22, 3 P.M.— ,, 5 P.M. ...	6·4	6·3
Jan. 22, 6 P.M.—Jan. 23, 9 A.M. ...	8·0	8·3

As the electroscope-method does not permit of measurements of the amount of ionization at a certain moment, but only of the average over a considerable time, it seemed advisable to carry out some experiments by the electrometer-method, to see whether it would be possible to detect well marked changes also in this case. The spontaneous leak was therefore measured in a brass vessel of about 3 litres, and in a tin vessel of about 10 litres capacity, during several months, by aid of a sensitive Dolezalek electrometer.

The values thus observed were found to be much more constant than those mentioned above; but there were certainly variations which exceeded the limit of error and could not be otherwise explained than by changes of the spontaneous ionization itself. The greatest differences between two sets of readings were 16 per cent. in the case of brass, and 15 per cent. in the case of tin; the greatest deviation from the mean of all observations did not exceed 10 per cent. in either case.

The number of ions produced in the c.cm. per second varied in the brass vessel between 29 and 35, and in the tin vessel between 42 and 50. These absolute values are not very accurate, as the capacities were not very carefully determined and are uncertain to at least 10 per cent.; but the relative measurements in the same vessel are certainly exact to 5–7 per cent.

It followed from these experiments, that the variations in

big vessels of several litres capacity are far less pronounced than those in small vessels of about 100 c.cm. capacity. Some experiments carried out by the electroscope-method with a vessel of about 10 litres volume confirmed this fact, which seems to indicate that the cause for these variations depends on the surface and not the volume of the vessel.

All experiments mentioned so far were made with air which had been for a considerable time within the vessel where its conductivity was to be measured. The question whether the leak alters when the air is replaced by fresh air has not been dealt with. Now it is known by the original measurements of Geitel * that the leak during the first four days after filling the vessel with fresh air increased to about five times its initial value. C. T. R. Wilson does not mention the same fact, and as Geitel used a very much bigger volume (13,000 to 163 c.cm.) it seemed likely that it is confined to large vessels. This is, however, not the case ; an increase of the initial conductivity can also be observed in the case of small vessels of silvered glass, though it is far less pronounced than in Geitel's experiments. Indeed, the change is very slow, and was therefore not noticed until it was found that the leak in a vessel is always too big, after it has been left for some time to itself. This point was further examined, and it was found that the leak after refilling the vessel increased in some cases by 25–35 per cent. in 5 to 7 days and then remained constant, or at least oscillated round a constant mean.

The behaviour was, however, by no means consistent in all cases ; sometimes the increase went on for a far longer period and sometimes the total increase did not exceed 20 per cent. Table IV. contains average leaks for two different bulbs during the first 7 to 9 days after refilling them with fresh air, the units are arbitrary. The table will show the irregular nature of the increase.

TABLE IV.

Day	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	16th
Bulb I. 97 c.c. ...	10·8	11·5	12·4	12·2	...	12·3	12·6			
,,	10·0	10·0	10·4	10·7	...	...	11·6	12·0		
,,	10·0	10·2	10·6	11·0	...	...	...	11·4		
Bulb II. 59 c.c. ...	10·2	10·3	10·3	10·8	10·9	10·8	10·9	11·0	11 1	12·3
,,	10·3	10·1	11·0	12·8	13·1	...	...	13·1	13·3	

* *Physikalische Zeitschrift*, ii. p. 116 (1900).

These results cannot be represented by a smooth curve ; but this could hardly be expected, as the value of the ionization, even after the maximum is reached, varies considerably. The increase during the first days is, however, quite certain, as the average for the first four days is always at least 20 per cent. lower than that for any later four days, and as the leak in a vessel is always reduced, when old air is replaced by fresh air.

This increase of the conductivity might be due to a radio-active emanation which is given off by the walls at a constant rate ; on this supposition the leak at any time t after refilling the vessel would be represented by the formula

$$I_t = I_0 \left(1 - \epsilon^{-\lambda t}\right),$$

where λ is the radio-active constant of the emanation and I_0 the final ionization after a long time has elapsed. It is, however, not possible to compare the above results with the formula, or with the behaviour of any known radio-active substance, on account of the irregularities mentioned.

The electroscope-method is in this case unsatisfactory, since it does not allow the leak during a short period to be measured.

II. *Observations at Different Pressures.*

As the ionization of air at atmospheric pressure is not constant, it could hardly be expected that observations at different pressures would show a better agreement. In fact, although the variations at lower pressures were on the whole smaller than at higher pressures, it was not possible to obtain values which lie accurately on a smooth curve. But these irregularities vanished on taking means of several observations at each pressure. Another way of obtaining consistent results was to observe the leak at atmospheric pressure in a second bulb placed near the one in which the variation with pressure was measured, and to regard only readings during a time in which the leak in the control-vessel did not vary appreciably. Furthermore it was, of course, necessary to use only air which had been sufficiently long in the vessel to have reached the maximum of ionization.

Tables V. and VI. contain the results of observations in two bulbs of 97 and 59 c.cm. capacity respectively. The values for the smaller bulb are averages of several independent observations each, as will be seen from the second column, the numbers for the larger bulb, however, were all obtained with the same sample of air, taking averages over 6 to 48 hours

for each pressure. (The results are represented in figs. 1 and 2, p. 566.)

Table V.

Bulb I.   Volume 97·3 c.cm.; electrostatic capacity 0·65 cm.
1 scale-division = 0·165 volt.

Pressure in mm. of mercury.	Leak in scale-divisions per hour.	$\dfrac{\text{Leak}}{\text{Pressure}}$.	$y=a_1p+b_1$, $a_1=0.0193$, $b_1=1$.
20·5	0·58	0·0283	
42	1·2	0·0286	
79	2·15	0·0272	
142	3·8	0·0268	3·7
241	5·9	0·0245	5·7
395	8·4	0·0212	8·6
538	11·7	0·0217	11·4
638	13·1	0·0205	13·3
770	14·8	0·0193	

Table VI.

Bulb II.   Volume 57·3 c.cm.; electrostatic capacity 1·63 cm.
1 scale-division = 0·2 volt.

Pressure in mm. of mercury.	Number of observations at this pressure.	Greatest deviation from the mean.	Leak in scale-divisions per hour.	$\dfrac{\text{Leak}}{\text{Pressure}}$.	$y=a_2p+b_2$, $a_2=0.0157$, $b_2=1.2$.
33	4	10 p.c.	0·88	0·0268	
74	7	9 ,,	1·71	0·0231	2·16
248	7	12 ,,	5·2	0·0210	5·1
325	3	3 ,,	6·3	0·0195	6·3
439	6	13 ,,	8·1	0·0185	8·1
544	3	7 ,,	9·9	0·0182	9·8
760	14	23 ,,	12·3	0·0162	

Both series show the same features, the leak being not strictly proportional to the pressure. The ratio of leak to pressure decreases with rising pressure, as already observed by C. T. R. Wilson. It will be seen that the values for the leak (y) for pressures from 150 to 600 mm. can be represented fairly accurately by a straight line which cuts the axis of the ordinates a short distance above the origin, and the equation of which is $y=ap+b$.

This fact seems to indicate that the ionization is (at least

partly) due to a radiation from the walls which is not of uniform type. On this supposition the term b would correspond to a very weak radiation, which is perfectly absorbed by as little as 3 cm. of air at $\frac{1}{5}$ of an atmosphere pressure ; the coefficient b corresponds to a more intense radiation of greater penetrative power. This radiation therefore ionises the gas proportionally to the pressure up to a pressure of about 600 mm.

The measurements are perhaps not accurate enough to lay much stress on this conclusion, or on the absolute value of the constant b ; it is, however, quite certain that the leak in this case is not strictly proportional to the pressure, as will be seen from Table VI. In fact, the greatest possible error for the observations at 74 and 33 mm. respectively is less than the deviation in the ratio of leak by pressure.

III. *Observations with Nickel-Carbonyl.*

The same remarks as for air apply to the measurements with $Ni(CO)_4$ at various pressures. It was, however, far easier to get consistent results in this case, as the leak is more than five times bigger than that in air at the same pressures, so that a whole series of observations could be executed in much shorter time, reducing the probability of changes in the ionization.

Several series of observations were performed in both bulbs used for measuring the leak in air at different pressures. As these all agreed satisfactorily, only one series for either vessel will be quoted; these are given in Tables VII. and VIII. and represented in figs. 1 and 2.

<table>
<tr><td colspan="4" align="center">TABLE VII.</td><td colspan="4" align="center">TABLE VIII.</td></tr>
<tr><td colspan="4" align="center">Bulb I.</td><td colspan="4" align="center">Bulb II.</td></tr>
<tr>
<td>Pressure in mm. of merc.</td><td>Leak in scale-divs. per hour.</td><td>$\dfrac{\text{Leak}}{\text{Pressure}}$</td><td>$y=a_3p+b_3$, $a_3=0{\cdot}096$, $b_3=0{\cdot}8$.</td>
<td>Pressure in mm. of merc.</td><td>Leak in scale-divs. per hour.</td><td>$\dfrac{\text{Leak}}{\text{Pressure}}$</td><td>$y=a_4p+b_4$, $a_4=0{\cdot}08$, $b_4=1{\cdot}2$.</td>
</tr>
<tr><td>209</td><td>·14·7</td><td>0·0700</td><td></td><td>200</td><td>13·9</td><td>0·0695</td><td></td></tr>
<tr><td>136</td><td>12·2</td><td>0·0897</td><td></td><td>150</td><td>12·6</td><td>0·0840</td><td></td></tr>
<tr><td>72</td><td>7·8</td><td>0·1083</td><td>7·7</td><td>116</td><td>10·1</td><td>0·0871</td><td>10·4</td></tr>
<tr><td>46</td><td>5·1</td><td>0·1102</td><td>5·2</td><td>74</td><td>7·1</td><td>0·0959</td><td>7·1</td></tr>
<tr><td>34</td><td>4·2</td><td>0·1235</td><td>4·0</td><td>50</td><td>5·4</td><td>0·1080</td><td>5·2</td></tr>
<tr><td>28</td><td>3·4</td><td>0·1214</td><td>3·5</td><td>28</td><td>3·05</td><td>0·1088</td><td></td></tr>
<tr><td>17</td><td>2·1</td><td>0·1235</td><td></td><td></td><td></td><td></td><td></td></tr>
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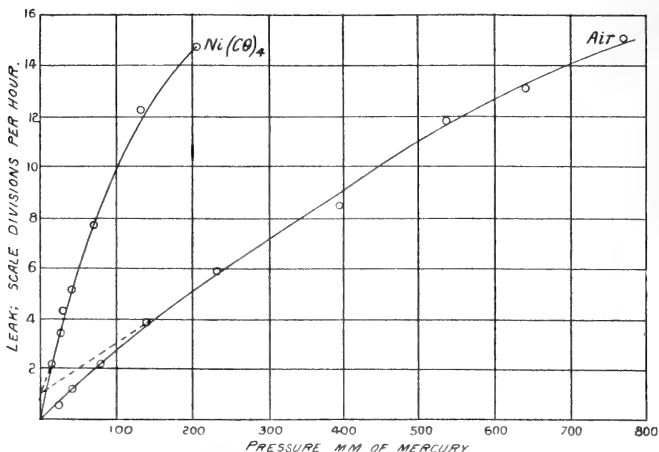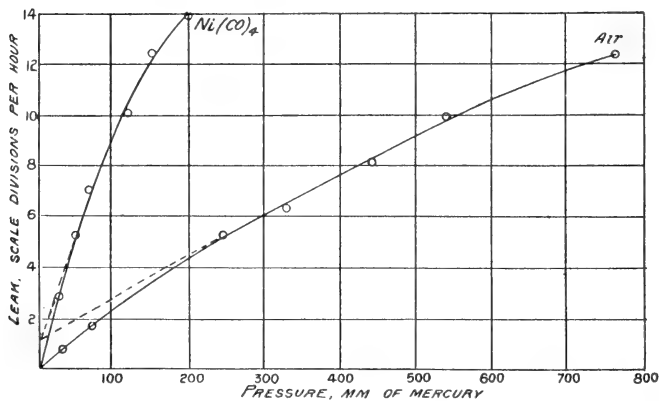
Fig. 1. (Bulb I.)

Fig. 2. (Bulb II.)   5

It will be seen that the curves show the same characteristic properties as in air ; the straight parts, however, extend from about 40 mm. to 120 mm. pressure. The coefficients b are very nearly the same as in air; the constants a, however, are about five times as big as in air $\left(\dfrac{a_3}{a_1} \doteq 5{\cdot}0 ;\ \dfrac{a_4}{a_2} = 5{\cdot}1\right)$. As the density of nickel-carbonyl is 5·9 times that of air, this fact shows that the more penetrating radiation is absorbed by different gases about proportionally to their density.

In fig. 3 the two curves for air and $Ni(CO)_4$ in bulb II. are plotted in such a way that the abscissæ are proportional to the density instead of the pressure. It will be noticed that the curves very nearly coincide.

The fact that the ionization in $Ni(CO)_4$ is not quite so big as might be expected from its density (5·1 instead of 5·9), may, perhaps, be explained by the circumstance that the $Ni(CO)_4$ was always introduced just before the experiments,

Fig. 3. (Bulb II.)
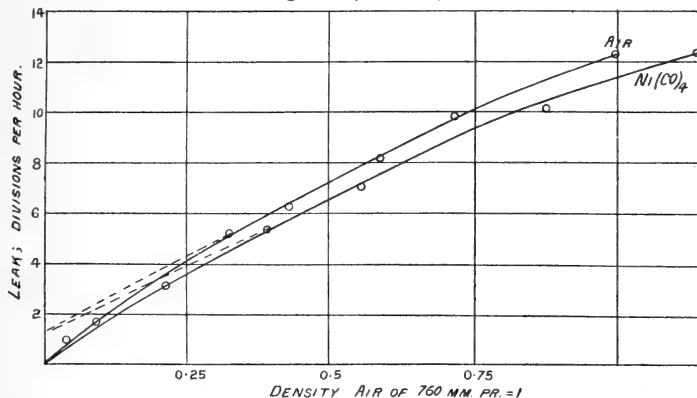
while the air had always been in the vessel for a considerable time. If values for freshly-introduced air are taken, the curve for air sinks even below that for $Ni(CO)_4$. It was not possible to test whether the leak in the latter showed the same increase with time as the leak in air, as $Ni(CO)_4$ could not be kept for a long time in the vessel without decomposing.

Conclusions.

1. The spontaneous ionization undergoes changes due to unexplained causes which, however, depend on local conditions.

2. The variations in small vessels are bigger than those in large vessels and amount sometimes to as much as 40 per cent.

3. The ionization in small vessels of silvered glass is not strictly proportional to the pressure ; but can be represented by a straight line over a considerable range, indicating a complex nature of the radiation from the walls.

4. The ionization in nickel-carbonyl is 5·1 times as great as that in air, and is thus almost proportional to the density.

These experiments were executed in the Cavendish Laboratory. My sincere thanks are due to Professor J. J. Thomson for his kind interest and help. I am also indebted to Dr. Ludwig Mond for supplying me with the nickel-carbonyl.

LIV. *New Standards of Optical Wave-length.*
By Prof. H. Kayser, *Bonn*[*].

THE Section A of the British Association has for so many years promoted the knowledge of spectroscopy, that I think it well to bring before the Section a question which becomes more and more urgent for the progress of spectroscopy in both the theoretical and practical applications, viz., a more accurate system of wave-lengths.

For many years spectroscopists had to be content with the determinations made by Ångström for the visible part of the spectrum, and extended later by Cornu to the ultra-violet part as far as 3000 A. For the wave-lengths to 2200 A, Liveing and Dewar, and Hartley, gave the first measurements. At that time errors between 1 and 5 A were not infrequent, *i. e.* wave-lengths could be determined to only about one one-thousandth of their value.

An immense advance was made by Rowland. He no longer tried to determine absolutely the wave-lengths of a great many lines by the grating, but, applying the much more accurate method of coincidences, found the values of some hundred lines between 2000 and 7600 A, relative to a value of the D-lines taken somewhat at random from the absolute measurements by Bell, Kurlbaum, Müller and Kempf, Pierce. The method of coincidences, as is well known, is founded on the theory of the concave grating, by which at the same point a line of wave-length λ in the first order, of $\frac{1}{2}\lambda$ in the second order, of $\frac{1}{3}\lambda$ in the third, &c., are brought to focus.

Rowland expressed the opinion that none of his standards could deviate by a hundredth of an A from the correct value; and this seemed probable when one considered the first-rate instruments used by Rowland, and the eminent ability of Rowland and his assistant Jewell for such work.

This faith received the first shock when Michelson, by his beautiful interferometer method, determined at Paris, in collaboration with Benoit, the absolute values of three Cd lines. Not only were the absolute values greatly different from Rowland's, but also the relative values showed discrepancies amounting to more than 0·03 A. Rowland's system received a much severer blow when Profs. Fabry and Perot, by another kind of interferometer, measured many iron and solar lines in the

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visible spectrum, and showed that Rowland's system contained values alternately too high and too low. It is now quite certain that Rowland overestimated the correctness of his measurements, that by using his numbers as standards we never can be sure of obtaining relatively exact numbers within 0·01 A. Besides these fluctuating errors pointed out by Perot and Fabry, many of Rowland's lines have individual errors amounting to 1 or 2 hundredths, as any one may see by using different standards for the determination of the same line. Some years ago I tried to give a table of iron standards free from these individual errors, but of course my table as based on Rowland's is subject to nearly the same fluctuations of values.

For the practically more important ultra-violet part of Rowland's system we have no evidence whatever of correctness or incorrectness; but it is highly probable that it will contain errors of the same kind as the visible part.

If one tries to determine the wave-length of a sharp line —and nearly 99 per cent. of all spectrum lines can be obtained sharp—by interpolation between neighbouring standards, he will not find it difficult, by using a good concave grating and a good dividing-engine, to get with a single measurement the value within five thousandths of an A, if he uses the same standards; but by using others he may get differences of one or two hundredths. So it appears that the uncertainty of our wave-length determinations is caused by the insufficiency of Rowland's standards.

To insist upon the importance of obtaining wave-lengths as correct as possible for astronomical purposes, for the identification of elements, for the series relations, &c. seems needless. So the necessity of creating a new system of standards is evident; and it seems time to consider the way by which a better system may be produced and the errors committed by Rowland avoided.

One of Rowland's errors was the use of mixed solar and arc lines which he thought were of identical wave-length. Since that time we know, by the publications of Jewell, Humphreys, and Mohler, that they are different. So this cause of errors could be easily avoided, and it seemed as if by applying the method of coincidences only to arc-lines a correct system could be produced. During the last twelve years I have three times tried to do so, but have always failed,—I did not know why.

After the publication of MM. Perot and Fabry's measurements of iron lines, I formed the plan of founding the method of coincidences on their measurements; it is much easier to

get a system of standards throughout the spectrum by using many fundamental lines than by using a single one.   But as I was not sure of the absolute correctness of Perot and Fabry's numbers, though they seemed very reliable, I felt obliged to convince myself.   This can be done by the method of coincidences, as one line can be got by that method from others.

While engaged in this research, there appeared a paper from Michelson showing that errors in the ruling of gratings are possible, which make the method inapplicable : a line of given wave-length in the first order does not coincide with another line of half that wave-length in the second order. I immediately changed the scope of my investigation, and made some tests to see whether Rowland's gratings have such an error.   I have two of Rowland's largest gratings ruled on his second and third dividing-engines.   The comparison of results obtained with them by the method of coincidences gave differences of more than 3 hundredths A, while repeated determinations with each grating alone agreed to a few thousandths.

It is thus apparent that Rowland's gratings are not suited for the application of this method, and it is probable that the larger part of the errors in Rowland's system are due to this cause.   As gratings are then not to be used for exact absolute determinations, they are useless also for relative measurements, if great accuracy is desired.   At present we have no better engines, though different American physicists are planning new constructions, as Michelson and Wadsworth : time must show if their engines are better.   We are now under the deplorable necessity of disregarding the method of coincidences in the creating of a new system of standards ; the spectra of gratings can be used only for interpolation between standards not too far apart.

It seems to me, then, that the only way open consists in the determination of a great number of arc-lines with some interferometer ; and probably Fabry and Perot's form of that instrument will prove most appropriate.   Its use is not easy, it requires long practice even for the visible part of the spectrum, and for the ultra-violet part the difficulties will be much greater.   Not a single observer, but several, should undertake this task, and when their numbers agree to one or two thousandths A for, say, fifty or a hundred lines throughout the spectrum, then again the grating may be applied for getting by interpolation some hundreds of lines more.   Then it will be possible to get all the spectral lines accurate to some

thousandths **A**, a great advance will have been made, and many questions that are now unapproachable can be answered.

Of course all the measurements of spectra already made—and I have myself worked a good deal in that direction—will be useless as far as wave-lengths are concerned. Prof. Hartmann, of Potsdam, thinks it already now possible to calculate from Perot and Fabry's measurements a table of corrections, by the application of which Rowland's measurements and all the others based on Rowland's standards may be corrected. I think that is impossible. We do not even know if the fluctuations in Rowland's system are truly known, as very few lines have been remeasured by Fabry and Perot ; and they may have fallen on some lines with large individual errors, and the curve of corrections may become quite another one by the use of other lines. But even if that were not the case, who knows from what individual standards every line of a spectrum has been determined ? Hasselberg is the only worker who gives in his tables the standards between which he has interpolated, so that his numbers can be corrected when corrections to Rowland's standards are known. The greater errors of individual lines in Rowland's tables cannot be corrected without remeasuring all the lines. And, further, it is quite impossible to deduce any table of corrections for the ultra-violet part. As the method of coincidences has broken down for gratings which have been tested, we can reach no conclusion as to the behaviour of Rowland's system in this part of the spectrum without new absolute determinations.

So I think that Rowland's system must be entirely abandoned; and a new system must be created. As for all spectroscopic purposes only relatively correct values are needed, the new system might be based on Rowland's false value for the D lines ; then new measurements would coincide within some hundredths A with the old ones. I think it more logical, if an entirely new system is to be created, to base it on Michelson's absolutely exact values of the Cd lines. But this question is of course of minor importance ; of great importance is only, that the new system should be international.

LV. *Notices respecting New Books.*

The Evolution of Earth-Structure, with a Theory of Geomorphic Changes. By T. MELLARD READE. Longmans, Green & Co.

THIS work is a continuation and in part a recapitulation of the author's work on Mountain-building. Secular changes of level of land-surfaces are, according to the author, to be accounted for on the assumption of changes of volume of deep-seated matter:—
" The volumes and the specific gravities of the constituent minerals are subject to increase and decrease consequent upon internal changes of temperature and chemical interactions . . . . . . the *loci* of these opposite actions change with the sequence of the ages " (p. 38). As might be expected by those who have reflected upon this possible explanation, the author fails to give adequate reasons for such recurrent changes of temperature.

The author further dwells on the view, already stated in 'The Origin of Mountain-Ranges,' that changes of temperature in the crust will lead to the ridging up and lateral creep of the surface layers. But the adequacy of probable temperature changes in strata to produce the effects observed in Nature will be questioned by many. Several experiments on various modes of applying compression to plastic materials are described, and the results shown by photographs. Some of the author's papers on cognate subjects are reprinted.   J. J.

Untersuchungen über die Radioaktiven Substanzen. Von Mme. S. CURIE. *Uebersetzt und mit Litteratur-Ergänzungen versehen von* W. KAUFMANN. Mit eingedruckten Abbildungen. Braunschweig: F. Vieweg und Sohn. 1904. Pp. viii + 132.

IN this interesting monograph we have an account, by Madame Curie herself, of the epoch-making researches which have justly earned her a world-wide reputation. The account is rendered all the more fascinating by the simple and unpretentious style in which the writer unfolds the story of her classical researches. The work is divided into five chapters. After a brief general introduction the authoress deals in Chapter I. with the radio-activity of uranium and thorium and the radio-active minerals. Chapter II. contains an account of the discovery of the new radio-active elements— polonium, radium, and actinium. Chapter III. deals with the nature of the radiations emitted by the new radio-active substances, and Chapter IV. with induced radio-activity. The very brief concluding Chapter V. is devoted to a consideration of the nature and cause of radio-activity.

To the references contained in the body of the work, the translator has added a most useful bibliographical appendix, bringing the references down to October 1903.
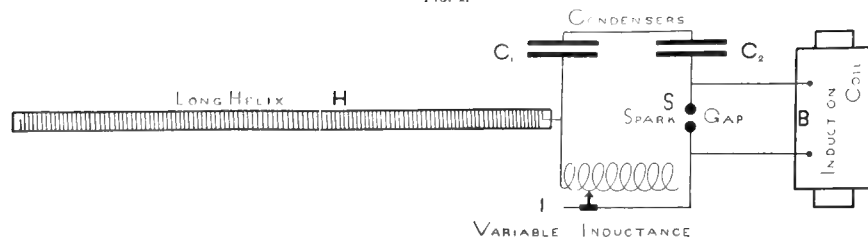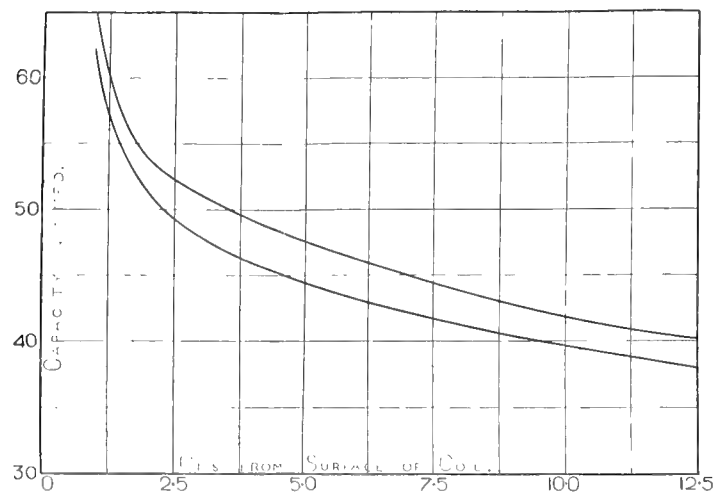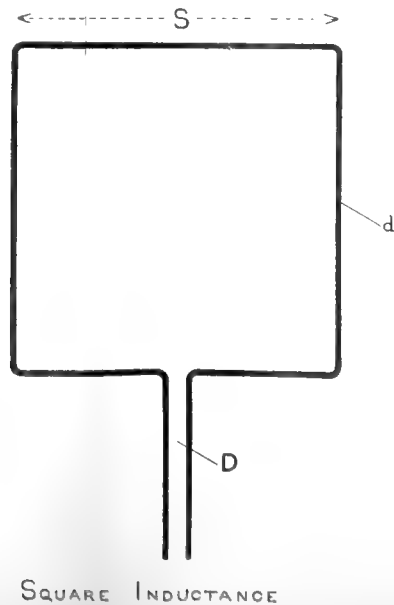
A

FIG. 1.

CONDENSERS

C_1 C_2

LONG HELIX H

S
SPARK GAP

B
INDUCTION COIL

VARIABLE INDUCTANCE

FIG. 2.

FIG. 3.

O ------- 200 ------- E
FUNDAMENTAL

O ----60---- N_1 ------140------ E
1ST HARMONIC

O --34-- N_1 ---80--- N_2 ----86---- E
2ND HARMONIC

O -23- N_1 --57-- N_2 --58-- N_3 --62-- E
3RD HARMONIC

O -18- N_1 -44- N_2 -44- N_3 -46- N_4 -48- E
4TH HARMONIC

O -15- N_1 -36- N_2 -36- N_3 -37- N_4 -37- N_5 -39- E
5TH HARMONIC

LENGTHS IN CMS

FIG. 4.

S

d

D

SQUARE INDUCTANCE

FIG. 5.

V

S_L A_N N A_N P_1 P_2 A

H H

Telegraph Transmitter.

C

T_2 T_1 S

To the Induction Coil.

E E E

Arrangement of the Kummeter for measuring the wave-length of a wireless telegraph transmitter.

D WIRE.

EXTENSION AND RECOVERY OF LEAD WIRE.

LOAD 700 GRAMS.
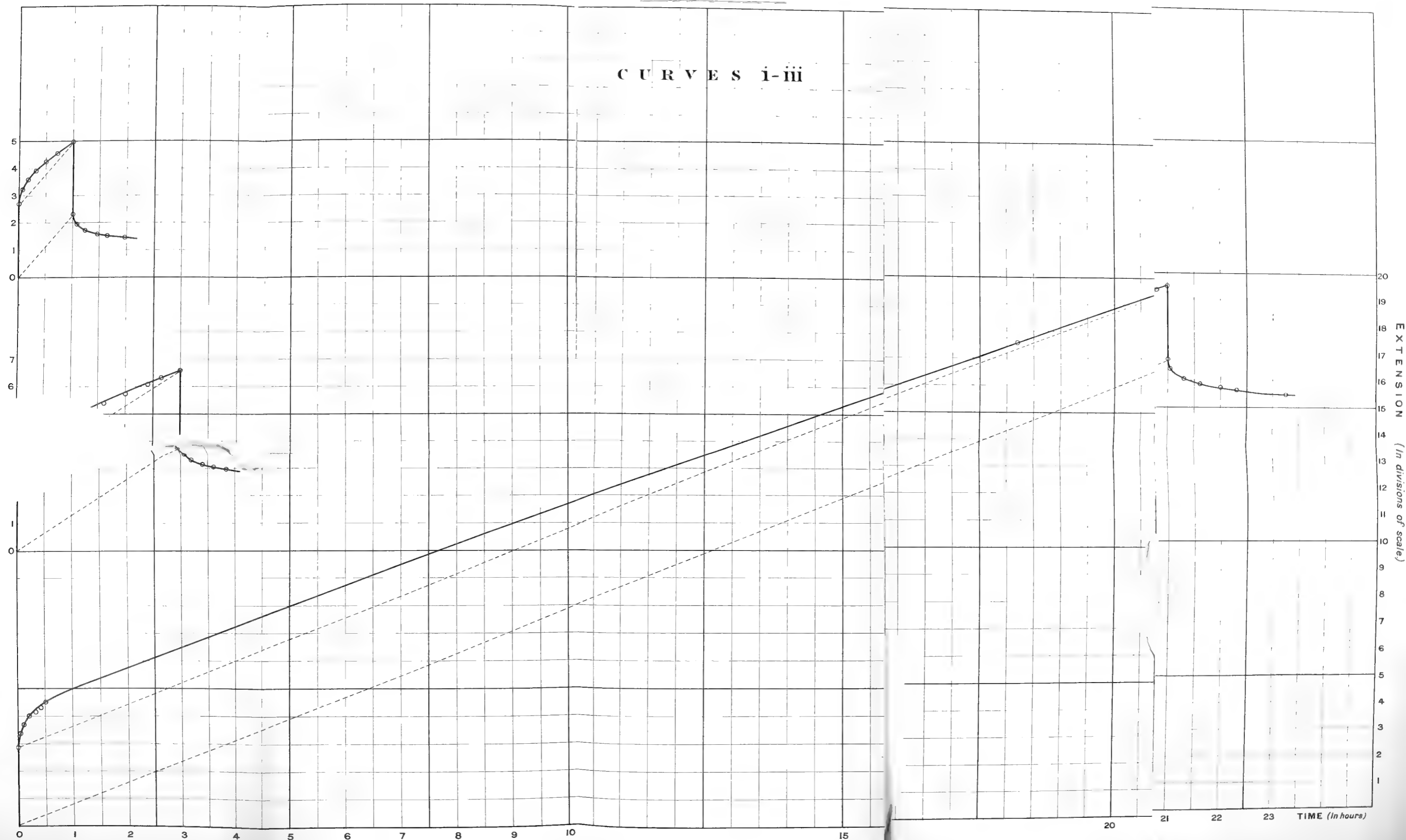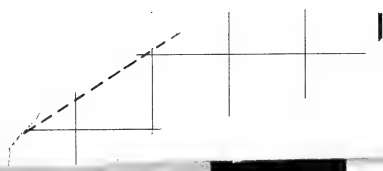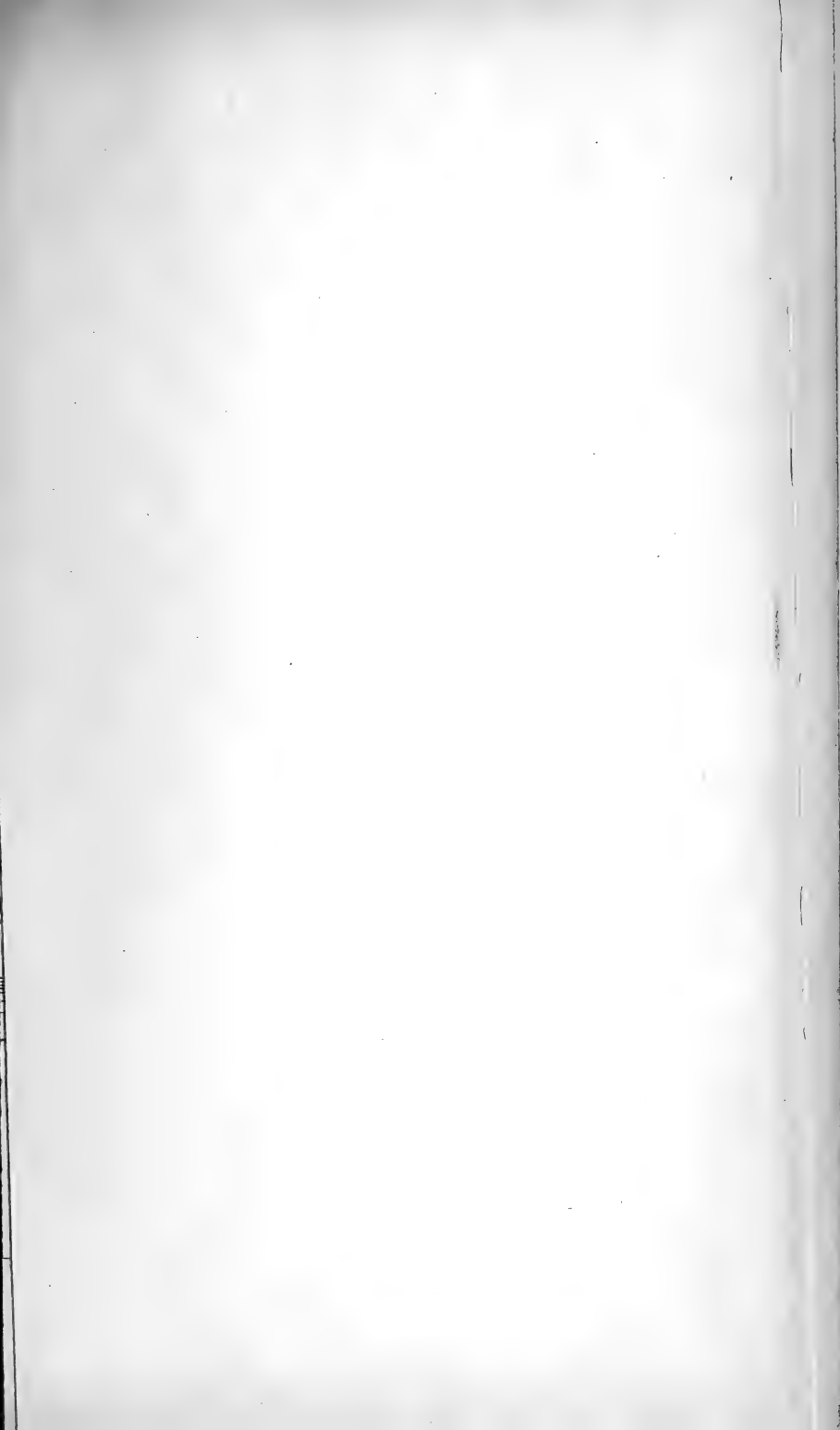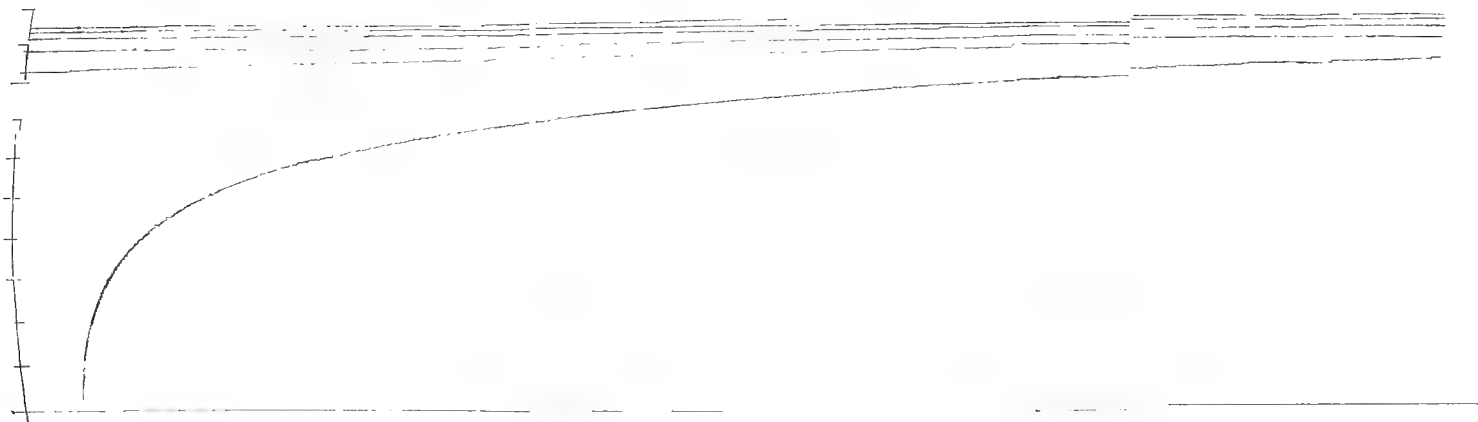
CURVES i-iii

CURVE VIII.

CURVE IX.
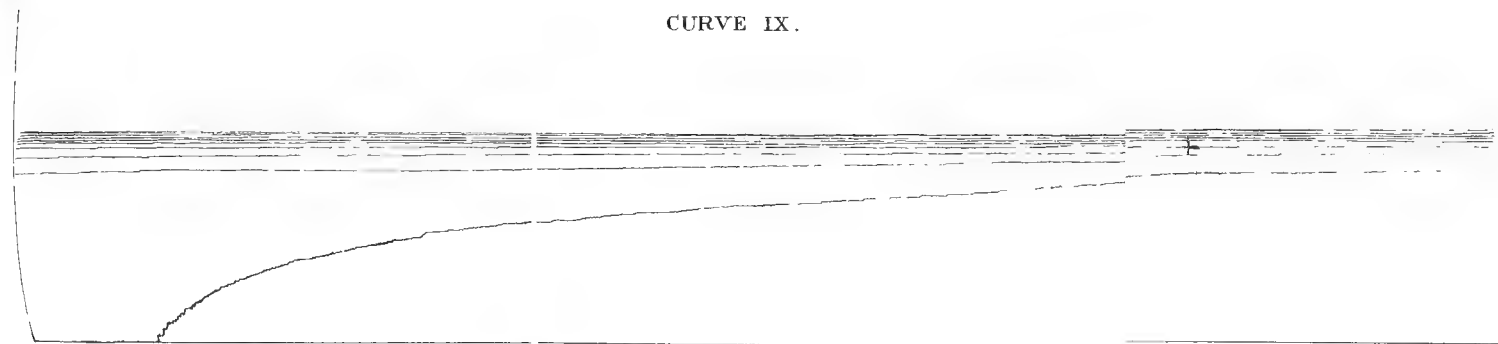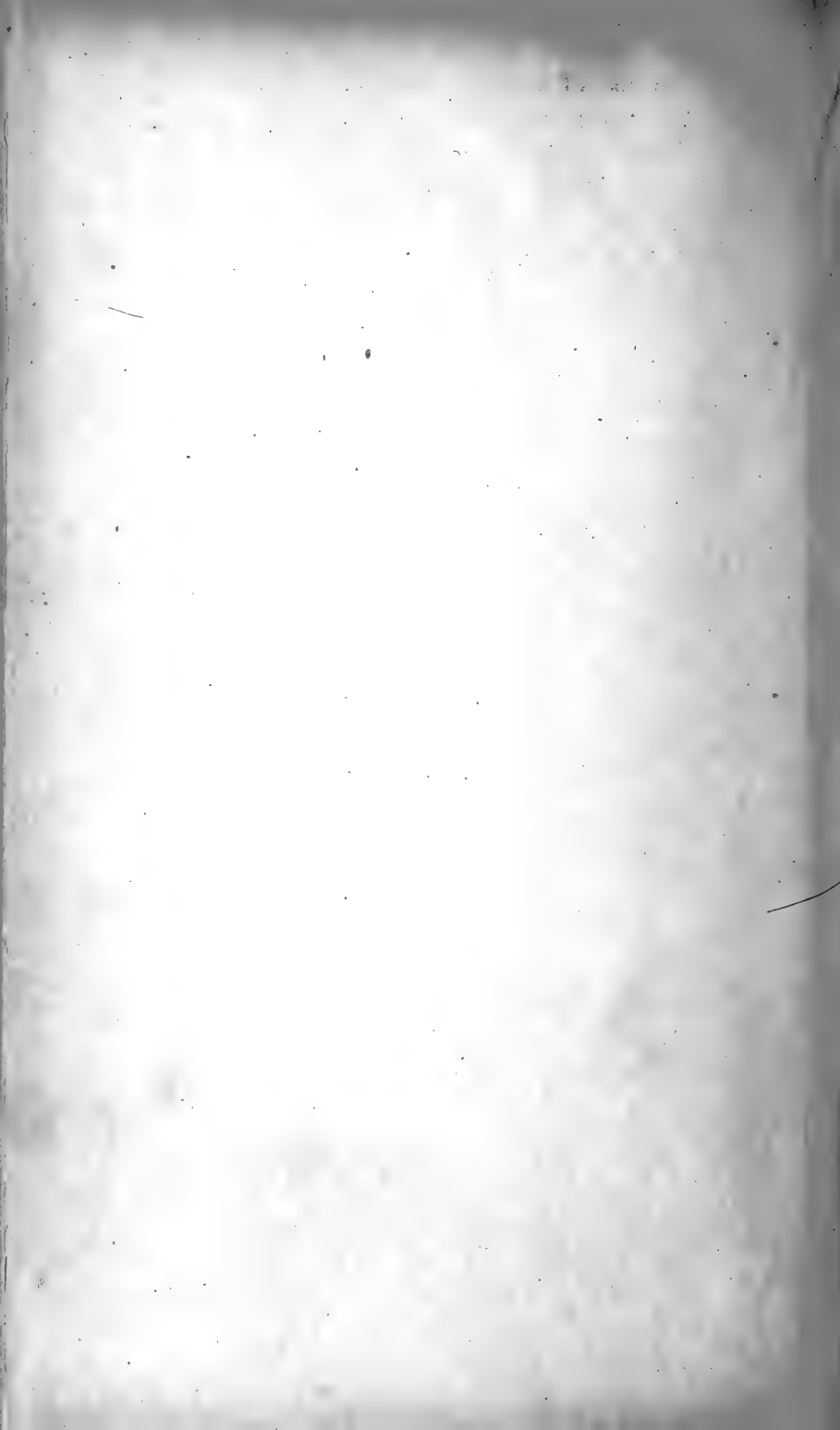
LVI. *On the Vibrations of Strings excited by Plucking and by Resonance.* By W. B. Morton, *M.A., and* T. B. Vinycomb, *M.A., Queen's College, Belfast*[*].

[Plate XVI.]

Introduction.

A PHOTOGRAPHIC method of recording the motion of a particular point on a vibrating string was devised by Krigar-Menzel and Raps[†] in 1891. It consists in forming, in the plane of vibration of the string, a real image of a brightly illuminated slit, and photographing this on a moving plate. The final image of the slit is thus broken by a dark point where the string crosses it. The combination of the up-and-down motion of the string and horizontal motion of the plate causes this point to trace a wavy line, which is the graph of displacement against time for the chosen point of the string. The mode of vibration can thus be compared with theory.

The method has been applied by its inventors to the examination of bowed[‡] and of plucked[‡] strings; by Kaufmann[§] to pianoforte strings; and by Klinkert[||] to strings kept in vibration by electromagnetic means.

Our original object was to obtain a record of the initial stages in the motion of a string set in sympathetic vibration

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[*] Communicated by the Authors.
[†] Krigar-Menzel and Raps, Wied. *Ann.* vol. xliv. p. 623 (1891).
[‡] Krigar-Menzel and Raps, *ibid.* vol. l. p. 444 (1893).
[§] Kaufmann, *ibid.* vol. liv. p. 675 (1895).
[||] Klinkert, *ibid.* vol. lxv. p. 849 (1898).

by another string, tuned to the same pitch and mounted on the same supporting frame. We wished to see how promptly and how closely the primary vibrations were copied by the secondary string. The primary string was plucked, as this gives a characteristic and definite form of vibration-curve.

At an early stage in the experiments we noticed certain peculiarities in the vibration-forms of the plucked string which called for explanation. In the experiments of Krigar-Menzel and Raps these forms gradually diverged from the ideal in a certain definite way, which they showed could be explained by the yielding of the supports at the ends of the string. In some of our photographs quite different forms appeared. We made further experiments in order to find out the cause of these differences; and in the course of them we examined the effect of altering the size, tension, and material of the string, and also the nature of the supporting structure.

Experimental Arrangement.

In our arrangement the string, placed horizontally, was drawn aside in the vertical plane and held in a notch cut in the edge of a vertical blade, near its lower end. The blade was movable on a horizontal axis, parallel to the string, and carried at its upper end a piece of soft iron which could be drawn aside by an electromagnet, so releasing the string Parallel and close to the notched blade was a fixed blade. Its edge, bearing against the string, prevented it from following the notch when the latter was drawn aside. The efficient action of this " plucker " was shown by the conformity of the first vibrations of the string to the theoretical form. The whole arrangement was made to work electrically. The photographic plate was carried by a long pendulum. This was held back by an electromagnetic catch, and released by depressing a key. At a certain point of its course the pendulum knocked over a key actuating the " plucker," and when at the middle of its swing it came in the path of the beam of light from the lantern. By varying the position of the plucking-key we could get different stages of the motion caught on the plate.

Theoretical Motion of Ideal String with Fixed Ends.

The theoretical motion of a plucked string may be described as follows [*]. Initially the string is made up of two straight pieces meeting at the point of discontinuity where the string

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[*] Helmholtz, 'Sensations of Tone' (Eng. trans.), p. 54; Rayleigh, 'Sound,' vol. i. p. 231.

is plucked. Complete the parallelogram of which these parts are adjacent sides. The opposite sides constitute the other position of rest of the string. To get the intermediate forms, suppose two points to start from the point of pluck and to move uniformly in opposite directions round the parallelogram, passing each other at the points of discontinuity of the extreme forms. The shape of the string at any instant is made up of the line joining these two travelling points together with portions of the sides of the parallelogram.

It is easy to see that each point of the string will remain at rest for certain intervals at each extreme position, and will move from one extreme to the other at a uniform speed. So the vibration-curve will be made up of straight lines alternately horizontal and sloped (see the first vibrations on the accompanying photographs), the relative durations of the different stages will depend on the positions of the point plucked and the point whose motion is observed. The appropriate formulæ are easily deduced, and will be found discussed very fully in the first paper of Krigar-Menzel and Raps.

Motion of a Resonating String.

Photographs 2 & 3 (Pl. XVI.) show the initial stages of the motion in our earlier experiments with two strings, stretched on a simple wooden frame without a resonance-cavity. It will be seen that the disturbance of the secondary string begins at once. The primary vibrations rapidly lose their symmetry, and their behaviour is copied by the secondary vibrations, the downward motion being more gradual than the upward. Further, it is to be noted that only the lower harmonics the series included in the primary vibrations are transferred to the secondary string. Indeed, the secondary forms may be built up of the fundamental and octave only. This shows that, although the higher harmonics rapidly disappear, as we shall see, from the motion of the plucked string, they are not taken up by the frame. We may take the frame to have a much lower natural period than the string, and so explain the greater readiness with which it takes the lower notes.

The only remaining point to be noticed concerns the phase-relation between the two vibrations, which is closely connected with the transfer of energy taking place between the strings. The simple mode of steady vibration with motionless nodes, the same phase at all points between two nodes, and a sudden change to the opposite phase in passing a node, is incompatible with an energy transfer in either direction. The work done by any portion of the string as its ends move outward

from the mean position is equal to that done on the string in the inward motion. In order that this balance may be upset, we must have a difference of phase between the displacement of the string at any point and its inclination. This involves a progressive change of phase in the vibrations as we go along the string. The "nodes" are now points of minimum amplitude, falling midway between the loops where the amplitude is greatest. There is a phase-difference π between consecutive loops; but this is now spread over the whole intervening segment instead of being concentrated at the node. The change of phase is much more rapid near the node than elsewhere. We shall show that the rate of change of phase per unit length varies directly as the energy transfer across that point of the string, and inversely as the square of the amplitude. The phase at the node itself will clearly be midway between the phases of the adjacent loops; so that this point will be passing though the middle of its very small excursion when the parts of the string near the loops are in their extreme positions.

Lord Rayleigh [*] has examined a special case of motion of this kind in his discussion of the steady motion of a string when there is a frictional term in the equation of motion. We may apply general considerations as follows.

Let x be the distance of a point on the string from an arbitrary origin, y the lateral displacement of this point. Suppose that $y = f(x) \sin (pt + \epsilon)$, where ϵ is a function of x which we may take as vanishing at the origin. Then, if T is the tension of the string, and if we consider the string lying on the positive side of the origin, the energy *leaving* this part of the string, at the origin, is

$$\int \mathrm{T} \left(\frac{dy}{dx}\right)_0 dy$$

$$= \int \mathrm{T} \left(\frac{dy}{dx}\right)_0 p f(0) \cos pt \, dt \; ;$$

and

$$\left(\frac{dy}{dx}\right)_0 = f'(0) \sin pt + f(0) \cos pt \cdot \left(\frac{d\epsilon}{dx}\right)_0.$$

[*] 'Sound,' vol. i. p. 198. There is an error in formula (9) of § 134, owing to the omission of second-order terms in taking approximate values for α and β. The term in κ^2 in the numerator should be

$$\frac{\kappa^2 x}{4ap} \left[\frac{px}{a} + \sin \frac{px}{a} \cos \frac{px}{a} \right],$$

with a corresponding change in the denominator.

Integrating through a period $\dfrac{2\pi}{p}$, we find the energy transfer from the string at $x = 0$

$$= \pi \mathrm{T} \{f(0)\}^2 \left(\frac{d\epsilon}{dx}\right)_0;$$

∴ rate of change of phase $\propto \dfrac{\text{energy loss}}{(\text{amplitude})^2}.$

We see that if a string is losing energy by its ends, as is the case with the plucked string in our experiments, there is a lag of phase from the centre outwards. When the ends are outside the nodal points, this lag is greater than $\dfrac{\pi}{2}$ but less than π. This is, as Lord Rayleigh has shown, the state of the case when the ends are yielding and have a natural period longer than that of the string, the pitch of the note being then higher than for a string of the same length with fixed ends. Conversely, for the secondary string, which is absorbing energy by its ends, we have the central parts lagging behind the ends. Using "crest" and "trough" for the extreme positions, as shown on the vibration-curves, we should expect the crest of the end-motion to fall before the trough of the primary vibration, and the crest of the secondary vibration to come after the crest of the end-motion. The result is that the crest of the secondary is found, as is shown on the photographs, *after* the trough of the primary vibration.

Deviations from the ideal forms in the motion of a Plucked String. Theoretical.

In the experiments of Krigar-Menzel and Raps the deviations from the ideal shown by the vibration-forms consisted in a slow *forward* inclination of the horizontal parts and a very slight curvature of the sloping parts. The tops and bottoms remained straight, or showed very small wrinkles, and the corners remained sharp. We get this result when we employ a *fine* steel wire (photo. 1, Pl. XVI.).

Working with strings of other thicknesses and materials, we have found in addition, first, marked wrinkles generally unsymmetrical in position with rounding of the corners; and second, in certain cases a *backward* sloping of the tops and bottoms of the vibration-curves.

If we regard the form of the vibration-curve as built up of a Fourier series, we have two ways in which the shape can be altered; viz. (1) by a change in the relative *amplitudes*, and (2) by a change in the relative *phases* of the simple

harmonic components.   The latter change will account for the sloping of the horizontal parts of the curve; the former for symmetrical wrinkling and rounding of corners; the two combined for the unsymmetrical wrinkling.

(1) The alteration in the relative amplitudes is due to the fact that the higher frequencies are much more rapidly damped out by the internal viscosity of the material than are the lower. This was first pointed out by Lord Kelvin for the case of torsional oscillations. The effect in the present case is to continuously reduce the number of components present until finally, in photographs of advanced stages of the motion, there is only a simple sine-curve left. This preferential damping of the higher harmonics is specially marked with gut strings.

A comparison of the results got by putting the same string on different frames, shows that the viscosity concerned is chiefly in the string itself. As already mentioned, the frame takes up the *lower* tones more readily.

(2) The progressive alteration in the relative phases of the components means that the overtones of the string do not form an accurately harmonic series. This may arise from three causes :—

(*a*) *The yielding of the supports.*—If M is the effective mass of the end-support, its elasticity being neglected ; T, *l*, the tension and length of the string, and *a* the velocity of sound along it ; then the *s*th partial tone is raised in pitch through the interval[*]

$$1 + \frac{2\mathrm{T}l}{\mathrm{M}a^2\pi^2} \cdot \frac{1}{s^2}.$$

Thus the higher partials are too flat in comparison with the lower. Krigar-Menzel and Raps have shown that this will cause the horizontal parts of the curve to slope *forwards*.

(*b*) *The rigidity of the wire.*—The effect of this is to raise the *s*th partial through the interval [†]

$$1 + \frac{\pi^3 r^4 q}{8l^2\mathrm{T}} \cdot s^2,$$

where *r* is the radius of the wire and *q* is Young's modulus for its material. The higher overtones are raised too much : so the effect of rigidity is opposite to that of end-motion, and will make the horizontal parts slope *backwards*.

(*c*) *Inequality in the sectional area of the string.*—A small

[*] Rayleigh, 'Sound,' vol. i. p. 204.
[†] Rayleigh, 'Sound,' vol. i. p. 298; where " T " is used for our $\frac{\mathrm{T}}{\pi r^2}$.

isolated load whose mass is μ times that of the string, at a point dividing the string in the ratio $z : 1 - z$ lowers the pitch of the sth overtone through the interval[*]

$$\sqrt{1 + 2\mu \sin^2 sz\pi}.$$

The relative displacements of the pitches will of course alter with the position of the load. On examination, it appears that a greater flattening of the higher partials is the more probable result. For example, if the load is on either of the outside thirds of the string, the octave is too flat, relatively to the fundamental. But with a proper arrangement of density-variations along the string, this cause of disturbance might imitate either of the two others.

Comparison of Observed Results with Theory.

In the case of *metal strings*, which may be taken to be uniform, we should expect the forward or backward slope to appear according as the effect of motion of the ends, or of the rigidity of the string, predominates. If we write $1 + \dfrac{P}{s^2}$ and $1 + Qs^2$ for the expressions occurring under heads (*a*) and (*b*) above, we may call P and Q the end-coefficient and the rigidity-coefficient respectively. It is obvious that the rigidity effect will rapidly increase in importance as we go to overtones of high order ; but the form of the curve will be settled in the main by the relative pitch-displacements of the lower partials. The magnitude $\dfrac{P}{Q}$ will give a measure of the tendency of the forward slope to prevail ; *e. g.*, if $\dfrac{P}{Q} > 4$, then the displaced octave is flat in reference to the displaced fundamental. Putting in the values we get

$$\frac{P}{Q} = \frac{\text{end-coefficient}}{\text{rigidity-coefficient}} = \frac{16}{M\pi^5} \frac{l^3 T^2}{a^2 r^4 q}.$$

It is convenient to use the frequency n and the density ρ of the string instead of T and a. The formula then becomes

$$\frac{P}{Q} = \frac{64}{M\pi^3} \frac{l^5 n^2 \rho^2}{q}.$$

Thus increasing the length of the string and raising the pitch would tend to bring on the forward slope. The radius of the string does not appear in the final expression ; so the form of the vibration-curve should be the same for strings of

[*] Rayleigh, 'Sound,' vol. i. p. 215.

different thicknesses and the same length, tuned to the same pitch. In our experiments with pianoforte wires we obtained results in qualitative agreement with these conclusions.

Working with *gut strings* the results were less regular. This we ascribe partly to the irregularity in the diameter and the want of homogeneity in structure of the strings, and partly to the varying effect of the moisture absorbed from the air. One string, which showed a pronounced rigidity effect when dry, gave equally pronounced end-motion shapes when slightly damped. The frequency with which, as compared with steel, the rigidity forms were obtained, seemed at first sight inconsistent with the greater rigidity of the metal. But in the expression found above for $\dfrac{P}{Q}$, we have the *square* of the density in the numerator and the first power of Young's modulus in the denominator, so that the lightness of the gut may balance the effect of its smaller elasticity. We made some measurements of " q " for gut strings, taking care that the string was not allowed to untwist when the load was increased. The value came out $3.8 \times 10^4 \dfrac{\text{kilos}}{\text{cm.}^2}$. The density is about 1.1. For steel the corresponding values are 2.2×10^6 and 7.7.

$$\therefore \ \frac{\rho^2}{q} = 3.2 \times 10^{-5} \text{ for gut,}$$

and 2.7×10^{-5} for steel ;

values of the same order of magnitude.

Gut strings show great internal viscosity and consequent rapid extinction of the high partials.

Now it is known from the general theory that those partials which have a node either at the point which is plucked, or the point whose motion is observed, do not enter into the composition of the vibration-curve. So with the higher partials disappearing, and certain of the lower ones evicted by a proper choice of the points of pluck and of observation, we can obtain curves built up of very few components. Photograph 4 is obtained from a gut string, plucked at $\frac{1}{5}$ of its length from one end and observed at $\frac{1}{2}$. The rapidly recurring cycle of forms shows that we have got practically the fundamental and third partial unaccompanied by higher harmonics ; and that the mistuning of the third partial is considerable. The cycle occupies 18 vibrations, whence it is easy to deduce that the upper component is too sharp by the interval $\frac{55}{54}$, relative to

the lower. Photograph 5 is from the same string plucked at $\frac{1}{5}$ and observed at $\frac{1}{3}$. The simple harmonic components are now limited practically to the fundamental and octave, the latter being too sharp by the interval $\frac{129}{128}$.

Vibration-forms and Timbre.

An interesting point which arose in the course of the investigation was that the vibration-forms were independent of the quality of the tone heard, and of the presence on the same frame of strings resonating with the plucked string. We photographed the motion of a gut string of a guitar (the G string) under three conditions : (1) with the other strings removed ; (2) with the other strings on the instrument and tuned to resonate : the lowest string to the octave below the plucked string, the next two to unison with it, and the two upper strings to the octave above ; (3) the string mounted on a rough wooden frame, on which it gave a tone of very inferior quality. The vibration-forms followed exactly the same course in all three cases. This seems to imply that the supporting frames take the same component vibrations from the string, but act differently in their way of communicating to the air the vibrations imparted to them. The vibrations of the string are given to the air *via* the sounding-board. It would seem that the selection of the harmonics, determining the good or bad quality of the tone heard, is made at the transference from sounding-board to air, and not at that from string to sounding-board.

Queen's College, Belfast.
August 2nd, 1904.

LVII. *On the Lateral Vibration of Bars.*
By C. A. B. GARRETT, *University College, Nottingham* [*].

[Plate XVII.]

THE problem of the lateral vibration of bars has been treated very fully by Lord Rayleigh[†]. He also shows that in the case of a bar fixed at one end we can get an approximate expression for the frequency of the fundamental vibration, if we assume the shape of the vibrating bar to be the same as if it were pulled aside by a force at the end. Further, he goes on to show[‡] that we get a closer approximation, if we assume the shape to be the same as if pulled

[*] Communicated by Dr. E. H. Barton.
[†] 'Theory of Sound,' vol. i. chap. viii.
[‡] *Loc. cit.* § 182.

aside by a force applied at a point on the bar distant a quarter of its length from the free end.

In obtaining this result, he quotes expressions derived in a former chapter for the kinetic and potential energy of the vibrating bar. Moreover, he simply gives the result as an interesting fact, and gives no experimental justification of his assumptions.

It occurred to me that it might be possible to put the solution in a somewhat simpler form, involving only very elementary differentiation and integration. This was done, and experiments were then conducted with a view to the justification or otherwise of the assumptions. These experiments show that a bar pulled aside by a force applied about one-fifth of its length from the free end, approximates very closely to the form assumed by the same bar when vibrating. Then the simple theory adapted to this state of things gives a value for the frequency within 1·4 per cent. of that obtained by the full theory.

Simple Theory.

Assume for a first approximation, as Rayleigh does, that the shape of the vibrating bar is the same as that of the same bar pulled aside by a force at the free end. Knowing the equation to the curve assumed by the bar, we can find the moment about the fixed end of all the forces supposed to be producing motion of the particles of the bar ; then equating this to the bending moment at the fixed end, we get an expression enabling us to find the frequency of vibration.

Fig. 1.
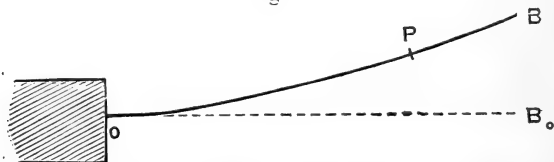
Let OB be the vibrating bar, fixed at O ; OB_c being the
        mean position ;
    L be the length of the bar ;
    κ the radius of gyration of the cross-section about the
        neutral axis ;
    U the velocity of longitudinal waves in the bar ;
    E Young's modulus for the bar ;
    σ the volume density of the bar ;
    A the area of cross-section of the bar ;
    x and y coordinates of P measured parallel and per-
        pendicular to OB_0.
    ρ_0 the radius of curvature of the bar at O.

Let us assume that we may consider the motion of P to be perpendicular to OB_0, and that the kinetic energy of rotation is negligible in comparison to that of translation.

It may be easily proved (see any book on Elasticity) that the curve is given by the equation

$$y = \frac{W}{EA\kappa^2} \frac{x^2(3L-x)}{6},$$

W being the force applied at the end. Then, denoting by y_1 the deflexion of B,

$$\frac{y}{y_1} = \frac{x^2(3L-x)}{2L^3}, \quad . \quad . \quad . \quad . \quad (1)$$

$$\therefore \quad \frac{d^2y}{dt^2} \quad \text{or} \quad \ddot{y} = \ddot{y}_1 \frac{x^2(3L-x)}{2L^3}. \quad . \quad . \quad (2)$$

Now the moment about O of the forces causing motion of the bar is :—

$$\int_0^L x\ddot{y}\sigma A dx$$

$$= \frac{\sigma A\ddot{y}_1}{2L^3} \int_0^L x^3(3L-x)dx \quad \text{from (2)}$$

$$= \frac{11}{40}\sigma A L^2\ddot{y}_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Also the bending moment at O,

$$= \frac{EA\kappa^2}{\rho_0} = EA\kappa^2\left(\frac{d^2y}{dx^2}\right)_{x=0}$$

$$= \frac{3y_1}{L^2} EA\kappa^2 \quad \text{from} \quad (1). \quad . \quad . \quad (4)$$

Equating (3) to (4) we have

$$\frac{\ddot{y}_1}{y_1} = \frac{120}{11} \frac{\kappa^2}{L^4} \frac{E}{\sigma}. \quad . \quad . \quad . \quad . \quad (5)$$

This shows us that the bar vibrates in a simple harmonic manner and the frequency

$$N_1 = \frac{1}{2\pi}\sqrt{\frac{\ddot{y}_1}{y_1}}$$

$$= \cdot 525 \frac{\kappa U}{L^2}. \quad . \quad . \quad . \quad . \quad (6)$$

Rayleigh gives the frequency in the form

$$N = \frac{m^2}{2\pi} \frac{\kappa U}{L^2},$$

m for the fundamental being $1 \cdot 192 \frac{\pi}{2}$. Our theory gives an $m_1 = 1 \cdot 16 \frac{\pi}{2}$, or

$$\frac{N_1}{N} = \cdot 95.$$

Second Approximation.

Now let the bending force be applied at a point on the bar distant pL from the fixed end.

In this case let y_1 be the deflexion of the point of application of the force. Then the bending moment at O, as before, is

$$\frac{3y_1}{p^2 L^2} E A \kappa^2. \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Also the moment of the forces causing motion of the bar is

$$\frac{11}{40} \sigma A (pL)^2 \ddot{y}_1 + \sigma A \ddot{y}_1 \int_{pL}^{L} x \left[1 + \frac{x - pL}{y_1} \mu \right] dx,$$

where μ is the slope of the bar between $x = pL$ and $x = L$. $\frac{\mu}{y_1}$ is easily seen to be $\frac{3}{2pL}$ from (1).

$\therefore$ moment of forces about O

$$= \sigma A p^2 L^2 \ddot{y}_1 \frac{p^3 - 10p + 20}{40\rho} . \quad . \quad . \quad . \quad (8)$$

Equating (7) and (8) we have

$$N_2 = \frac{1}{2\pi} \frac{\kappa U}{L^2} \sqrt{\frac{120}{p(p^3 - 10p + 20)}} . \quad . \quad . \quad (9)$$

Photographs of the Vibrating Bars.

Instantaneous photographs were taken of a bar when vibrating and of the same bar when pulled aside to the same *end* displacement. The bar used was a steel strip, and the dimensions were : length 94 cms., breadth 1·25 cms., thickness ·13 cm. This bar is clamped tightly in a vice and set

up immediately in front of a white screen ruled in squares. The bar is set in motion by drawing aside and then releasing. At first the bar was pulled by the end, but afterwards it was always pulled by a point one-fifth of the way down. After a good number of swings the bar is photographed when near the full extent of its swing. From the negative we know the position of the bar when taken. It is then pulled aside by a force applied at any point and held in such a position that the *end* is in the same position as when photographed in motion. A photograph is then taken of the bar held thus, the camera being in exactly the same position as before. We thus obtain one photograph of the dynamical curve (as we may call the shape of the vibrating bar) and a number of statical curves. By double printing we compare the former with each of the latter.

The first photograph (Pl. XVII. fig. 2) shows the bar vibrating, compared with the same bar held aside by a force at the end. The conditions of the experiment make the two curves coincide at the ends, but near the middle they are quite distinctly separate. The vibrating rod appears on the outside.

In the second photograph (fig. 3) the bar is held by a force applied one-tenth of the way along the bar from the free end. The two curves almost coincide, a slight divergence being indicated by a thickening of the apparently single line.

In the third (fig. 4) the force is applied one-fifth of the way down. Here we have almost perfect coincidence.

In the fourth (fig. 5) the force is applied three-tenths of the way down. We have again two distinct curves, as in the first case, but now the vibrating bar appears on the inside.

Displacement observations.—A second method of experiment was adopted, which we may call the microscope method.

It will be seen from the photographs (as might be expected) that the greatest divergence between the two curves is near the middle of the bar. Then we can make comparisons between the curves by direct observations of the displacement of the bar from its mean position (1) at the free end, and (2) at the middle. This was done as follows:—two traversing microscopes were focussed on the bar when pulled aside, one at the free end and the other at the middle. It was found that if we start the bar vibrating by pulling it aside to a fixed block and releasing, the amplitude after a given number of swings will be a fixed amount, and the condition of things

can be repeated.   To find the required displacements for the dynamical curve we proceed thus :—

The bar is set in vibration by holding it up to the block A (fig. 6) and then releasing. We are guided by the photographs

Fig. 6.
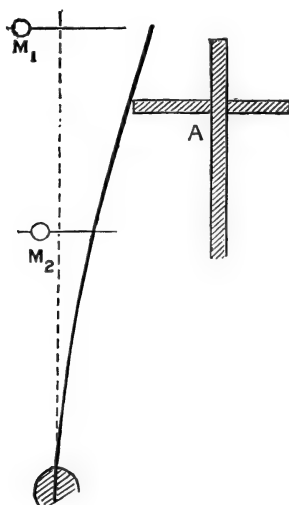
to set A so as to touch the bar one-fifth of the way down. The position of A is adjusted until the bar comes up to the cross-wire in M_1 at (say) the 20th swing. M_2 is then run along until the bar comes up to its cross-wire after the same number of swings ; the reading of M_2 is taken. The bar is now held aside in such a position that the top is again on the cross-wire of M_1. M_2 is again set on the bar and the reading taken. In the latter case we may apply the bending force at any point along the bar, and the reading of M_2 will be different in each case.

In this way we make the displacement of the end the same for all cases, while that of the middle varies with the conditions, and may be regarded for our purpose as the characteristic of a given curve. Three distinct sets of readings are given below (p. 587). In each case the displacement of the end of the bar is 7·68 cms.

We see from these results that the two curves would coincide at the middle and the two ends, if the force were applied at a point on the bar distant ·23 of the length from the free end.

| Character of the curve.	Distance of force from free end.	Displacement of the middle of the bar.		
		A.	B.	C.
Dynamical...	...	2·63	2·63	2·63
Statical	·0 × L	2·45	2·47	2·46
,, ...	·1 × L	2·51	2·53	2·53
,, ...	·2 × L	2·59	2·60	2·60
,, ...	·3 × L	2·70	2·71	2·71

Prime and Overtones.—When we set a bar in vibration by drawing it aside and releasing, we must have present a series of vibrations in order to build up the initial state of things ; these die away at different rates. If we had only the fundamental present, we should get a regular dying away of amplitude at all points on the bar ; then the ratio of the amplitudes at the middle and the end would be constant throughout. The presence of the overtones makes this vary within certain small limits. This point is shown experimentally in the following manner :—The stop A and the microscopes M_1 and M_2 are adjusted as previously described, but now we take readings at the extreme of the first swing. M_1 then remains fixed for the rest of the experiment, while A and M_2 are adjusted to take readings after three, five, seven, &c., swings (one swing being half a complete vibration). Thus after n swings we have made the amplitude of the top of the bar a constant amount α, and therefore the ratio of the amplitudes of the middle and the end will be simply proportional to the displacement as read by M_2. Any change in the curve of the bar, other than simply dying away, will be shown by a change in this displacement. We have then a series of readings of M_2 as n is varied ; the variation in the former is shown very well by plotting. In this case series of readings were taken with the stop A (1) at the end of the bar (see curve C_1 fig. 7) ; (2) one-fifth of the way down (see curve C_2) ; (3) two-fifths of the way down (see curve C_3). The three diagrams are given below (p. 588).

The total displacement at the middle was 26 mms., the greatest deviation from the mean 2 mms., while the experimental error in the most favourable cases was only about $\frac{1}{10}$ mm.

The points determined lie on a regular curve, something like a sine-graph of decreasing amplitude. C_1 and C_3 are very nearly the reverse of each other, C_1 having a crest where C_3 has a hollow, and *vice versa*. This follows from the fact that we are a considerable distance from the point where the

statical and dynamical curves nearly agree, and on opposite
sides of it in the two cases. In the case of C_2 we are very
near the required point, consequently the curve becomes

Fig. 7.

Time from commencement of vibration.
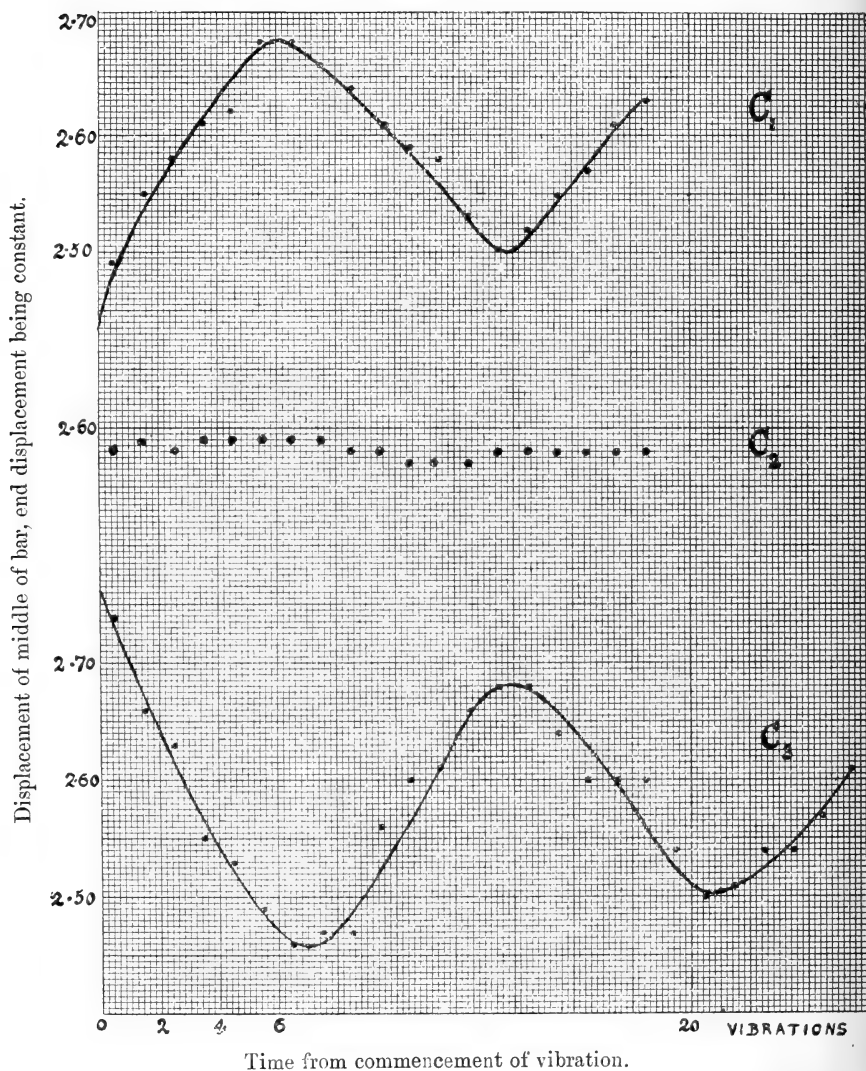
almost a straight line parallel to the axis of x. The irregu-
larities due to experimental error are here comparable with
the variation due to the presence of overtones, although there
is still some indication of the presence of the latter.

Final results.—The departure of the curves from a straight horizontal line shows the presence of overtones as well as the prime tone ; while the fact that the curves appear to asymptote to this line shows that the overtones die away quicker than the fundamental.

By both methods of experiment we see that the two curves agree best when $p = \cdot 8$; then substitute this value in (9) and we get a value for the frequency of vibration such that

$$\frac{N_1}{N} = \cdot 986.$$

I hope to make further experiments on tuning-forks or bars of acoustic frequency. I desire to render my best thanks to one of my colleagues, Dr. E. H. Barton, for his kind help in the work.

University College, Nottingham,
July 14, 1904.

LVIII. *The Limits of Economy of Material in Frame-structures.* By A. G. M. MICHELL, *M.C.E., Melbourne* [*].

MAXWELL has shown [†] that for all frames under a given system of applied forces

$$\Sigma . l_p f_p - \Sigma . l_q f_q = C, \quad . \quad . \quad . \quad . \quad (1)$$

where f_p is the tension in any tie-bar of length l_p, f_q the thrust in any strut of length l_q, and the first sum is taken for all the ties, the second for all the struts. C is shown to be a function of the applied forces and the coordinates of their points of application, and independent of the form of the frame.

Starting from this result, we can find in certain cases lower limits to the quantity of material necessary to sustain given forces, and also assign the forms of frames which attain the limit of economy.

If the greatest tensile stress allowable in the material which is to be employed is P, and the greatest compressive stress Q, the least volume of material in a given frame, consistent with security, is

$$\Sigma . l_p \frac{f_p}{P} + \Sigma . l_q . \frac{f_q}{Q} = V. \quad . \quad . \quad . \quad (2)$$

Now, considering a number of different frames in equilibrium under the same set of external forces, in that one of the frames in which V is least

$$2PQ . V + (P-Q)C \quad \text{is also least;}$$

i. e., $2PQ \left\{ \Sigma . l_p \dfrac{f_p}{P} + \Sigma . l_q \dfrac{f_q}{Q} \right\} + (P-Q) \{ \Sigma . l_p f_p - \Sigma . l_q f_q \}$

$$= (P+Q)\{ \Sigma . l_p f_p + \Sigma . l_q f_q \} \quad \text{is least,}$$

or $\qquad \Sigma . l [f]$ is least, ... (3)

where $[f]$ denotes the numerical value of the force in any bar, and the sum is taken for both struts and ties.

One proof given by Maxwell of the equation (1) above depends on consideration of the virtual work of the applied forces and internal stresses during an imposed uniform dilatation or contraction of the frame. Consideration of a more general type of imposed deformation will furnish information as to the quantity $\Sigma . l [f]$.

Let the space within a given boundary, which encloses a number of different frames subjected in turn to the same set of applied forces, undergo an arbitrary deformation of which the frames partake and such that no linear element in the space suffers an extension or contraction numerically greater than $\epsilon . \delta l$, where δl is the length of the element and ϵ a given small fraction.

In this deformation, let any bar of length l of one of the frames, A, undergo the small change of length el, which is to be taken as positive when it increases the existing strain in the bar due to the applied forces, negative in the contrary case. The increase in the elastic energy of the bar is elf, and for the whole frame

$$\Sigma . elf = \delta W,$$

by the principle of Virtual Work, where δW is the virtual work of the applied forces, and is independent of the form of the frame A.

Thus $\qquad \delta W = \Sigma . elf \not> \Sigma . [e] l [f]$

$$\not> \Sigma . \epsilon l [f] \not> \epsilon \Sigma . l [f]$$

or $\qquad \Sigma . l_A [f]_A \not< \dfrac{\delta W}{\epsilon},$

indicating by the suffixes that the inequality applies to the frame A.

If, however, a frame, M, can be found, such that all its parts have their strains increased equally and by as much

as any elementary line in the deformed space, *i. e.* if $e = \epsilon$ in all parts, the signs of inequality may be replaced by that of equality, and

$$\Sigma . l_{\mathrm{M}} \left[f \right]_{\mathrm{M}} = \frac{\delta \mathrm{W}}{\epsilon} \ngtr \Sigma . l_{\mathrm{A}} f_{\mathrm{A}},$$

so that $\Sigma . l_{\mathrm{M}} \left[f \right]_{\mathrm{M}}$ is a minimum, and consequently from (3), V_m, the volume of material in the frame M, is also a minimum.

A frame therefore attains the limit of economy of material possible in any frame-structure under the same applied forces, if the space occupied by it can be subjected to an appropriate small deformation, such that the strains in all the bars of the frame are increased by equal fractions of their lengths, not less than the fractional change of length of any element of the space.

If the space subjected to the deformation extends to infinity in all directions, the volume of the frame is a minimum relatively to all others, otherwise it will have been shown to be a minimum only relatively to those within the same assigned finite boundary.

The condition $e = \epsilon$ can evidently be satisfied when all the bars of a frame have stresses of the same sign. The test deformation to be applied is then a uniform dilatation or contraction (according as the frame is in tension or compression) of a space enclosing the frame and extending to any finite boundary or to infinity at pleasure.

The simplest minimum frames of this special class are :—

   I. Ties and struts subjected to a single pair of equal and opposed forces.
   II. Triangular and tetrahedral frames under forces applied at the angles of the figure, and acting along lines which intersect within the figure.
   III. Catenaries in general, the points of application of the applied forces, as well as the forces themselves, being given.

In all these cases the minimum volume of the frame is

$$\mathrm{V}_m = \frac{\Sigma . l[f]}{\mathrm{P}} = \frac{\delta \mathrm{W}}{\epsilon \mathrm{P}} = \frac{\Sigma . \epsilon \mathrm{F} r \cos \theta}{\epsilon \mathrm{P}}$$

$$= \frac{\Sigma . \mathrm{F} r \cos \theta}{\mathrm{P}},$$

where F is one of the applied forces, r the distance of its point of application, R, from an arbitrary fixed point O,

2 T 2

θ the angle between RO and the direction of F, and P is the allowable stress in the material.

A more general class of frames satisfying the condition $e = \epsilon$, consists of those whose bars, both before and after the appropriate deformation, form curves of orthogonal systems.

In such a system the test strains may be equal and of the same sign, or equal and opposite, in directions at right angles, without the strains in the bars being exceeded by those of any other lines in the field. In the first case the strain is evidently the same in all directions; in the second, if λ, $-\lambda$ are the strains in the principal directions, the strain in any direction at an angle θ to one of them is $\pm\lambda \cos 2\theta = \lambda'$,

$$\therefore \ [\lambda'] \not> [\lambda].$$

The condition that a two-dimensional orthogonal system shall remain orthogonal after equal and opposite extensions of its two series of curves, is that the inclination between any two adjacent curves of the same series is constant throughout their length. This is easily seen as follows:—

Let l be the elementary length, ϕ the change of direction of the curve S_1, between its points of intersection with two
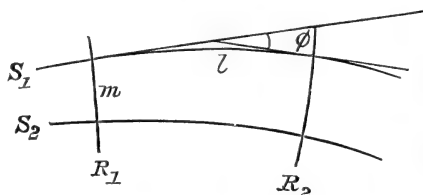
adjacent curves R_1R_2 of the other series, and let m be the elementary length of R_1 between the two curves S_1, S_2 of the S-series, m being taken small in comparison with l.

Let the curves of the S-series be extended by the small fraction λ of their length.

If the element remains rectangular the change in the angle ϕ must be equal to the change in the inclination of the elements R_1 and R_2 to each other,

$$\therefore \ \delta\phi = \frac{\lambda l - \lambda(l + \frac{dl}{dm} \cdot m)}{m} = -\lambda \frac{dl}{dm} = \lambda\phi.$$

Take now a new element, with sides of comparable length, l, n, formed by the pairs of curves R_1R_2, S_1S_k. Let ψ be

the change of direction of R_1 between S_1 and S_k, and let λ' be the extension of the R curves.
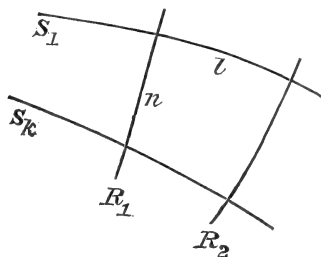
Then, as before, $\delta\psi = \lambda'\psi$; and making a circuit around the element, since the angles remain right angles,

$$\lambda \cdot \frac{d\phi}{dn} n - \lambda' \cdot \frac{d\psi}{dl} \cdot l = 0,$$

but $\dfrac{d\phi}{dn} \cdot n - \dfrac{d\psi}{dl} \cdot l = 0$, because the angles were right angles before the deformation.   Thus since $\lambda' \neq \lambda$, by assumption

$$\frac{d\phi}{dn} = 0, \quad \text{and} \quad \frac{d\psi}{dl} = 0. \qquad \text{Q.E.D.}$$

There are two general classes of orthogonal curves satisfying the required conditions, viz. :—

I. Systems of tangents and involutes derived from any evolute curves.

(Since such systems are bounded by the evolute curves, the corresponding frames are in general of minimum volume only relatively to others with the same finite boundaries.)

II. Orthogonal systems of equiangular spirals, with systems of concentric circles and their radii, and rectangular networks of straight lines, as special cases.

Frames whose bars coincide with the curves of any of these systems are therefore frames of minimum quantity of material, for any system of forces consistent with their equilibrium and continuity of displacement.

Frames may also be built up of parts of different systems of these classes provided that continuity of displacement is secured along the lines of junction.

Examples of such frames of minimum quantity of material, for some elementary systems of forces, are given in the accompanying figures.

Compression bars in each case are indicated by thick, and tension bars by fine, continuous lines.   Those portions of the

lines of principal strain on which material bars are not required are shown by dotted lines.

1. (Fig. 1.) A single force F applied at A, and acting at right angles to the line AB, is balanced by an equal and opposite force and a couple, of moment F × AB, applied at B.

Fig. 1.
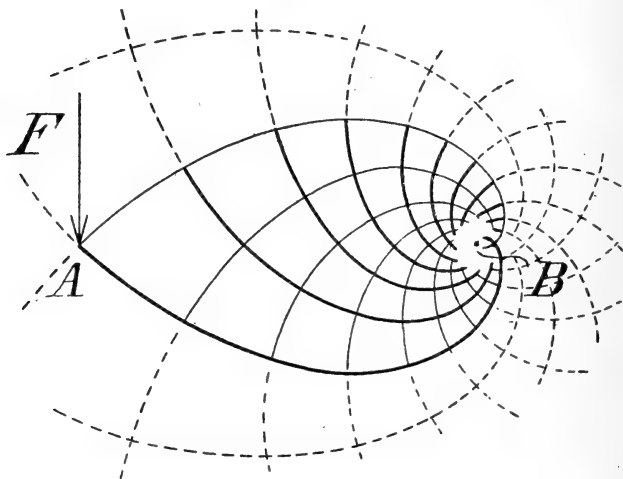
The minimum frame is formed of two similar equiangular spirals having their origin at B and intersecting orthogonally at A, together with all the other spirals orthogonal to these and enclosed between them.

All the spirals of one series are equally extended, those of the other system equally compressed to the same degree. The spirals may be extended to infinity, and the infinite field will remain continuous with equal and opposite strains in the principal directions at all points. The necessary volume of material is infinite if the frame is continued to a mere point at B; but if forces equivalent to the force F, and the couple F × AB at B, are distributed over a small circle with its centre at B, and of radius r_0, the volume necessary is

$$\mathrm{F}a \cdot \log \frac{a}{r_0} \cdot \left(\frac{1}{\mathrm{P}} + \frac{1}{\mathrm{Q}} \right),$$

where $a = \mathrm{AB}$, and P, Q are the stresses allowed for tension and compression.

2. *Centrally-loaded Beam.*—(Fig. 2.) A single force, F, is applied at C, the middle point of the line AB, and is balanced by equal parallel forces at A and B.

The minimum frame is composed of the two quadrantal bars DF and EG, having their common centre at C, of all

Fig. 2.
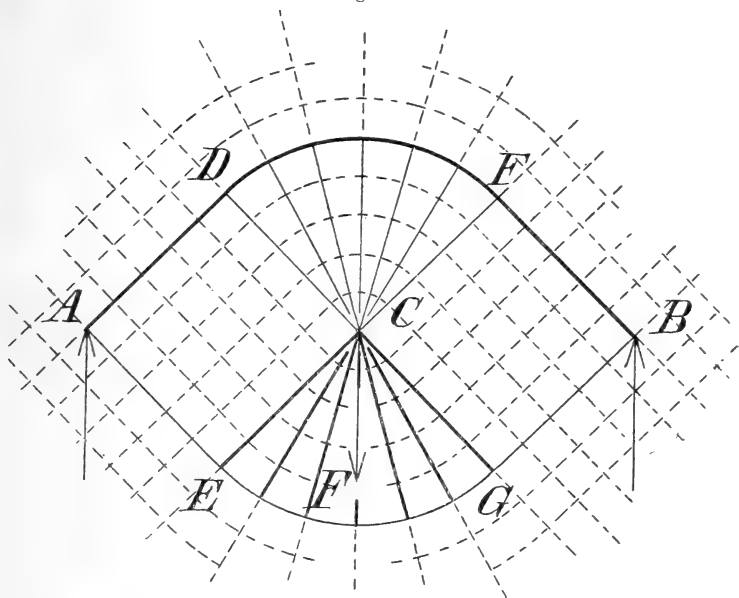
the radii of these quadrants, and their four tangents AD, AE, BF, and BG. The orthogonal system is completed and extended to infinity as indicated by the dotted lines.

The quadrants above C and all straight lines extending obliquely downwards and outwards from the line of action of F are compressed, all the other lines extended.

The necessary volume of material is

$$F a\left(\frac{1}{2}+\frac{\pi}{4}\right)\left(\frac{1}{P}+\frac{1}{Q}\right),$$

where $a = AC = CB$.

2*a*. (Fig. 3.) With the same applied forces the condition may be imposed that the frame shall lie wholly on one side of the line AB.

The form of the minimum frame is then as in fig. 3, and consists of the semicircle on AB and its radii. The figure to be subjected to the test deformation consists of the semi-infinite plane below AB, all the radii from C being extended and the semicircles about C compressed.

The minimum volume of the frame is

$$\mathrm{F}a \cdot \frac{\pi}{2}\Big(\frac{1}{\mathrm{P}} + \frac{1}{\mathrm{Q}}\Big),$$

and therefore exceeds the frame of fig. 2, in which no limitation of extent was imposed, in the ratio $2\pi : (2+\pi)$, or $1\cdot22 : 1$.

Fig. 3.
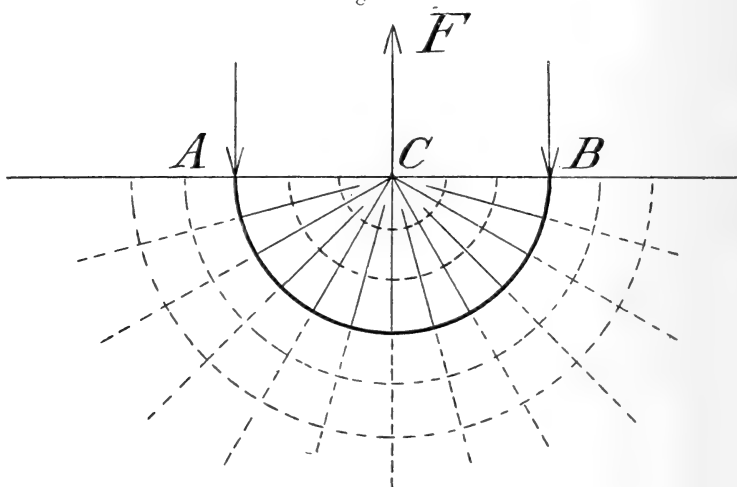
Fig. 4.
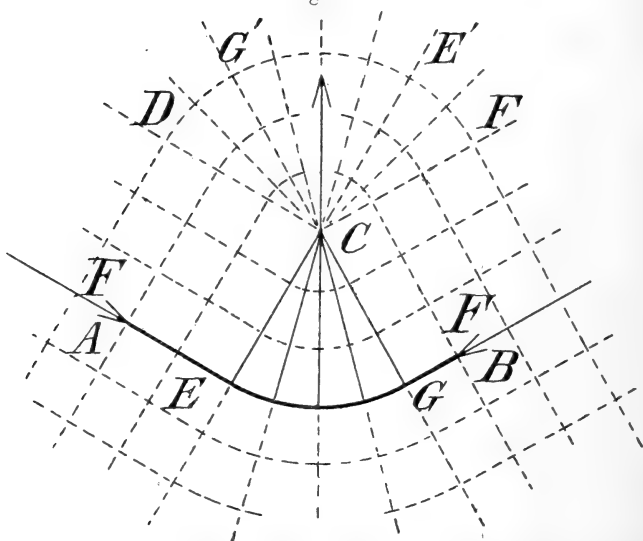
3. (Fig. 4.) Constructions similar to that of figs. 2 & 3 give the minimum frames for three forces, two of which are

equal and directed to a point on the line of action of the third outside the triangle formed by the points of application of the forces. Fig. 4 is an example.

The sectors DCG′, FCE′ undergo uniform bulk compression, the remainder of the field pure shear as in fig. 2.

The volume of the frame is

$$\mathrm{F} \times \left\{ \mathrm{CE} \times \alpha \times \left(\frac{1}{\mathrm{P}} + \frac{1}{\mathrm{Q}}\right) + (\mathrm{AE} + \mathrm{BG}) \times \frac{1}{\mathrm{Q}} \right\},$$

where α is the angle ECG.

4. (Fig. 5.) Equal and opposite couples applied at points A, B on the straight line AB.

Fig. 5.
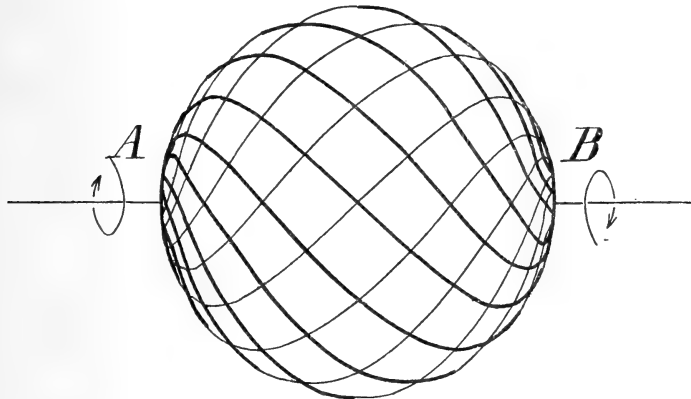
The minimum frame consists of the series of rhumb-lines inclined at 45° to the meridians of the sphere having its poles at A and B.

All the rhumb-lines of one series and all similar lines on spheres concentric with the spherical frame are uniformly and equally extended, all the lines orthogonal to these on the spheres equally compressed.

The third system of orthogonals, viz., radii from the centre of the sphere, are unchanged in length and simply rotated about the axis AB.

The minimum volume of the frame is

$$\frac{2\mathrm{L}}{a} \log \tan\left(\frac{\pi}{4} + \frac{\lambda}{2}\right) \times \left(\frac{1}{\mathrm{P}} + \frac{1}{\mathrm{Q}}\right),$$

where L is the moment of the transmitted couple, $2a = \mathrm{AB}$, and λ is the latitude of small circles about each pole corresponding to the circle of radius r_0 in Case 1.

Melbourne, April 19, 1904.

LIX. *Theory of the Rainbow due to a Circular Source of Light.* By K. Aichi, *Rigakushi, and* T. Tanakadate, *Rigakushi, Imperial University, Tokyo* *.

1. **A**IRY † was the first to establish a satisfactory theory of the rainbow on the undulatory theory of light, based on the assumption of a point source of light. Airy, however, assumed that the equation of the meridian section of the emergent wave-surface referred to the tangent, and the normal to the curve at the point of inflexion is $y = \dfrac{-x^3}{3a}$,

a being an undetermined constant. This point in Airy's theory was afterwards developed in detail by Boitel ‡, Larmor §, and especially by Mascart ‖ and L. Lorenz ¶. Recently, the colours of the rainbow were minutely investigated by Pernter **, by using Maxwell's theory of compound colours. In this paper Pernter added a short calculation on the colours due to a source of finite dimension, by mere numerical addition of the results for the case of a point source; and he considered this to be sufficient to represent the colours due to the sun. It is to be remarked that his values †† of Airy's integral $f^2(z)$, on which the whole calculation is based, are sometimes discrepant from those originally given by Airy for $z < 4$; and for $z > 4$ we could not arrive at his results after repeated calculations. So far as we are aware, the various calculations are as yet limited only to cases which, strictly speaking, hold only for a point source of light. These considerations led us to undertake the following investigation. It may, therefore, be looked upon as an extension of Airy's theory, when the apparent diameter of the sun is taken into account.

Experiment must go hand in hand with theory. Miller ‡‡ and Pulfrich §§ verified Airy's theory in the special case of two dimensions with a cylindrical stream of water (or glass rod), and a straight slit as the source of light. But a question

suggests itself in conjunction with the problem of the circular source of light—if we take account of the breadth of the slit, assuming its length to be infinite, what difference will occur? This must be answered.   In the following, we shall start by briefly stating Airy's theory, then proceed to find differences when the source of light is replaced by a small circular disk ; and after some additional note for the two-dimensional case, experimental results will be discussed ; and lastly the colours of the rainbow due to the sun are calculated in one case, which may be taken as an illustration of the difference between the point and the circular source.

2. It will be necessary, in the first place, to state Airy's theory in a form convenient for subsequent investigation. Describe a unit sphere having the centre C coinciding with that of a raindrop, and let a point O on the sphere be the place of observation, and S the direction of the sun supposed to be a point as seen from C.   The position of the observer with respect to the sun is specified by the angle SCO, or by the angle θ, which is equal to the angle of minimum deviation D *minus* SCO.

Put $\qquad$ $r=$ radius of the drop, $\qquad$ $n=$ index of refraction,

$p-1=$ number of internal reflexions,

and $\qquad$ $h = \dfrac{(p^2-1)^2}{p^2(n^2-1)} \cdot \sqrt{\dfrac{p^2-n^2}{n^2-1}} ;$

then the intensity of light at O is given by

$$\iota(\theta) = \text{const.} \left(\frac{r^7}{h^2 \lambda \cos^2 \theta} \right)^{\frac{1}{3}} f^2(\mathrm{K}\theta),$$

where

$$\mathrm{K} = 2 \left(\frac{6}{h} \right)^{\frac{1}{3}} \left(\frac{r}{\lambda} \right)^{\frac{2}{3}},$$

$$f(\mathrm{K}\theta) = \int_0^\infty \cos \frac{\pi}{2} (u^3 - \mathrm{K}\theta u) du.$$

Airy expanded $f(\mathrm{K}\theta)$ as a power series of $\mathrm{K}\theta$, which is not convenient for practical calculation for values of $\mathrm{K}\theta > 3$, though it always remains convergent.   On the other hand, especially for larger values of θ, the following semiconvergent series due to Stokes * can be employed with advantage :—

* Collected Papers, ii. p. 329 (London, 1883).

$$f(\mathrm{K}\theta) = 2^{\frac{1}{2}} 3^{-\frac{1}{4}} (\mathrm{K}\theta)^{-\frac{1}{4}} \mathrm{M} \cos\left(\mu - \frac{\pi}{4} - \delta\right),$$

where

$$\mu = \pi \left(\frac{\mathrm{K}\theta}{3}\right)^{\frac{3}{2}},$$

$$\mathrm{M} = 1 - 0 \cdot 0347 \mu^{-2}, \quad \tan \delta = 0 \cdot 0694 \mu^{-1};$$

or, approximately,

$$f(\mathrm{K}\theta) = 2^{\frac{1}{2}} 3^{-\frac{1}{4}} (\mathrm{K}\theta)^{-\frac{1}{4}} \cos{}_{\pi} \left\{\left(\frac{\mathrm{K}\theta}{3}\right)^{\frac{3}{2}} - \frac{1}{4}\right\}.$$

It must be remarked that both expansions only represent $f(\mathrm{K}\theta)$ for $\theta > 0$. But for $\theta < 0$, $f(\mathrm{K}\theta)$ being a function having no characteristic property, it is at once seen that no important difference between a point and a circular source makes its appearance. In the following discussion the places where $\theta < 0$ are therefore excluded from consideration.

3. Passing now to the case of a circular source, the apparent diameter of the source 2Φ will be supposed so small that we can neglect Φ^2, and confine our attention to the neighbourhood of the minimum deviation, so that $\theta^2 = 0$, $\cos \theta = 1$. Take the elementary area of the projection S′ of the source on the sphere, S being that of the centre of the circular disk, and denote the angle between S′S and SO by ψ, the angular distance between S′ and S by ϕ, and the angle S′CO by $\mathrm{D} - x$. Then we have the relation

$$\cos (\mathrm{D} - x) = \cos \phi \cos (\mathrm{D} - \theta) + \sin \phi \sin (\mathrm{D} - \theta) \cos \psi,$$

which reduces to the form

$$x = \theta + \phi \cos \psi,$$

since x, θ, ϕ are very small.

The intensity of light at O due to the elementary area S′ which is equal to $\phi d\phi d\psi$, is expressed by

$$i_{\phi d\phi d\psi}(\theta) = \text{const.}\left(\frac{r^7}{h^2\lambda}\right)^{\frac{1}{3}} f^2(\mathrm{K}x) = \text{const.}\left(\frac{r^7}{h^2\lambda}\right)^{\frac{1}{3}} f^2\{\mathrm{K}(\theta + \phi \cos \psi)\};$$

from which it follows at once as the expression for the total intensity at O,

$$\mathrm{I}(\theta, \Phi) = \text{const.}\left(\frac{r^7}{h^2\lambda}\right)^{\frac{1}{3}} \mathrm{F}(\mathrm{K}, \Phi, \mathrm{K}\theta),$$

where

$$F(K, \Phi, K\theta) = \frac{1}{\pi\Phi^2}\int_0^\Phi\int_0^{2\pi}\{\phi d\phi d\psi f^2\{K(\theta + \phi\cos\psi)\},$$

$$f^2\{K(\theta + \phi\cos\psi)\} = \int_0^\infty \cos\frac{\pi}{2}\{u^3 - K(\theta + \phi\cos\psi)u\}du.$$

Thus the function f^2 in Airy's theory is replaced by a more general function F. From the form of the functions f^2 and F, the differences between a point and a circular source are to be found. $f^2(K\theta)$ does not change for different values of K, if we reduce the scale of θ properly, because it is a function of $K\theta$, but not a function of K and θ separately; this, however, does not hold for F, which is a function of K as well as of $K\theta$. Thus it is necessary to consider F more in detail, though its evaluation as a function of K, Φ, and $K\theta$ is by no means easy.

If we try to expand $f^2\{K(\theta + \phi\cos\psi)\}$ in a power series of $K\phi\cos\psi$, then its coefficients gradually increase with $K\theta$, and are very inconvenient for values of $K\theta > 1$. If we change $f^2\{K(\theta + \phi\cos\psi)\}$ to a double integral

$$\int_0^\infty \cos\frac{\pi}{2}\{x^3 - K(\theta + \phi\cos\psi)x\}dx\int_0^\infty\cos\frac{\pi}{2}\{y^3 - K(\theta + \phi\cos\psi)y\}dy$$

$$= \frac{1}{2}\int_0^\infty dx\int_0^\infty dy\left[\cos\frac{\pi}{2}\{x^3 + y^3 - K\phi(x+y)\}\cos\frac{\pi}{2}\{K\phi\cos\psi(x+y)\}\right.$$

$$-\sin\frac{\pi}{2}\{x^3 + y^3 - K\phi(x+y)\}\sin\frac{\pi}{2}\{K\phi\cos\psi(x+y)\}$$

$$+\cos\frac{\pi}{2}\{x^3 - y^3 - K\phi(x-y)\}\cos\frac{\pi}{2}\{K\phi\cos\psi(x-y)\}$$

$$\left.-\sin\frac{\pi}{2}\{x^3 - y^3 - K\phi(x-y)\}\sin\frac{\pi}{2}\{K\phi\cos\psi(x-y)\}\right],$$

and integrate with respect to $\phi\cos\psi$, then the final form is

$$\int_0^\infty dx\int_0^\infty dy\left[\cos\frac{\pi}{2}\{x^3 + y^3 - K\Phi(x+y)\}\frac{J_1\left\{\frac{\pi}{2}K\Phi(x+y)\right\}}{\frac{\pi}{2}K\Phi(x+y)}\right.$$

$$\left.+\cos\frac{\pi}{2}\{x^3 - y^3 - K\Phi(x-y)\}\frac{J_1\left\{\frac{\pi}{2}K\Phi(x-y)\right\}}{\frac{\pi}{2}K\Phi(x-y)}\right].$$

Putting

$$x = x'\left(\frac{2}{\pi}\right)^{\frac{1}{3}}, \quad y = y'\left(\frac{2}{\pi}\right)^{\frac{1}{3}}, \quad K = K'\left(\frac{2}{\pi}\right)^{\frac{1}{3}},$$

and $y' = -y'$ in the latter half,

then applying the well-known sequence equation of the Bessel's functions

$$\frac{J_1(w)}{w} = \tfrac{1}{2}\{J_2(w) + J_0(w)\},$$

this can be reduced to the form

$$\int_0^\infty \int_{-\infty}^\infty dx'dy' \cos\{x'^3 + y'^3 - K'\theta(x' + y')\}[J_2\{K'(x' + y')\} + J_0\{K'(x' + y')\}],$$

which is almost intractable for practical calculation.

4. It will be advantageous to consider first the maxima and minima of F as compared with those of f^2, and then to discuss the general character, and finally proceed to the numerical calculation of F. For this purpose, transform the variables ϕ, ψ to x, y, which are given by $x = \phi \cos \psi$, $y = \phi \sin \psi$, and then integrate with respect to y. Putting $Kx = z$, we arrive at the expression

$$F(K, \Phi, K\theta) = \frac{2}{K\Phi^2 K^2}\int_{-K\Phi}^{+K\Phi} dz\sqrt{(K\phi)^2 - z^2} f^2(K\theta + z).$$

The maxima and minima of F are given by

$$\int_{-K\Phi}^{+K\Phi} dz\sqrt{(K\phi)^2 - z^2}\frac{\partial}{\partial\theta}\{f^2(K\theta + z)\} = 0;$$

i. e. $$\int_{-K\Phi}^{+K\Phi} dz\sqrt{(K\phi)^2 - z^2}\frac{\partial}{\partial z}\{f^2(K\theta + z)\} = 0.$$

Or, putting the mean value of $\sqrt{(K\phi)^2 - z^2}$ in the integral, we find the approximate relation

$$f^2(K\theta - K\Phi) = f^2(K\theta + K\Phi).$$

For the smaller values of θ, especially at the first maximum, f^2 has no symmetry on both sides of the maxima and minima ; so that the first maximum of F receives small displacement towards $\theta = 0$ as compared with f^2. This displacement becomes smaller and smaller for other maxima and minima. For larger values of θ, as f^2 is nearly symmetrical on both sides, the maxima and minima approximately coincide with those of f^2; nevertheless it does not follow that the maxima for f^2 always remain as maxima for F.

5. For the consideration of the general character of F, we shall begin with that of f^2. By Stokes's expression

$$f^2(\mathrm{K}\theta) = \frac{2}{\sqrt{3}} \frac{1}{\sqrt{\mathrm{K}\theta}} \cos^2 \pi \left\{ \left(\frac{\mathrm{K}\theta}{3}\right)^{\frac{3}{2}} - \frac{1}{4} \right\} = \frac{1}{\sqrt{3}\sqrt{\mathrm{K}\theta}} - \frac{\sin 2\pi \left(\dfrac{\mathrm{K}\theta}{3}\right)^{\frac{3}{2}}}{\sqrt{3}\sqrt{\mathrm{K}\theta}},$$

f^2 is composed of two terms: the mean term $\dfrac{1}{\sqrt{3}\sqrt{\mathrm{K}\theta}}$, and the oscillating term, whose amplitude is limited by the same numerical factor $\dfrac{1}{\sqrt{3}\sqrt{\mathrm{K}\theta}}$; therefore at maxima f^2 increases to $2 \times$ (mean term) and at minima decreases to zero. But the character of F is slightly different, since

$$\mathrm{F}(\mathrm{K}, \Phi, \mathrm{K}\theta) = \frac{2}{\pi\Phi^2\mathrm{K}^2} \int_{-\mathrm{K}\Phi}^{+\mathrm{K}\Phi} dz\sqrt{(\mathrm{K}\phi)^2 - z^2} \left[\frac{1}{\sqrt{3}\sqrt{\mathrm{K}\theta+z}} - \frac{\sin 2\pi\left(\dfrac{\mathrm{K}\theta+z}{3}\right)^{\frac{3}{2}}}{\sqrt{3}\sqrt{\mathrm{K}\theta+z}} \right].$$

The first term is equal to

$$\frac{2}{\pi\Phi^2\mathrm{K}^2} \int_{-\mathrm{K}\Phi}^{+\mathrm{K}\Phi} dz\sqrt{(\mathrm{K}\phi)^2 - z^2} \frac{1}{\sqrt{3}\sqrt{\mathrm{K}\theta}}\left(1 - \frac{z}{2\mathrm{K}\theta} + \dots\right),$$

and leads to the same mean term so far as the first order of $\dfrac{z}{\mathrm{K}\theta}$ is concerned. But the second term, after putting $\dfrac{\pi\mathrm{K}\Phi}{4} \dfrac{1}{\mathrm{K}\theta}$ as the mean value of $\dfrac{\sqrt{(\mathrm{K}\phi)^2 - z^2}}{\mathrm{K}\theta + z}$, and integrating with respect to a new variable $2\pi\left(\dfrac{\mathrm{K}\theta+z}{3}\right)^{\frac{3}{2}}$, becomes

$$\frac{\cos 2\pi\left(\dfrac{\mathrm{K}\theta - \mathrm{K}\Phi}{3}\right)^{\frac{3}{2}} - \cos 2\pi\left(\dfrac{\mathrm{K}\theta + \mathrm{K}\Phi}{3}\right)^{\frac{3}{2}}}{6\pi\mathrm{K}\Phi\mathrm{K}\theta} = \frac{\sin 2\pi\left(\dfrac{\mathrm{K}\theta}{3}\right)^{\frac{3}{2}}\sin\left(\sqrt{3}\pi\mathrm{K}\Phi\sqrt{\mathrm{K}\theta}\right)}{3\pi\mathrm{K}\Phi\mathrm{K}\theta}.$$

Thus the amplitude of the oscillating term is limited by $\dfrac{\sin\left(\sqrt{3}\pi\mathrm{K}\Phi\sqrt{\mathrm{K}\theta}\right)}{3\pi\mathrm{K}\Phi\mathrm{K}\theta}$ much smaller than $\dfrac{1}{\sqrt{3}\sqrt{\mathrm{K}\theta}}$; and, moreover, this may be positive or negative according to the sign of $\sin\left(\sqrt{3}\pi\mathrm{K}\Phi\sqrt{\mathrm{K}\theta}\right)$. For the smaller value of Φ, $\dfrac{1}{3\pi\mathrm{K}\Phi\mathrm{K}\theta}$ gradually increases and the period of $\sin\{\sqrt{3}\pi\mathrm{K}\Phi\sqrt{\mathrm{K}\theta}\}$ prolongs; in the limiting case $\Phi=0$, $\dfrac{\sin\{\sqrt{3}\pi\mathrm{K}\Phi\sqrt{\mathrm{K}\theta}\}}{3\pi\mathrm{K}\Phi\mathrm{K}\theta}$ becomes equal to $\dfrac{1}{\sqrt{3}\sqrt{\mathrm{K}\theta}}$, as

expected. Also for the smaller value of K, *i. e.* of *r*, the same reasoning will hold true.

From this approximate expression,

$$F \doteq \frac{1}{\sqrt{3}\sqrt{K\theta}} - \frac{\sin 2\pi \left(\dfrac{K\theta}{3}\right)^{\frac{3}{2}} \sin\{\sqrt{3}\pi K\Phi\sqrt{K\theta}\}}{3\pi K\Phi K\theta},$$

it follows that F does not increase at maxima to $2 \times$ (mean term), but only to $\left(\text{mean term} + \dfrac{1}{3\pi K\Phi K\theta}\right)$; and at minima it does not diminish to zero, but only to $\left(\text{mean term} - \dfrac{1}{3\pi K\Phi K\theta}\right)$; moreover, for values of θ for which $\sin\{\sqrt{3}\pi K\Phi\sqrt{K\theta}\} < 0$, the maxima of $\sin 2\pi\left(\dfrac{K\theta}{3}\right)^{\frac{3}{2}}$ changes to minima and minima to maxima. Finally, the expression for the intensity being

$$i(\theta) = \text{const.} \left(\frac{r^7}{h^2\lambda}\right)^{\frac{1}{3}} f^2(K\theta) \quad . \quad . \text{ for point source,}$$

$$I(\theta, \Phi) = \text{const.} \left(\frac{r^7}{h^2\lambda}\right)^{\frac{1}{3}} F(K, \Phi, K\theta) \text{ for circular source;}$$

it follows, firstly, that for larger value of Φ, the difference of $i(\theta)$ and $I(\theta, \Phi)$ becomes larger; secondly, that for larger value of r, the difference of maximum and minimum values of $i(\theta)$ becomes larger in virtue of $r^{\frac{7}{3}}$, but for $I(\theta, \Phi)$ at the same time it is diminished by the presence of F.

6. We shall now treat the case which has often been tested by experiment with the glass rod, and straight slit as the source of light. If we take into account the breadth of the slit, there is no difficulty in applying the similar reasoning as above, to arrive at the expression

$$I(\theta, \Phi) = \text{const.} \left(\frac{r^7}{h^2\lambda}\right)^{\frac{1}{3}} F(K, \Phi, K\theta),$$

$$F(K, \Phi, K\theta) = \int_{-K\Phi}^{+K\Phi} f^2(K\theta + z)dz,$$

where $2\Phi =$ the breadth of the slit as viewed at the place of the glass rod.

In § 5 we always substituted the mean value of $\sqrt{(K\phi)^2 - z^2}$ before integration, so that the expression for F becomes only roughly approximate; but in the present case, there being no such term as $\sqrt{(K\phi)^2 - z^2}$, this expression for F must be

taken as nearly true. From this it follows that the effect of the breadth of the slit is more manifestly shown than for that of a circular source. Especially, the maxima and minima not only interchange places at certain points, but the interval between the maxima and minima slightly increases, as a consequence that the first maximum displaces towards $\theta = 0$, while the higher maxima, showing the same tendency, displace by smaller amount. In Pulfrich's experiment[*], where he takes third maximum as standard, the first and second displace slightly towards $\theta = 0$, and the other to the opposite side, as compared with Airy's values of f^2. This displacement may be due to the breadth of the slit.

7. For numerical calculation we must have recourse to mechanical quadrature. The method of procedure is as follows :—Draw a circle having the centre at $K\theta$ and radius equal to $K\Phi$, then plot a curve whose ordinate is equal to the product of the value of f^2 into the corresponding ordinate of this circle ; the area of the new curve divided by the area of this circle is the value of F at $K\theta$. The following table was constructed by this method.

$2\Phi =$ apparent diameter of the sun $= 32'$.

$K\theta$: (except 0) max. and min. of Airy's value.

$F_1 : r = 0.025$ cm. $\lambda = 5893 \times 10^{-8}$ cm. $K = 120.95$.

$F_2 : r = 0.05$ cm. $\lambda = 5893 \times 10^{-8}$ cm. $K = 192.00$.

$K\theta$.	f^2.	F_1.	F_2.
0	0·443	0·447	0·465
1·084	1·008	0·930	0·840
2·495	0·000	0·103	0·228
3·467	0·617	0·497	0·362
4·363	0·000	0·124	0·237
5·145	0·510	0·378	0·260
5·892	0·000	0·133	0·235
6·578	0·450	0·300	0·206
7·244	0·000	0·140	0·223
7·868	0·404	0·254	0·177
8·479	0·000	0·145	0·208
9·060	0·384	0·230	0·164
9·630	0·000	0·148	0·199
10·177	0·362	0·207	0·155
10·716	0·000	0·150	0·195

The graphical representation of this table is given at the end of the paper, where f^2 is represented by the broken line and $F_1 F_2$ in full lines (fig. 1, p. 609).

* Wied. *Ann.* xxxiii. pp. 205, 206 (1888).

8. Pernter calculated the colours of the rainbow due to the sun, but his calculation was not sufficient to establish the above-mentioned results. We repeated the calculation for one case. The theory of compound colours being the subject of much dispute, there is as yet no settled opinion. But we can admit that, excluding the physiological and psychological point of view, there are three primary colours, as Maxwell's experiment * shows. For the discussion of the colours of the rainbow, we may conveniently take only these three primary colours, in such a ratio as to produce white, and proceed in the manner indicated by Maxwell.

We take the primary colours

$\lambda = 6302 \times 10^{-8}$ cm.(Scarlet) 5211×10^{-8} cm.(Green) 4659×10^{-8} cm.(Blue)

corresponding to Maxwell's

$$[24] \qquad\qquad [46] \qquad\qquad [64]$$

in the ratio $1 : 1\cdot62 : 1\cdot60$, so as to produce white,

and	$n = 1\cdot332$	$1\cdot335$	$1\cdot339,$
whence	$K = 115\cdot30$	$129\cdot16$	$142\cdot70$
	$D = 42°\cdot22$	$41°\cdot80$	$41°\cdot21$

Thus we obtain the result represented by fig. 2 and fig. 3 (pp. 609, 610), the former corresponding to the case of the point source, and the latter to the circular source ($2\Phi = 32'$ mean angular diameter of the sun), r in both cases being $0\cdot025$ cm. The intensity of the scarlet ray is given by dotted, green by broken, blue by full lines, and the sum of the three intensities, *i. e.* the total intensity, by the curve (1), which is compounded by the portion of white and the portion of the two primary colours.

For example, in fig. 2 :—

I at $41°\cdot5$ consists of 26 per cent. scarlet, 50 per cent. green, 24 per cent. white,
   $40°\cdot5$,,   14   ,,   blue, 16   ,,   scarlet, 70   ,,   white,
   $38°$,,   26   ,,   green, 74   ,,   blue, 0   ,,   white.

In fig. 3:—

I at $41°\cdot5$ consists of 27 per cent. scarlet, 47 per cent. green, 26 per cent. white,
   $40°\cdot5$,,   25   ,,   blue, 10   ,,   scarlet, 65   ,,   white,
   $38°$,,   10   ,,   green, 22   .,   blue, 68   ,,   white,

where the angles correspond to $SCO = D - \theta$ in § 2.

The above calculation shows that in the colours of the

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* Scientific Papers, i. p. 410 (Cambridge, 1890).

supernumerary bows due to the sun, white colour predominates, and we cannot distinguish many numbers of the supernumerary bows. This explains the fact that the rainbow in nature is accompanied by a small number of supernumerary bows only, while according to Airy's theory the rainbow ought to be accompanied by numerous bows.

According to § 5, we notice that the difference between the maximum and minimum values of intensity becomes larger with the size of the drop for a point source ; but for a circular source the intensity depends on two factors, one of which enjoys the same property as for a point source, but the other produces a contrary effect. Montigny * says that supernumerary bows are numerous when the drops are small. This holds for the case of a circular source and supports our view, but he considers this as the result of Airy's theory, *i. e.* of a point source, which we cannot understand.

9. As the consequence of the above discussion, we obtain the following result, where (I) represents the case of a point source, (II) a circular source:—

(*a*) The positions of the maxima and minima of (II) approximately coincide with that of (I) (the first maximum of (II) is displaced by a small amount towards $\theta = 0$ as compared with (I), and for other maxima and minima this displacement becomes smaller and smaller). But the maxima of (II) may correspond to the minima of (I), and the minima to the maxima.

(*b*) The value of (II), which corresponds to the maximum of (I), is smaller than that of (I), and the minimum value of (II) is greater than that of (I). This difference between (I) and (II) becomes larger with the larger value of Φ (*i. e.* with the larger diameter of the source).

(*c*) As the value of θ increases, the maximum value of (I) and (II) gradually decreases, while the minimum value of (I) always remains 0 ; but the minimum value of (II) gradually increases until it becomes equal to the maximum value and assumes a stationary value, then the maxima and minima interchange, the difference at first increases and then decreases, then again assumes a stationary value, and so on. If in this interval between the two stationary values the maxima of (II) correspond to that of (I), then in the next interval the maxima of (II) correspond to the minima of (I).

(*d*) For larger values of *r* (radius of the drop) the intensity

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* Phil. Mag. ix. p. 389 (1880).

of (I) and (II) increases by $r^{\frac{7}{3}}$. But at the same time for (II), the difference between the maximum and the minimum values is diminished by another factor F.

(*e*) The above is more manifestly shown in the case of the laboratory experiment with a cylindrical glass rod and a straight slit as a source of light. The stationary points of (II) at which the maximum value coincides with the minimum are easily found by

$$K\Phi = m \times \text{interval of the maximum and minimum of (I)},$$

where *m* represents an integer.

(*f*) According to Airy's theory, the law of distribution of the colours of the rainbow is independent of the magnitude of the drop. But in the case of the finite source, the colour distributions are changed by the magnitude of the drop, especially in the supernumerary bows.

(*g*) The supernumerary bows almost lose the colour as the consequence of the finiteness of the source. This effect is more remarkable when the drop becomes larger.

10. To show the above-mentioned results, we repeated rough experiments with glass rods and a straight slit as a source of light.

Using homogeneous light, we see that when the slit is very narrow Airy's theory holds good, and that when the breadth of the slit is increased, the positions of bows (or fringes in this case) change very little. As another effect of the increase of the breadth of the slit, the bows become indistinct, especially in the supernumerary bows. This effect is remarkable when the diameter of the rod is large. We could not observe the turning point, at which the maxima and minima interchange, as the difference of the intensities is very small. But we can roughly say that the point at which the bows become almost indistinguishable corresponds to the position at which the breadth of the bow coincides with that of the slit.

Again, using white light, it is easy to see that the colours of the supernumerary bows change when the magnitude of the rod is changed, and that the supernumerary bows almost lose the colour and become indistinct when the breadth of the slit is increased.

Finally, we have to thank Prof. Nagaoka for suggesting the problem and giving kind advice during the course of the investigation.

Fig. 1.—Intensity Curve.
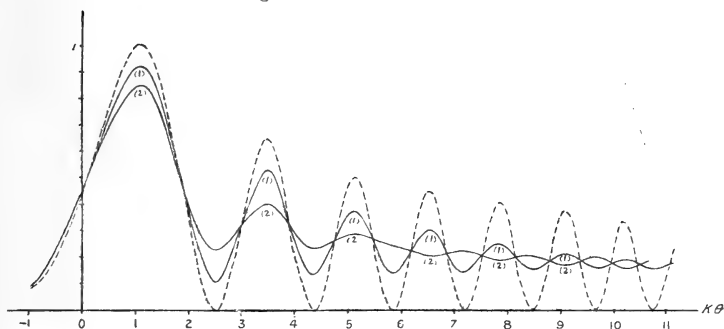
Dotted curve represents Airy's value of f^2.

Curve (1)　　　,,　　F_1 for circular source $2\Phi=32'$, $\lambda=5893\times10^{-8}$ cm., $r=0.025$ cm.

Curve (2)　　　,,　　F_2　　,,　　,,　　,,　　,,　　　$r=0.05$ cm.

Fig. 2.—Intensity Curves for Primary Colours (Point Source.)
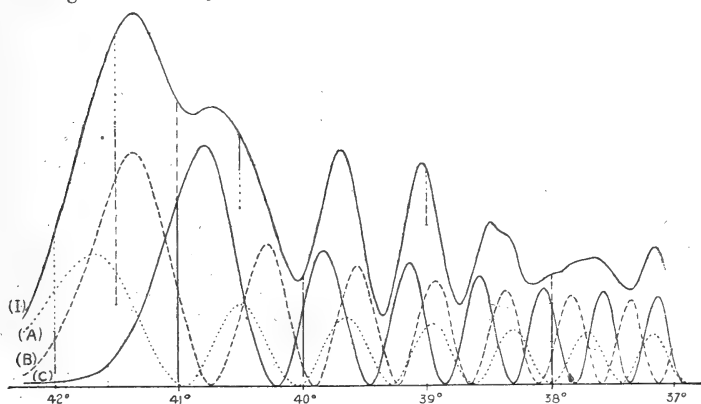
.... curve (A) represents Airy's value of $1.00f^2\left(\dfrac{r^7}{\lambda}\right)^{\frac{1}{3}}$ for $\lambda=6302\times10^{-8}$ cm.

--------- curve (B)　　,,　　　,,　　　,,　　$1.62f^2\left(\dfrac{r^7}{\lambda}\right)^{\frac{1}{3}}$,, $\lambda=5211$　,,

_____ curve (C)　　,,　　　,,　　　,,　　$1.60f^2\left(\dfrac{r^7}{\lambda}\right)^{\frac{1}{3}}$,, $\lambda=4659$　,,

_____ curve (I)　　,,　　total intensity.

Fig. 3.—Intensity Curves for Primary Colours (Circular Source $2\Phi=32'$).

.... curve (A) represents value of $1\cdot00\mathrm{F}\left(\dfrac{r7}{\lambda}\right)^{\frac{1}{3}}$ for $\lambda=6302\times10^{-8}$ cm. $r=0\cdot025$ cm.

- - - - - - - curve (B)   ,,   ,, $1\cdot62\mathrm{F}\left(\dfrac{r7}{\lambda}\right)^{\frac{1}{3}}$,, $\lambda=5211$,,   ,,

———— curve (C)   ,,   ,, $1\cdot60\mathrm{F}\left(\dfrac{r7}{\lambda}\right)^{\frac{1}{3}}$,, $\lambda=4659$,,   ,,

———— curve (I)   ,,   total intensity.
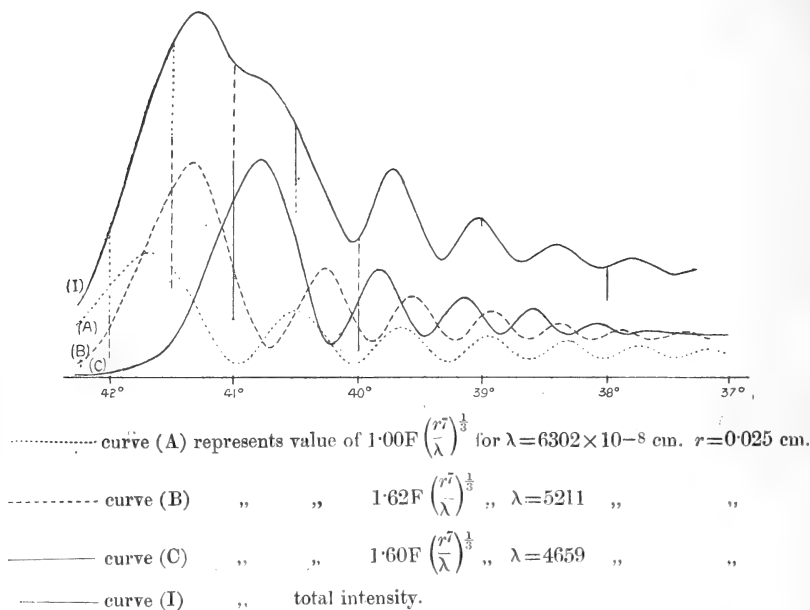
LX. *A Comparison of the Ionization produced in Gases by penetrating Röntgen and Radium Rays.* By A. S. EVE, *M.A., Lecturer in Mathematics at McGill University, Montreal* *.

EVIDENCE is gradually accumulating in favour of the view that the γ rays and Röntgen rays are identical in their nature. The case has been fully considered by Rutherford in 'Radioactivity,' pp. 141–146. The chief objection to the theory consisted in the wide divergence of the effects produced by the respective rays when ionizing various gases and vapours. The three types of rays from radium all produce ionization currents proportional to the densities of the gases through which the rays pass. The Röntgen rays have been found to diverge widely from this law.

The following table will furnish an example; it is an extract from a paper by Strutt, Proc. Roy. Soc. 5th Aug. 1903 :—

* Communicated by Prof. Rutherford, F.R.S.

Gas.	Relative Density.	γ Rays.	Röntgen Rays.
Air	1	1	1
H₂S	1·2	...	6
Chloroform	4·3	4·9	32
Methyl Iodide	5·0	4·8	72

The values in the right column were found with soft rays, and it has been noted that the results obtained varied considerably with the type of bulb employed. Professor Rutherford suggested that the more penetrating rays from a hard bulb might give figures approximating to those obtained from γ rays. Some preliminary experiments confirmed this forecast, and a short account of them was forwarded to ' Nature,' 10th March, 1904, to which Rutherford added a letter on the theoretical explanation of the results.

Apparatus.

A large hard bulb was employed with a powerful induction-coil and Wehnelt interruptor.

Fig. 1.
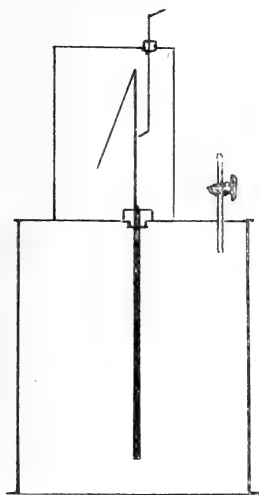
Two electroscopes were placed at equal distances, about 60 cms., from the focus of the bulb. Each electroscope was completely covered by a removable lead vessel 1·7 mm. in thickness. In the earlier experiments trouble was caused by some of the gases attacking the leaves of the electroscope. The apparatus used was therefore modified to the form shown in figs. 1 and 2. The lower vessels were brass cylinders with zinc ends, measuring 12 cms. in diameter, 13 cms. in height, and 1·8 mm. in thickness. Brass rods along the axes of the cylinders were insulated by ebonite corks and passed upwards into small electroscopes, 5·5 × 4 × 9 cms. These were covered by lead pots 1·7 mm. thick. The large and small lead covers were used whilst the bulb was running, and were removed in order to take the readings. The upper electroscopes were

further screened from the direct rays from the focus of the bulb by a block of lead 4·7 cms. thick.

Fig. 2.
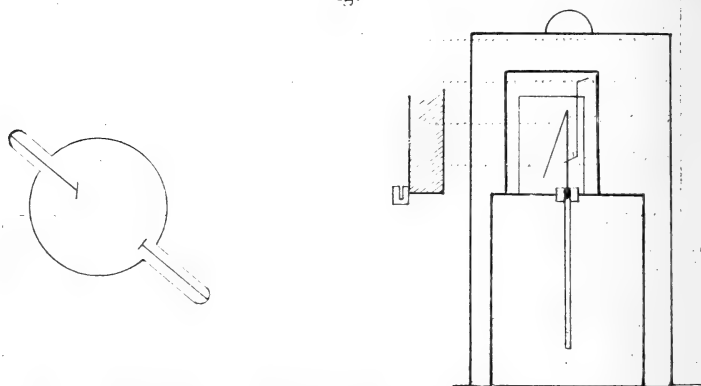
One of the brass cylinders was filled with air at barometric pressure, and served as a standard. The variations in the intensity of the rays from the bulb rendered such a standard necessary. The other cylinder was exhausted by a pump and filled with the required gas which passed through glass-wool and phosphorus pentoxide. In working with the vapours of chloroform, carbon tetrachloride, and methyl-iodide, readings were taken at various pressures less than the vapour-densities. The natural leaks were deducted from the deflexions due to the ionization currents, and the readings of the exhausted electroscope were reduced to a percentage of those of the standard electroscope.

Arrangements were made to take observations of the values for the γ rays. The radium was sealed in a glass tube placed in a hole in a cube of lead suspended behind the thick lead screen. The rays therefore passed through 1 cm. of lead before reaching the lower large cylinders, and through more than 6 cms. in order to strike the small electroscope above. In the early experiments some irregularities were traced to convection currents caused by the heat from the electric lamps when the lead vessels were removed. The heat was screened by two sheets of plate-glass 6 mm. thick and by a sheet of paper. The Röntgen rays therefore penetrated the walls of the bulb, 12 mms. of glass, 1·7 of lead, and 1·8 of brass. Care was taken to employ a saturating E.M.F.

Röntgen Rays.

The following table shows the values obtained in the present experiment as compared with those previously recorded for soft rays :—

Gas.	Relative Density.	Previous values.	Present values.
H	·07	·11*	·42
Air	1·0	1·0	1·0
H₂S	1·2	6	·9
SO₂...........................	2·2	8	2·3
Chloroform	4·3	32	4·6
Carbon Tetrachloride	5·3	45	4·9
Methyl Iodide	5·0	72	13·5

* Strutt ·11; J. J. Thomson ·33; Rutherford ·5; Perrin ·026.

It will be seen that hard rays show a closer agreement than soft rays with the density law which holds for the γ rays of radium. It is probable that a still harder bulb would further reduce the value obtained for methyl iodide. By interposing an additional lead screen it was shown that the value obtained depended more on the hardness of the bulb than on the amount of lead penetrated. This has been proved more directly by McClung ('Nature,' 17th March, 1904), who has obtained lower ionization values, except in the case of hydrogen, by increasing the hardness of the bulb employed.

The curves for pressure and current are shown in fig. 3, for air, H₂S, and H. Their curvature indicates a considerable amount of secondary radiation; this will be considered later. The portion of the curve between 18 and 50 mms. was found to be a straight line (fig. 4), and the readings for the gases used were taken between these limits. The value for hydrogen relative to air seemed to be constant for all pressures. The curve for H₂S intersected the curve for air. The scale of the diagram is too small to show this, but the values obtained were :

Pressure in mms.	Value relative to air.
760	1·45
218	1·36
63	1·05
50	·90

γ *Rays of Radium.*

Observations were made in a manner similar to those for Röntgen rays. The current-pressure curve between 30 and

Fig. 3.

X Rays
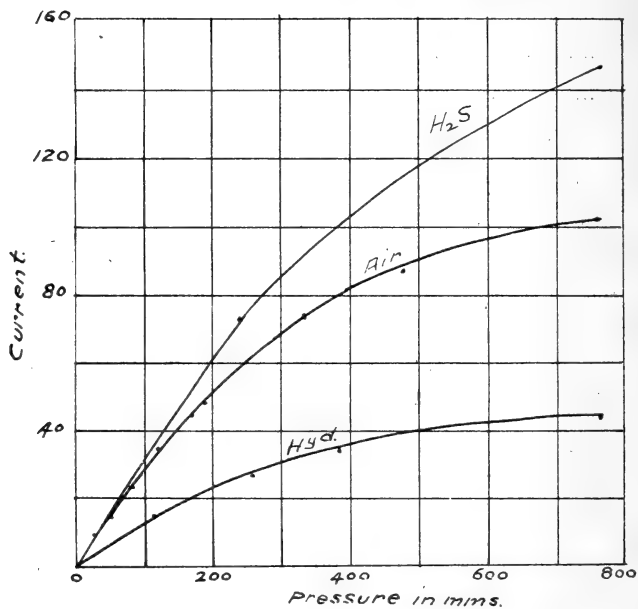
Fig. 4.

X Rays. Air.
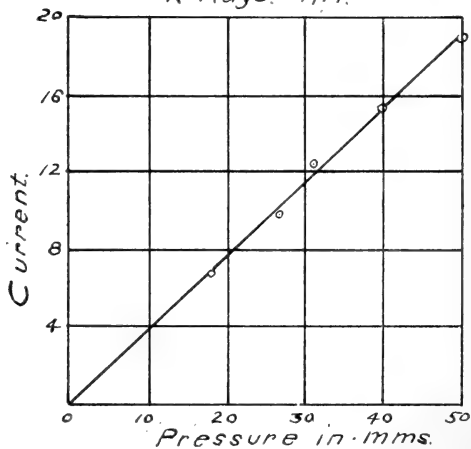
760 mms. was found to be a straight line (fig. 5). The values obtained were practically the same as those given by Strutt, Proc. Roy. Soc. 5th Aug. 1903.

Fig. 5.

ɣ *Rays. Air.*
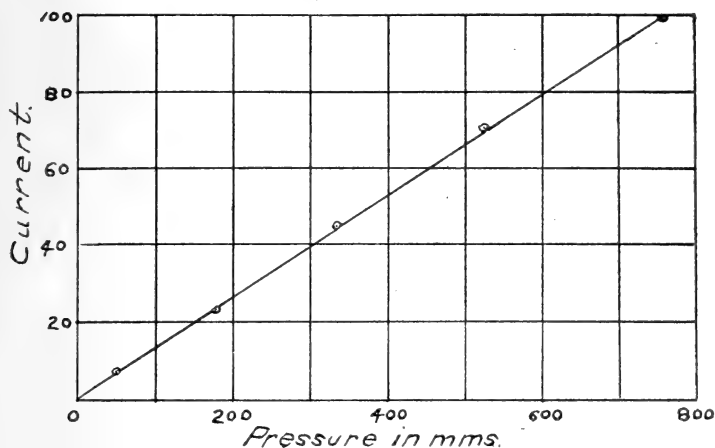
Gas. ...	Density.	Strutt.	Present Experiments.
H	·07	·17	·19
Air	1·0	1·0	1·0
H_2S	1·2	...	1·23
Chloroform	4·32	4·88	4·8
Methyl Iodide	5·31	5·67	5·6
Carbon Tetrachloride	5·05	4·80	5·2

Secondary Radiation.

With the Röntgen rays there was abundant evidence of secondary and tertiary radiation. The block of lead and the large and small lead covers effectively screened the upper electroscopes. The amount of secondary radiation in the lower brass cylinders was proved to be large. A sheet of cardboard painted with phosphoric acid reduced the current from 100 to 40, two sheets from 100 to 26. The cardboard appeared to be an unsatisfactory conductor, and the interior of the brass cylinder was therefore coated with sheet

aluminium ·85 mms. thick.   This reduced the ionization-current from 100 to 22, and a screen of double thickness to 20·6.   The electroscope was then filled with air and various gases, and the observations were repeated.   As the deflexions of the leaves were only about a quarter of their former value, it was necessary to run the bulb for a considerable time.   The results were not as accurate as in the first instance.   Although 78 per cent. of the rays had been cut off, the values obtained agreed with those previously determined. There cannot be any wide divergence between the relative conductivities for primary and secondary rays.   In the case of the secondary radiation caused by the rays, the interior aluminium screen of ·85 mms. thickness reduced the ionization-current from 100 to 79, and a double thickness from 100 to 70.   These screens had a negligible effect if placed on the outside of the electroscope.   The absorption coefficients may therefore be calculated on the assumption that the primary rays are unaffected.   If I_0 be the initial intensity, and I_t be the intensity after penetration through thickness t, then λ the coefficient of absorption may be calculated from the equation $I_t = I_0 e^{-\lambda t}$.

The values of λ for secondary rays from lead penetrating aluminium were found to be

> Secondary γ rays 10
> Secondary Röntgen rays 47

The former were therefore about five times as penetrating as the latter.

The curves in fig. 3 indicate that secondary Röntgen rays are absorbed in a few centimetres of air, except at low pressures, whilst the straight line in fig. 5 shows that the γ rays pass the air with little absorption.   This agrees with Becquerel's results obtained by photographic methods.

By interposing lead screens outside the electroscope the penetrating powers of the primary rays were also compared. The values of λ for lead with Röntgen rays were 24·6 for the first 1·7 mms. and 21·0 for the thickness 1·7 to 3·4.   There is a similar decrease in the value of λ for lead with γ rays from ·64 to ·47, as the thickness ranges from 2 to 96 mms. Taking mean values, it may be said that γ rays are about 40 times as penetrating as the Röntgen rays used.

Whilst these experiments have been in progress, papers have appeared in the *Annalen der Physik* (Nos. 6, 7, 1904) by F. Paschen.   In the first he describes the repetition, with γ rays, of the experiments made by Strutt in the case of β rays (Phil. Mag. 1903, [6] vi. p. 538).

On the other hand, J. J. Thomson, using a thicker lead case round the radium employed, failed to obtain any such results (Soddy, Radioactivity, p. 181). McClelland's observations by other methods also indicate that the γ rays do not carry a charge (Phil. Mag. July 1904).

Paschen, in his second paper, adds further indirect evidence of the view that the rays are projected particles with a negative charge.

Since this paper was forwarded to England, the writer has made some further experiments, which form the subject matter of a future communication.

It may be convenient to give a brief summary :—

(1) An endeavour to deflect the γ rays in a very powerful magnetic field has failed to show any curvature of the rays.

(2) The β rays can be deflected entirely away from the γ rays, so that the two classes of rays belong to different types.

(3) If radium is enclosed in a mass of lead, several centimetres thick, negatively charged particles are projected from the outer layers of the lead ; these can be readily deviated in a magnetic field and are absorbed by one or two millimetres of lead, whereas the γ rays will penetrate several centimetres. Thus the γ rays give rise to secondary radiation of the cathode type, and it is these rays which Paschen has detected and which he incorrectly concluded to be γ rays.

Direct experiments by Becquerel, Rutherford, and Villard have failed to detect any deflexion of the γ rays in a powerful magnetic field.

The theoretical aspect of the case is summarized by Rutherford in ' Nature,' 10th March, 1904 :—

" As a result of the sudden expulsion of the β particle from radium, it is to be expected that a narrow electromagnetic pulse, *i. e.* a ' hard ' or penetrating type of Röntgen rays would be generated. In addition, on account of the great speed of the β particle, it is able to penetrate through a considerable thickness of matter before it is stopped. A broad pulse or ' soft ' Röntgen ray should thus arise at the point of incidence of the β rays."

It has been pointed out that the γ rays arise at the time of the expulsion of the β ray and not by the bombardment of the radium itself, since the amount of γ rays from radium is independent of its degree of concentration.

From the present theoretical view that Röntgen rays are pulses, it follows that a type of Röntgen rays should be produced by the β ray, and from other evidence we should expect these to be of a very penetrating character.

The evidence that γ rays are Röntgen rays is two-fold :—

(1) The γ rays are not deflected by a magnetic field.
(2) In the changes of radium the β and γ rays always appear together and in the same proportion. This result is to be expected if the γ rays are produced by the β.

In any case the present paper removes an objection to this view. It is true that methyl iodide remains as a marked exception to the density law, but it is probable that harder rays than those employed would bring this vapour into line with others.

In conclusion, I have to thank Professor Rutherford for suggesting the experiment, and for his encouragement and advice in carrying it out.

McGill University, Montreal, July 1904.

LXI. *Notes on the Radioactivity of Various Materials.* By Hon. R. J. STRUTT, *Fellow of Trinity College, Cambridge**.

IN this note I shall put together some rather fragmentary observations on the presence of radium in various materials. The note may be regarded as supplementary to a paper published in the Proc. Roy. Soc. lxxiii. p. 192.

The method of experimenting was the same as is described in that paper. The emanation was extracted from the substance by heat, and introduced into an electroscope : the rate of leak was read in the usual way.

The following Table (p. 619) gives the results for the various materials, on the same scale as in the former paper. These were not, for the most part, carried out more than once ; and must not be regarded as much more than indications.

1. This refers to a hard deposit, left in the lead pipes of the ancient Roman baths. It contains less iron than the modern deposits, and is much less radioactive. It would be rash to draw any positive conclusion from the latter fact, for we are ignorant of the precise conditions under which the Roman deposits were formed. But the idea suggests itself that the radium has been here separated for 2000 years from its parent (uranium ?), and is consequently much diminished in activity. I have tested the most active of the modern deposits, but found no uranium in 100 grams of it. One milligram should have been easily detected.

2–5. Lord Blythswood and Mr. Allen have found the gas of the Bath springs to contain emanation. I wished to compare the activity of this gas with that of the other materials,

No.	Material.	Rate of Leak per gr. of Solid or per c.c. of Gas.	Rate of Leak per litre of water.
1.	Deposit from Roman pipes, Bath	·45	—
2.	Gas bubbling up at King's Spring, Bath	5·0	—
3.	„ „ Cross Bath, Bath	3·5	—
4.	Dissolved gas, King's Spring, Bath	—	270
5.	„ „ old Royal Spring, Bath.................. ..	—	430
6.	Mud from hot well, Clifton....................................	nil.	—
7.	Dissolved salt, St. Anne's Well, Brighton.................	·5	·46
8.	Chalybeate deposit from sides of this well.................	nil.	—
9.	Dissolved salt from water, Kelso, N.B.	nil.	nil.
10.	Dissolved salt, water from Red Sandstone, N. Cheshire.	nil.	nil.
11.	Fur, Cambridge hot-water pipes	nil.	—
12.	Deposit, Bakewell hot spring	nil.	—
13.	Black deposit, from Aldfield Spa, Ripon	nil.	—
14.	Dissolved salt, from ditto	1·5	6·0
15.	Dissolved salt, barium water, Llangammarch Wells, Central Wales	1·8	7·8
16.	Thanet sand	nil.	—
17.	Native platinum	nil.	—

and, more especially, with that due to the traces of radium dissolved in the water. It was found that the activity of the dissolved gas extracted by boiling from the freshly collected water was very great compared with that due to the dissolved radium. The gas bubbling up with the water was not so active as that extracted by boiling. If the radioactivity of the Bath waters has any connexion with their reputed medicinal value, the dissolved emanation, not the traces of dissolved radium salt, must be the agent principally concerned.

There are slight differences between the activity of the gases collected by boiling or otherwise at the various springs. These, however, may well be due to differences of temperature, and to differences in the conditions under which the bubbling gases are exposed to the dissolving action of the water, near the place of collection.

The quantities of emanation in the water show clearly that it must have had access to large quantities of radium. Indeed, the radioactivity so frequently associated with thermal springs is suggestive of the conclusion that radium is a far more abundant constituent of the internal portions of the earth than of the external ones.

6–17. These remaining items do not call for any special comment.

Distinct radioactivity appears to be much commoner in the saline residues of waters than in rocks.

Terling Place, Witham.
　　Oct. 10th, 1904.

LXII. *The Magnetization of Iron in Bulk.*
By W. M. Thornton, *D.Sc. (Vict.)*[*].

§ 1. A method of testing the permeability of iron in bulk, with experiments on large rings.

§ 2. The influence of core-currents upon the growth of the magnetizing current. Comparison of coils with solid and laminated cores.

§ 3. Calculation of subsidence or growth of current in coil with large solid core of circular section. Evidence of sudden molecular movement as the iron approaches saturation.

THE permeability of specimens of iron has long been known to depend upon their size, shape, and previous history. The usual method of determining magnetic properties by tests on small samples cut or specially cast from a larger mass cannot be entirely relied on to give the same values that would be obtained if it were possible to apply tests to the material in bulk. It is especially necessary, in order to have uniform results, even from a batch of samples of the same metal, that they should have had the same annealing or have been subjected to the same process and time of cooling. If they could be conveniently compared, a 20 per cent. difference between the behaviour of a small specimen and of the actual magnet frame would be found to be not uncommon, and the permissible variation between castings of the same group is sometimes specified of this magnitude.

One method of testing has been developed[†] free from most objections but not entirely free, because the amount of metal examined is a very small portion of the whole. It seems, in fact, that accuracy cannot be guaranteed unless the specimens are large enough to be considered as " in bulk," and are tested as a whole.

The two methods at present in use with the smaller specimens are the ballistic and the magnetometric. The latter is only suitable for long thin rods, the former for cores which, either by reason of the low inductance or high resistance of their windings, have small time-constants, so that the total inductive change following any alteration of the exciting current is practically instantaneous.

Some years ago the writer had occasion to measure the slow magnetic movements which occur on making or short-circuiting the exciting current of large dynamo-electric machines[‡]. To do this a recording millivoltmeter was connected to an exploring coil surrounding the core. Any

* Communicated by the Physical Society : read June 24, 1904.
† Drysdale, Journal Inst. Elect. Engrs. vol. xxxi. p. 283.
‡ *Vide* 'The Electrical Engineer,' vol. xxix. p. 523 ff.

change in magnetization was then registered by the induced voltage in this secondary circuit, and the absolute accuracy of the result was made to depend only on the standardization of a voltage and the speed of the record. For let a voltage e be induced during time t in a coil with n turns. The magnetic movement in the core which has produced this is then

$$N = \int \frac{e}{n} \cdot dt,$$

And if $\bar{e}$ be the mean voltage during the total time T of change

$$N = \frac{\bar{e}T}{n},$$

corresponding, where a is the cross-sectional area of the core, to a change in flux density of

$$B = \frac{\bar{e}T}{na}.$$

The immediate object of these tests was to measure directly the leakage coefficients of large machines. Incidentally the permeabilities of various parts of the magnetic circuits were also determined. It was then suggested by Mr. C. C. Hawkins that since these were values for iron in bulk, the same method might be used for testing large rings specially wound for the purpose.

The lower limit to the size of ring depends only on the point at which the iron can be considered to be in bulk. So far as this alone is concerned a mass of anything over 100 lb. would probably be sufficient, in the shape of a yoke-ring of the usual proportions; and it is evident that to avoid expense the smallest size should be used, provided that the method of testing to be employed can be applied with accuracy and convenience.

In the experiments previously referred to the magnet core was of cast steel 15 inches diameter, the whole frame weighing several tons. This circuit had an inductance of about 50 henries and a resistance of 10 ohms, the time-constant therefore being 5 seconds.

The method of testing adopted in this case was to make the exciting circuit, allow the current to become steady, and then to short-circuit the windings, recording photographically the voltage induced in the exploring coil. It was found that the methods were uniformly accurate, and in

applying the test to sample rings a time-constant of this magnitude was aimed at.

Two rings were kindly prepared by Messrs. W. H. Allen, Son & Co., one of cast iron the other of cast steel *, both being the yoke-rings of standard machines with the brackets and facings turned off. The former was wound with 1000, the latter with 777 turns of copper wire to carry 50 amperes for a few seconds without undue heating, having respectively 2·9 and 1·9 ohms resistance cold. In each case, however, the attempt to use the "make, short-circuit" method, recording photographically, had to be abandoned on account of the difficulty experienced in obtaining satisfactory records. The motion of the spot of light was so rapid that the photograph was too faint for accurate measurement. Another method had then to be found, and recourse was eventually had to the simple device of altering the current gradually with several brief pauses, recording as before the induced voltage in the secondary or exploring coil.

A vertical wooden pipe 10 inches square by 6 feet long was filled with dilute sulphuric acid and fitted with lead end-plates to which the voltage required to give the desired current was applied. A movable lead electrode was suspended from a pulley so that it could be readily raised or lowered. The movable and lower electrodes were connected through a standardized Weston ammeter to the primary winding on the ring. With the movable plate at the bottom the winding was short-circuited, when at the top it received the full exciting voltage.

The galvanometer used in the secondary circuit was by Pitkin, of the moving-coil type with the lower suspension made bifilar. It was just aperiodic. The recording drum, 5 inches diameter by 10 inches long, was contained in a box provided with an adjustable shutter, and was at first driven by a clock within the drum. It made one revolution a minute and ran for ten minutes, the speed slowly decreasing from the start.

To avoid this drop of speed a small spindle was screwed into the end of the axle of the drum, and, passing through the end of the box, was fitted with a pulley so that by using a gramophone-clock as motor a suitable constant speed could be obtained for the record.

In applying the magnetizing current the pauses, which were about every ten amperes, were always long enough for the spot of light to remain for a moment at rest so that the curves as recorded were as follows (fig. 1).

* *Vide* Journal Inst. of Elect. Engrs. vol. xxxiii. p. 539.

The upper areas represent the changes of magnetic flux with increasing currents, the lower with decreasing. The area A of each curve was measured with a planimeter.

Fig. 1.—Typical Record for Half a Hysteresis Loop.

(Each area denotes the change of B for an observed change of H.)
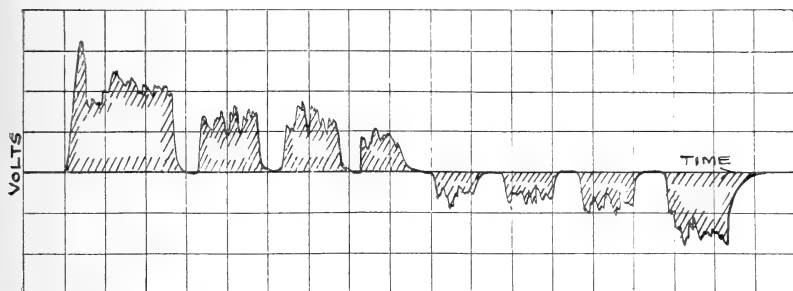
Knowing, then, the voltage e for one centimetre deflexion and the seconds s the periphery of the drum took to move one centimetre, the area multiplied by the product es gave the total lines threading the exploring coil. The corresponding change in flux density

$$B = \frac{Aes \times 10^8}{na} = kA \text{ say };$$

A having in succession the values recorded. The flux density at any current is then k times the sum of the areas up to that point. The value of the magnetizing current at each pause was noted by the observer. The areas can be readily checked by allowing the current to change from zero to the maximum and back to zero without pause, when it is found that the agreement between the areas so found and the separate steps is within 1 per cent.

The difference between the areas above and below the zero line represents the residual magnetism.

The results of the tests on the two rings are given in Tables I. and II. and in fig. 2.

The observations are reduced in a manner similar to the " step by step " method of ballistic testing. The labour could be greatly reduced by the use of a quantometer *, if that most useful instrument were generally available. Dr. Beattie having kindly lent his, the tests were repeated, obtaining

* *Vide* The Electrician, vol. l. p. 383.

2 X 2

FIG. 2.—Magnetization Curves of Large Rings.
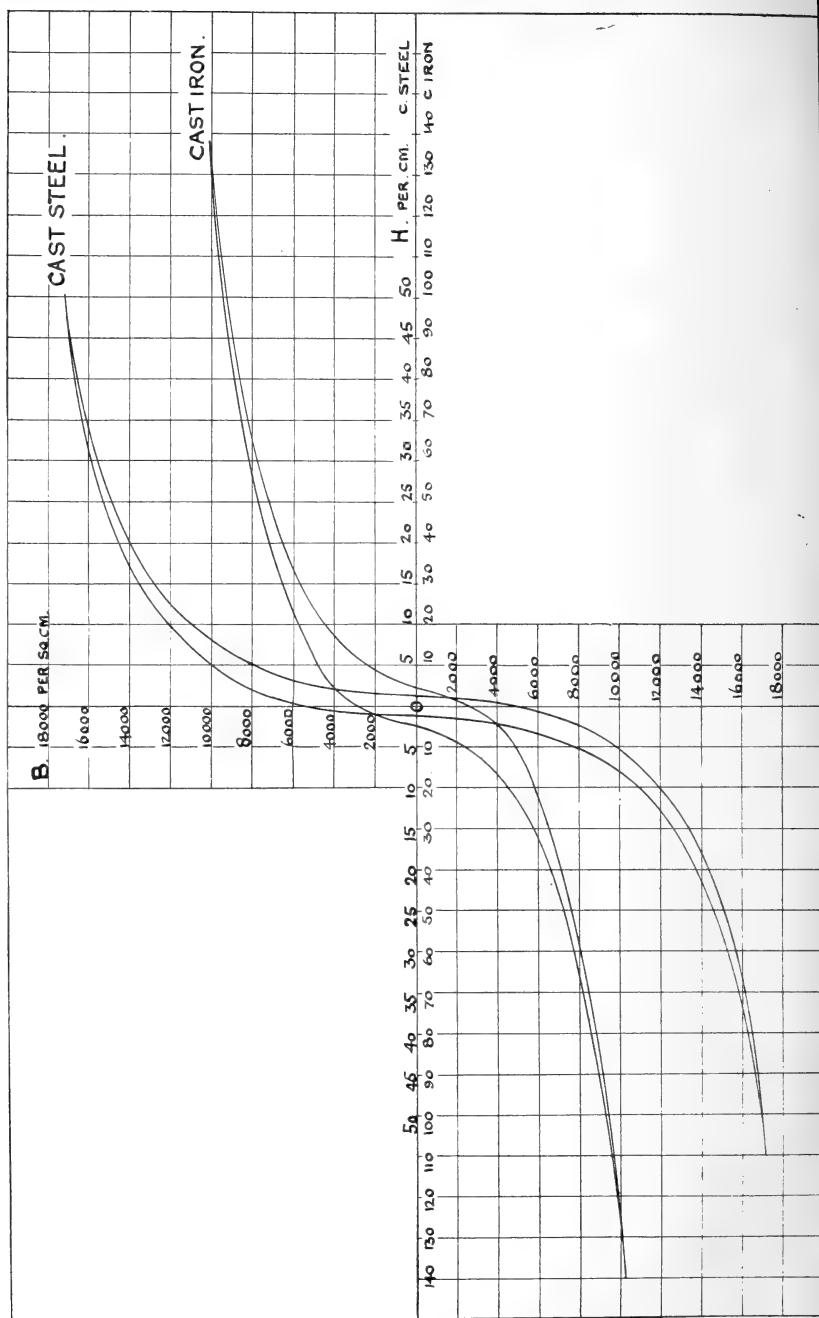
TABLE I.—Cast Steel Ring.

H.	B inc.	μ.	B dec.	μ.	w ergs per c.c.
0	−5000	−∞	5000	+∞	
·628	− 4000	−6370	6500	10340	
1·25	0	0	7000	5510	
2·51	+5000	1990	8000	3180	1200
5·0	7900	1580	9600	1920	2490
7·54	9500	1260	11000	1460	3300
10·05	11000	1095	12000	1194	4300
12·57	12000	955	13000	1034	4900
17·6	13600	774	14200	807·5	5900
23·6	14500	614	15000	635	6630
25·1	15000	598	15300	610	7000
37·8	16300	432	16500	436	7800
50·4	17100	340	17150	341	8500
62·8	17300	275	17350	276	
75·2	17600	233	17600	233	
109·3	18100	165·5	18100	165·5	
125·7	18500	147·3	18500	147·3	
201	19600	97·6	19600	97·6	

Volume of Cast Steel Ring *1452* cub. centim.

TABLE II.—Cast Iron Ring.

H.	B increasing.	μ.	B decreasing.	μ.	w ergs per c.c.
0	−2900	∞	2900	∞	
2·5	−1800	720	3000	1353	
3·78	− 950	250	3400	900	
4·4	0	0	4000	796	
6·28	1000	159	4250	678	380
12·67	3000	239	5000	398	2800
25·14	5150	203	6100	243	5200
37·7	6500	172·5	7000	185·7	7700
50·28	7200	143	7550	150	8900
62·8	7800	124	8100	129	10,200
73·4	8300	113	8500	116	11,100
88·0	8750	100	9000	102·2	12,200
100·5	9150	91	9300	92·5	13,000
125·7	9800	78	9950	79	15,000
138·0	10100	73·5	10200	74	15,250
163·2	10600	65	10650	65·25	
188·5	11000	58·6	11000	58·5	
251·4	11900	47·3	11850	47·25	

Volume of Cast Iron Ring *3258* cub. centim.

agreement within 2 per cent. The method of testing was now as originally designed. The ring windings were connected to the generator through reversing and spring-break change-over switches [*]. The operations were then :—

1. Make the circuit and allow the current to reach a steady state.
2. Short-circuit the windings and allow the current to fall to zero.
3. Reverse the coil-connexions and repeat the process.

This was done several times for each current, the quantometer readings being eventually reduced as in ordinary ballistic testing.

The instrument is so easily worked, and its readings are so constant when there is a strong field in its air-gap, that if it were better known it would no doubt be used for magnetic testing on a large scale in preference to any other. For measuring large changes of magnetization, these methods, the only two which are not ballistic, represent extremes. In the first with a recording voltmeter the best results are obtained when the current is varied as slowly and uniformly as possible; in the second with the quantometer, when the change is as rapid as the inductance of the winding and the tailing-off effect of the core currents will permit. The latter do not affect the accuracy of either method.

§ 2. The rings exhibit in a very marked manner the effects of eddy currents and of change of permeability in the core on the growth of the magnetizing current. Curves I_1, I_2, fig. 3, were taken, using an oscillograph as a dead-beat ammeter, and recording the deflexions photographically, as previously described, but with the drum making one revolution a second. They are remarkable on account of the pause in the rate of growth observed in each, and for the difference between the two curves. I_1 corresponds to a molecular change from $+B_r$, the residual magnetism, to $+B_m$, the maximum induction-density; I_2 from $+B_r$ to $-B_m$. The magnetic movement is therefore much greater in the second case, and the current is accordingly more retarded.

In order to study the reaction of the core currents on the magnetizing circuit, it is necessary to compare the curves of fig. 3 with those obtained when the core is laminated. For this purpose a 20-kilowatt transformer was used, having about the same weight of iron as the rings. The high-tension winding was excited from storage-cells, and on closing the switch the current rose as shown in fig. 4 (p. 628). Here the

* Ibid. vol. lii. p. 479, " On a master switch for Central Stations."

curve I_1 indicates as before the rise from zero after the core has been already magnetized in the same direction; I_2 when

Fig. 3.—Rise of Current Magnetizing Cast Steel Ring.
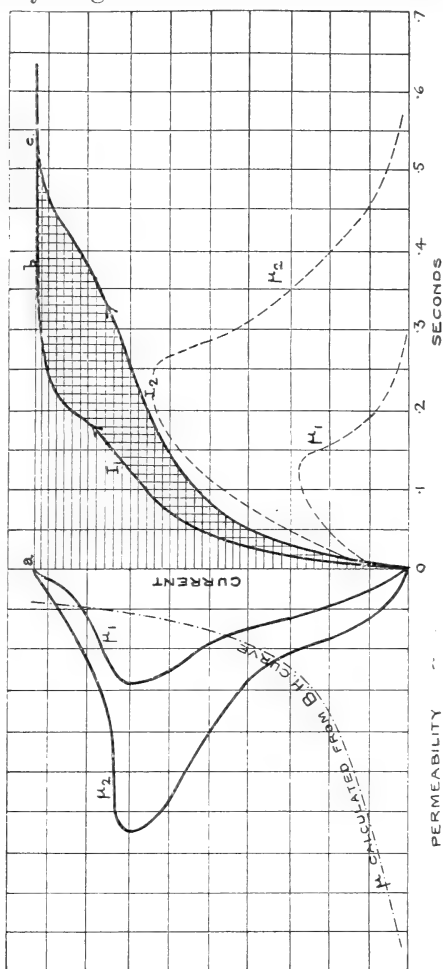
the magnetization is being reversed. The following are the details of construction affecting the curve :—

Turns on primary winding 97
Resistance of primary winding . . . ·75 ohm.
Reluctance of core (l/A calculated) . . ·15/μ.
Calculated inductance $\mu = 100$ . . . L = ·31 henry.
Weight of iron magnetized . . . . 510 lb.
Maximum value of H applied to core . 67
Calculated time constant . . . . . ·41 second.

The curves of rise were difficult to obtain photographically, the spot of light moving at about 140 cms. a second. It was necessary to greatly overdevelop the extra-rapid bromide paper used.

Fig. 4.—Rise of Current in Coil with Laminated Core.
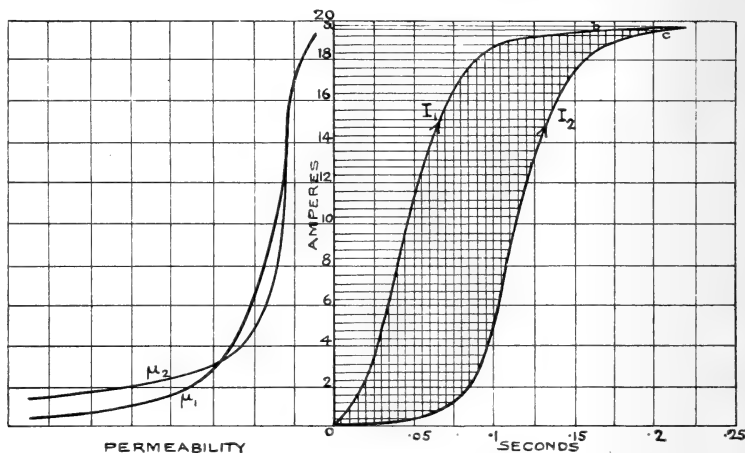
The general expression for the voltage in an inductive circuit to which a steady voltage e is applied is

$$e = ri + \frac{dN}{dt}. \qquad \qquad (1)$$

Here N is the total magnetic change and equals Li. The inductance L is, in all cases where iron is used, a variable. Hence

$$e = ri + L\frac{di}{dt} + i\frac{dL}{dt}; \qquad \qquad (2)$$

and since L is proportional to μ we may write

$$e = ri + k\mu\frac{di}{dt} + ki\frac{d\mu}{dt} \qquad \qquad (3)$$

or

$$k\mu \frac{di}{dt} + i\left(r + k\frac{d\mu}{dt}\right) = e. \qquad \qquad (4)$$

In curve I_2, fig. 4, the current pauses for some time after switching on, $d\mu/dt$ must therefore be exceedingly large since both i and di/dt are small. If desired it can be found, for all the other quantities in (4) are determinable.

In curve I_1 there is scarcely any initial pause, which is to be expected, since the core starts well on the way to saturation.

It is of some practical importance to notice that the horizontally shaded areas *abo, aco* represent the magnetism which the current induces whilst rising.

Equation (1) may be written

$$dN = (e - ri)dt\,;$$

so that

$$\frac{N}{r} = \int (i_0 - i)dt, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (5)$$

where i_0 is the final steady current.

The value $(i_0 - i)$ is the intercept between the curve and the horizontal tangent. The right-hand side therefore is the area A between the curve, the vertical axis, and this tangent.

Thus

$$N = k'A \times 10^8 = kA \text{ say.} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (6)$$

k' being the volt seconds per sq. cm. of the record, known from the scale of the instrument. This is independent of whether the core is solid or laminated.

If A_1, A_2 are the respective areas *abo, aco*, c the cross-sectional area of the core, and t the turns on the winding, then the change of flux density corresponding to an area is

$$B = \frac{N}{tc} = \frac{kA}{tc}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (7)$$

Denoting by B_1, B_2, the flux densities corresponding to A_1, A_2, the following relations are evident :—

$$\frac{B_1 + B_2}{2} = B_m, \text{ the maximum induction,}$$

$$\frac{B_2 - B_1}{2} = B_r, \text{ the residual magnetism.}$$

Thus

$$\frac{B_1 + B_2}{B_2 - B_1} = \frac{B_m}{B_r}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (8)$$

Also

$$A_1 + A_2 = B_m . \frac{2tc}{k} \quad \cdot \quad \cdot \quad \cdot \quad (9)$$

$$A_2 - A_1 = B_r \frac{2tc}{k}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (10)$$

The sum of the horizontally shaded areas therefore represents the maximum induction density reached, their difference, that is the vertically shaded area forming the loop *bco*, the residual magnetism. It is clear that if there were no retentivity the curves would be identical.

These relations can be illustrated from figs. 3 & 4 as follows :—

	Cast Steel Ring.	Transformer.
Area A_2 (by planimeter).	6 sq. in.	10·75 sq. in.
„ A_1	3·1 „	4·67 „
$(A_2 + A_1)$	9·1 „	15·42 „
$(A_2 - A_1)$	2·9 „	6·08 „
Ratio B_m/B_r	3·1	2·5
Sectional area C	75·1 sq. cm.	420 sq. cm.
Turns on winding	777	97
Resistance in circuit ...	1·9 ohms.	5 ohms.
Scale of record .... $\{$	10 amp. = 1 inch.	4 amp. = 1 inch.
	·125 sec. = 1 inch.	·05 sec. = 1 inch.
k	$2·3 \times 10^5$ per sq. in.	$1·0 \times 10^4$ per sq. in.
B_m (from eqn. 9)	17,900 per sq. cm.	18,900 per sq. cm.
B_r („ „ 10).........	5,770 „	7,560 „
H max.	225 „	67

Comparing the values of B_m and B_r obtained in this way with those of § 1 for the ring, it is seen that there is a fairly close agreement for the maximum induction, but about 15 per cent. difference in the residual.   Of the two methods, the latter is the more likely to be correct in determining the residual flux.

These relations suggest a very simple method of measuring the induction through the windings of large electromagnets, such as the exciting circuits of large dynamo-electric machines, which may be in use but has not come to the writer's notice.   It is to record the rise of current when the magnetization is suddenly switched on, and to measure the area corresponding to *abo* in fig. 3.   The curve will in general shape resemble curve A in fig. 5; and since most large exciting currents take from 5 to 60 seconds to reach their full value, the speed at which the record must be made is not excessive. Proceeding in this way, the magnetic leakage between the windings and the armature can be measured by ordinary test-room instruments.

A still further simplification suggests itself in the case of machines where the time constant is above five seconds. Instead of *recording* the current rise, the times, taken by a stop-watch, for the current to reach given values in turn are plotted.   The first rush cannot be measured, but after about three-quarters of a second readings can be obtained which coincide with the recorded curve.   To measure the magnetism of a large generator or motor it is then only necessary to have a deadbeat ammeter and a stop-watch.

Comparing figs. 3 and 4, the first thing observed is that the magnetizing current at first rises more rapidly when the core is solid than when laminated, an effect predicted by Mr. Heaviside but not hitherto observed. This is due to the

Fig. 5.—Calculated and Observed Coil Currents.

shielding effects of the eddy currents, the magnetizing influence of the coil current not reaching the central portion of the core immediately. As the magnetizing wave-front enters, there is a double effect, the flux density in the outer layers increasing with the coil current, and the area magnetized also increasing. The result is that the permeability of the core apparently increases, as shown in fig. 3. The point where this begins to fall suddenly indicates the cessation of the core currents. This takes place in about ·3 second, the

rate of penetration is therefore in this case about 3·3 inches a second.

When the permeability is assumed constant and no core currents flow, equation 2 takes the well-known form

$$\text{L}\frac{di}{dt} + ri = e \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (11)$$

from which

$$i = \frac{e}{r}\left(1 - \epsilon^{-\frac{rt}{\text{L}}}\right). \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (12)$$

If this curve be drawn with a mean value of L suitable to the circuit, the corresponding curve with core currents allowed to flow appears with a superposed wave. The solution of (4) is impossible unless μ can be expressed as a function of time; and even if this were known, there would be a further complication in expressing the magnetic force as a function of the radius. The only course open is to find experimentally the curve of rise, and from it to deduce the changes of μ. It is seen from figs. 3 and 4 that the relation of μ to t does not lend itself to simple treatment.

The effect of the core currents may be summarized thus. When the core is of such dimensions or shape that the magnetizing wave from the circumference reaches the centre in a time equal to or less than the time-constant of the circuit, the rise is wavy; when it is greater the core currents merely prolong the time of growth without distorting the curve of rise.

§ 3. The distribution and reaction of induced currents in solid cores present the most difficult and complex problems in applied magnetism. The regulation of alternators, the heating of magnet cores due to armature reaction, the sluggishness of response exhibited by machines with solid cores which depend upon series excitation, are examples. Most of the practical cases are beyond the reach of exact analysis; but in order to see whether Mr. Heaviside's solution for the rise of magnetizing current in a coil surrounding a solid core gave any clue to the observed pause in the rate of rise when the core is very large, the following numerical illustration was worked out. The curves he gave * for a core 1 cm. radius are smooth and free from distortion. He expresses the magnetic distribution within a circular solid core, the magne- tizing coil of which has just been short-circuited, by a series

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* Electrical Papers, vol. i. p. 398.

of terms of gradually diminishing amplitude and increasing coefficient of decrement *, viz. :

$$H_r = \frac{2H_0}{na(1+s^2n^2)} \cdot \frac{J_0(nr)}{J_1(na)} \cdot e^{Dt} ;$$

where H_r is the magnetic intensity at a radius r, H_0 the initial value at the boundary. The radius of the core is a; n the successive roots of the equation

$$J_0(na) = snJ_1(na),$$

J_0 and J_1 being the usual Bessel functions. The coefficient $s = \dfrac{L\rho}{2\pi\mu aR}$, in which L is the inductance and R the resistance of the coil winding; so that

$$s = \frac{2A\rho T^2}{laR} ;$$

where A is the cross-sectional area of the core, l its mean length, ρ its specific resistance, μ its permeability, and T the turns on the winding.

D the damping coefficient $= \dfrac{-\rho n^2}{4\pi\mu}$.

In the case of the cast steel ring $s = \cdot 6055$, assuming as a first approximation that the core is circular.

For $a = 4\cdot 87$ cms. $l = 193$ cms.
       $\rho = 12,000.$ $T = 777.$
       $r = 1\cdot 9 \times 10^9.$

Thus

$$\frac{H_a}{H_0} = \frac{\cdot 2486}{1 + \cdot 3675n^2} \cdot e^{Dt} .$$

The n's are the roots of $\dfrac{J_0(z)}{J_1(z)} = \cdot 1243z.$

This can be most readily solved by inspection and trial from tables of J_0 and J_1. The roots so found are given in the following Table, which also gives the working of the different terms. It will be seen that the sum of the first twenty terms of the amplitude is only $\cdot 9$ instead of nearly unity. The reason for this is that in the expression for s the conductivity enters as an unknown figure. The best values

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* *Loc. cit.* p. 397.

TABLE III.

$z = na$	n	n^2	$(1 + \cdot3675n^2)$	A	D ($t =$)	·2 sec		·5 sec		1·0 sec		2 sec		3 sec		4 sec	
						e^{Dt}	$A e^{Dt}$	e^{Dt}	$A e^{Dt}$	e^{Dt}	$A e^{Dt}$	e^{Dt}	$A e^{Dt}$	e^{Dt}	$A e^{Dt}$	e^{Dt}	$A e^{Dt}$
2·13	·437	·1915	1·07	·2325	−·333	1·068	·218	1·18	·197	1·39	·1673	1·94	·12	2·717	·085	3·79	·061
4·94	1·014	1·025	1·377	·1805	−1·78	1·42	·127	2·43	·074	5·94	·0304	34·9	·005	206	·0008		
7·85	1·613	2·61	1·96	·1270	−4·53	2·45	·0517	96	·0134	89	·0014						
10·83	2·225	4·96	2·82	·0885	−8·62	5·6	·0156	74	·00119								
13·86	2·845	8·12	3·98	·0625	−14·1	16·75	·0037										
16·9	3·47	12·06	5·44	·0457	−20·95												
				Ratio H_a/H_o = ·7367			·4162		·2856		·1991		·125		·0858		·061

The sum of the amplitudes of the first 15 terms = ·87, of first 20 terms = ·9.

for cast steel containing silicon give an average of 12,000 of ρ, but the composition of the steel has so great an influence that any numerical estimate is uncertain. If the value chosen were too large the roots would also be too large, and the amplitudes therefore diminished. The ratio H_0/H_a is plotted in fig. 5; and since the values of H_a and H_0 are both proportional to the coil-current, this ratio shows the theoretical change of the latter with time when the permeability is constant. The decay of current in a short-circuited coil is in this case the same as the growth of current when first switching on, the curve as drawn being merely inverted.

The curve B of fig. 5 is the rise of current in a coil surrounding a solid core 41 cms. diameter. The small dip observed beyond the bend must be attributed to a rapid molecular movement. The change in the curvature is sudden, and is always obtained even with repeated magnetizations in the same direction. The above analysis shows that no such dip would be obtained if the permeability were constant. It can then only be attributed to a sudden increase in the number of molecular magnets brought into alignment. The observed effect resembles the phenomenon of crystallization in a supersaturated solution, starting at the circumference. It is in fact the direct observation, only possible when dealing with large masses of iron, of the sudden slip noticed in the magnets of Ewing's hysteresis model when the field approaches saturation.

The growth and subsidence of magnetic induction in rectangular cores cannot well be calculated except on the assumption that the core currents follow rectangular paths. This is approximately true when the thickness is small compared with the length of the section, but not when it is comparable to it.

Photographs have been taken by Professor Hele-Shaw of the flow of liquid between the minor and major semiaxes of rectangles. These show clearly the transition from a circular path at the centre to an approximately rectangular path at the outer boundary. At intermediate points the curves are too complex for the action of the core currents to be calculated where the thickness is not small compared with the length, on account of the difficulty of finding an expression for the instantaneous magnetic force in terms of the distance from the centre.

LXIII. *Slow Transformation Products of Radium.* *By* E.
  Rutherford, *F.R.S., Macdonald Professor of Physics,*
  McGill University, Montreal *.

IT has been previously shown † that radium undergoes
  disintegration through a series of well marked stages.
The radium first of all produces the radium emanation, and
this in turn is transformed into an active deposit, which
behaves as a solid, and gives rise to the phenomena of excited
activity. I have recently shown that this active deposit
undergoes three further rapid transformations ‡. For con-
venience, the products in the active deposit will be termed
Radium A, Radium B, and Radium C respectively §. The
change from A to B is accompanied by α rays alone, the
change B into C is a rayless change, while the change C
into D gives rise to α, β, and γ rays. The time T for each
of the products of radium to be half transformed is shown in
the following table :—

	T.	Rays.
Radium	......	α rays.
Radium Emanation ..	4 days	α rays.
Radium A	3 minutes	α rays.
Radium B Active deposit	21 minutes	no rays.
Radium C	28 minutes	α, β, γ rays.

The changes in radium are not, however, completed at this
stage, for it will be shown that there is very strong evidence
that there are at least two more slow transformations.

M. and Mme. Curie ‖ observed that a body exposed in the
presence of radium emanation did not, after removal, com-
pletely lose all its activity. A residual activity always
remained, which they state was of the order of 1/20,000 of
the initial activity. It will be seen later, however, that the
magnitude of this residual activity depends not only on the
amount of emanation to which the body has been exposed,
but also on the time of exposure. For an exposure of several

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* Communicated to the International Electrical Congress, St. Louis,
Sept. 16, 1904. Communicated by the Author.
  † Rutherford and Soddy, Phil. Mag. April & May 1903.
  ‡ Bakerian Lecture, Roy. Soc. Lond. 1904.
  § *Note.*—The term emanation X, which I previously employed to
designate the matter Radium A, is not very suitable, and I have discarded
it in favour of the present nomenclature, which is simple and elastic.
  ‖ *Thèses presentées à la Faculté des Sciences,* Paris, 1903, p. 116.

hours, the residual activity is less than one millionth of the activity immediately after removal.   Giesel * also observed that a platinum wire, after exposure to the emanation, showed residual activity which, he states, consists only of α rays.

An account will now be given of some investigations made by the writer on the nature of this residual activity and the chemical properties of the active matter itself.   It is first of all necessary to show that the residual activity arises in consequence of a deposit of radioactive matter, and is not due to some action of the intense radiations to which the body made active has been subjected.

The inside of a long glass tube was covered with equal areas of thin metal, including aluminium, iron, copper, silver, lead, and platinum.   A large amount of radium emanation was introduced into the tube, and the tube closed.   After seven days the metal plates were removed, and, after allowing two days to elapse for the ordinary excited activity to disappear, the residual activity of the plates was tested by an electrometer.   The activity of the plates was found to be unequal, being greatest for copper and silver, and least for aluminium.   The activity of copper was twice as great as that of aluminium.   After standing for another week, the activity of the plates was again tested.   The activity of each had diminished in the interval to some extent, but the initial differences observed had to a large extent disappeared. After reaching a minimum value the activity of each plate slowly but steadily increased at the same rate.   After a month's interval the activity of each of the plates was nearly the same, and over three times the minimum value.   The initial irregularities in the decay curves of the different metals are, in all probability, due to slight but different degrees of absorption of the radium emanation by the metal plates, the absorption being greatest for copper and silver and least for aluminium.   As the occluded emanation was slowly released or lost its activity, the activity of the metal fell to a limiting value.   The absorption of the radium emanation by lead, paraffin, and caoutchouc was some time ago observed by Curie and Danne †.

The residual activity on the plates comprised both α and β rays, the latter being present, in all cases, in a very unusual proportion.   The equality of the activity and the identity of the radiation emitted from each plate shows that the residual activity is due to changes of some form of matter deposited on the plates, and that it cannot be ascribed to an action of

* *Berichte d. D. Chem. Gesell.* p. 2368 (1903).
† *Comptes Rendus,* cxxxvi. p. 364 (1903).

the intense radiations ; for if such were the case, it would be expected that the activity produced on the different plates would vary not only in quantity, but also in quality. This result is confirmed by the observation that the active matter can be removed from a platinum plate by solution in sulphuric acid, and has other distinctive chemical and physical properties.

The variation of the residual activity with time will first be considered. A platinum plate was exposed in the presence of the radium emanation for seven days. The amount of emanation initially present was equal to that obtained from about 3 milligrams of pure radium bromide. The plate immediately after removal gave a saturation-current, measured between parallel plates by a galvanometer, of $1 \cdot 5 \times 10^{-7}$ ampere. For some hours after removal the activity decayed according to an exponential law with the time, falling to half value in 28 minutes. Three days after removal the active plate gave a saturation-current, measured by an electrometer, of 5×10^{-13} ampere ; *i. e.*, 1/300,000 of the initial activity. The activity was observed to increase steadily with the time.

Fig. 1.
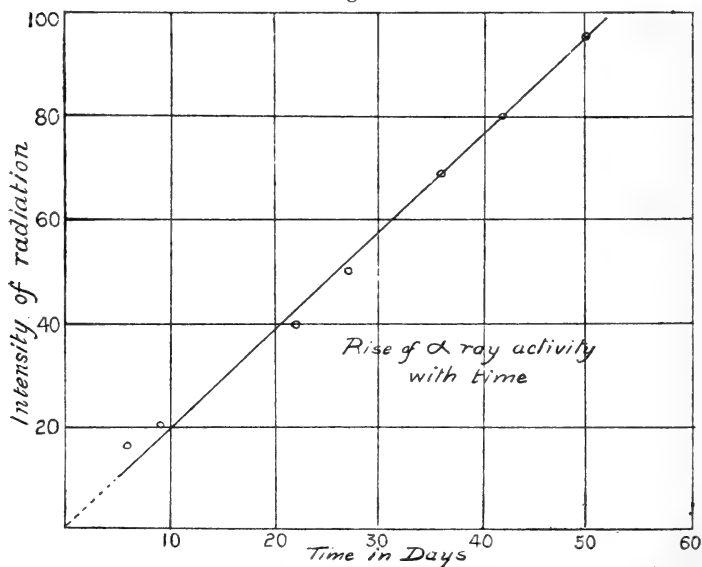
The results are shown in fig. 1, where the time is reckoned from the middle of the time of exposure to the emanation.

The curve is a straight line passing through the origin. The activity increases uniformly with the time for the

interval of two months over which the observations have extended.

Some results indicate that this steady increase with time continues for at least nine months. The emanation from 30 milligrams of radium bromide was condensed in a glass tube, which was then sealed. After a month's interval the tube was opened, and dilute sulphuric acid introduced. The acid dissolved the active residue deposited in the tube. On driving off the sulphuric acid by heat, a radioactive deposit was obtained. The first determination of the activity of this residue was made about six weeks after the introduction of the emanation. The activity eight months later was found to be about seven times the initial value. The results could not be very accurately obtained, as a portion of the activity had been removed in the interval by a bismuth rod placed in a solution of the active matter. The result, however, indicated that the activity had steadily increased over a period of nine months.

Radiations from the Active Matter.

The residual activity consists of both α and β rays, the latter being present initially in an unusually large proportion. The proportion of α to β rays from the platinum plate, one month after removal, was at the most one fiftieth of that from a thin film of radium bromide in radioactive equilibrium. Unlike the α ray activity, the activity measured by the β rays remains constant, and, in consequence, the proportion of α to β rays steadily increases with the time. The experiments showed that the intensity of the β rays did not vary much, if at all, over a period of nine months. The want of proportionality between the α and β rays shows that the two types of rays arise from different products. This conclusion is confirmed by experiments, now to be described, which show that the products giving rise to α and β rays can be temporarily separated from one another by physical and chemical means.

Effect of Temperature on the Activity.

An active platinum plate was exposed to varying temperatures in an electric furnace, and the activity tested after exposure at atmospheric temperature. Four minutes' exposure in the furnace, at first at $430°$ C., and afterwards at $800°$ C., had little, if any, effect on the activity. After four minutes at $1000°$ C. the activity decreased about 20 per cent., and a further exposure of eight minutes at a temperature of about $1050°$ C. almost completely removed the α ray activity.

The activity recovered by the β rays was, on the other hand, not appreciably changed by the high temperature. Further heating, however, at a still higher temperature, caused a decrease of the activity measured by the β rays, showing that the β ray product was also volatile. These results show that the active matter consists of two kinds. The part which emits β rays is non-volatile at 1000° C., but the other part which emits α rays is almost completely volatilized at that temperature.

Separation of the Constituents by means of a Bismuth Plate.

The active matter of slow decay was obtained in solution by introducing dilute sulphuric acid into a glass tube in which the emanation from 30 milligrams of radium bromide had been stored for a month. The solution showed strong activity and gave out both α and β rays, the latter, as in other cases, being present in an unusually large proportion.

When a polished bismuth disk was kept for some hours in the solution, it became strongly active. The active matter deposited on the bismuth gave out α rays, but no trace of β rays. After several bismuth disks had been successively left in the solution, the active matter, which emits α rays, was almost completely removed. This was shown by evaporating down the solution after treatment. The β ray activity remained unchanged, but the α ray activity had been reduced to about 10 per cent. of its original value. The active matter deposited in the bismuth does not appreciably change in activity in the course of one month, and some observations point to the conclusion that there is not much change in five months. The observations in the latter case were, however, not precise enough to be sure that there was not a small percentage variation during that time. Experiments are now in progress to examine, with accuracy, the activity of the bismuth plate from time to time, and it is hoped that observations extending over the ensuing year will fix the rate of decay of this product, provided the rate of change is rapid enough to be measurable in a year's interval. The results obtained in this way are in agreement with those deduced by heating the active deposit to a high temperature. The active deposit contains two kinds of matter, viz. :—

(1) A product giving out only β rays, which is soluble in sulphuric acid, but non-volatile at 1000° C., and which is not deposited on bismuth.

(2) A product giving out only α rays, which is soluble in sulphuric acid, volatile at 1000° C., and is deposited from a solution on bismuth.

Explanation of the Results.

We have seen that the α ray activity *increases* if the β ray product is present, but remains sensibly constant or changes very slowly if the α ray product is removed from the β ray product by the action of a bismuth plate. The β ray activity remains sensibly constant independently of whether the α ray product is present or not. These results show that the β ray product is the parent of the α ray product. The amount of residual activity from the radium emanation depends on the amount of emanation present and the time of exposure to the emanation. These results show that the active deposit of slow decay is a decomposition product of the emanation, and, since the first three transition products of the emanation, viz. radium A, radium B, and radium C, have been carefully analysed and shown to be consecutive, it is natural to suppose that the matter of slow rate of change is a product of the last rapid change in radium C.

Following the nomenclature suggested, radium C gives rise to the β ray product, which will be called radium D, while radium D changes into the α ray product, which will be called radium E. The product radium D gives out only β rays. The transition products of the disintegration of radium are shown diagrammatically in fig. 2.

Fig. 2.

RADIUM   RAD. EM.   RAD. A   RAD. B   RAD. C   RAD. D   RAD. E

ACTIVE DEPOSIT
OF
RAPID CHANGE

ACTIVE DEPOSIT
OF
SLOW CHANGE
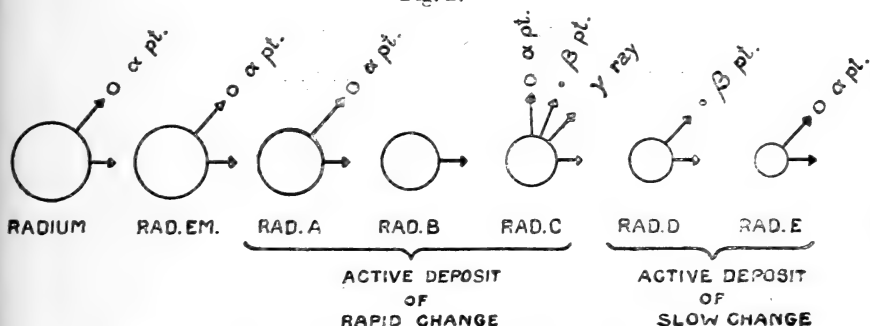
No further changes have so far been observed. The active solution of radium D and E was tested to see if an emanation were present. A trace of the radium emanation was always observed, but this was probably due to a slight trace of radium carried over into the emanation vessel. This point is, however, under further investigation.

Theory of two Successive Changes.

In all cases of radioactive change that have been examined, the amount of unchanged matter N_t present at any time t is

given by $\dfrac{N_t}{N_0} = e^{-\lambda t}$, where N_0 is the amount initially present, and λ is the constant of change. Differentiating, $\dfrac{dN_t}{dt} = -\lambda N_t$, or the rate of change is always proportional to the amount present.

Suppose that P_0 particles of the product radium **D** are deposited during the time of exposure to the emanation. This time is supposed to be so short that the amount of change of radium **D** during the time of exposure is very small.

> Let P = number of particles of matter radium **D** present at any time.
> Q = number of particles of radium **E** present at any time.
> λ_1 = constant of change of radium **D**.
> λ_2 = constant of change of radium **E**.

Then $P = P_0 e^{-\lambda_1 t}$.

As the matter **D** changes into **E**, the value of Q at first increases. The increase dQ in the time dt is given by the difference between the number of particles of **E** $(\lambda_1 P)$ supplied by the change of **D** into **E** and the number of **E** $(\lambda_2 Q)$ which change into **F**.

Then
$$dQ = \lambda_1 P\,dt - \lambda_2 Q\,dt,$$
and
$$\frac{dQ}{dt} = \lambda_1 P_0 e^{-\lambda_1 t} - \lambda_2 Q.$$

The solution of the equation is of the form
$$Q = a e^{-\lambda_1 t} + b e^{-\lambda_2 t}.$$

Since $Q = 0$ when $t = 0$, we have
$$a = -b = \frac{\lambda_1}{\lambda_2 - \lambda_1} \cdot P_0,$$
and
$$Q = \frac{\lambda_1 P_0}{\lambda_2 - \lambda_1} (e^{-\lambda_1 t} - e^{-\lambda_2 t}).$$

For small values of t, $Q = \lambda_1 P_0 \cdot t$; i. e., the value of Q increases proportionately with the time. The value of Q passes through a maximum at a time T, given by $e^{(\lambda_2 - \lambda_1)T} = \dfrac{\lambda_2}{\lambda_1}$, and then decreases with the time.

The initial increase of the α ray activity with the time is thus in agreement with the view that radium **E** (which emits only α rays) is produced from radium **D**. The time of observation (two months) has not yet been long enough to obtain

more than the initial part of the α ray curve. The results, however, show that an interval of two months is very short compared with the time required for the product D or E to be half transformed.

Although the times of observation have been too short to determine experimentally either the value of λ_1 or λ_2, it is possible, on certain assumptions, to form a rough estimate of these values.

It has been experimentally observed that each of the products of radium which emit α rays supplies about an equal proportion of the activity of radium when in radio-active equilibrium. Since, when equilibrium is reached, the same number of particles of each of the successive products must break up per second, this is an expression of the fact that every atom of each product breaks up with the expulsion of an equal number (probably one) of α particles. Now radium D is directly derived from radium C, and since the rate of change of D is very slow compared with that of C, the number of particles of D initially present must be very nearly equal to the number of particles of radium C which break up during the time that radium D is being formed. Now, suppose that each atom of radium C and D emits one β particle with the same velocity. The ionization produced by each particle will be the same under the same experimental conditions, and the integrated value of the saturation current due to the β rays over the time that the body is exposed to radium C must equal the corresponding integrated value for the β rays during the life of radium D. Suppose, for example, that a quantity of emanation is introduced into a glass tube and left to stand for a month. During that interval the emanation has nearly all been transformed. The activity due to the β rays from it will reach a maximum several hours after the introduction of the emanation, and will then decay with the time falling to half value in four days. Let i_1 be the maximum saturation current due to the β rays, measured in a suitable testing vessel. The total quantity Q_1 of electricity passing between the plates of the testing vessel during the life of the emanation is approximately given by

$$Q_1 = \int_0^\infty i\, dt = \int_0^\infty i_1 e^{-\lambda t} dt = \frac{i_1}{\lambda},$$

where λ is the constant of change of the emanation.

In a similar way, if i_2 is the initial current due to the β rays from the radium D deposited in the tube (measured

under identical experimental conditions) the corresponding value of $Q_2 = \dfrac{i_2}{\lambda_1}$ where λ_1 is the constant of change of D.

Since by hypothesis $\quad Q_1 = Q_2$,

$$\frac{\lambda_1}{\lambda} = \frac{i_2}{i_1}.$$

The ratio $\dfrac{i_2}{i_1}$ is determined experimentally, and since λ for the emanation is known, λ_1 is determined.

The details of the experiments by which the ratio i_2/i_1 was determined need not be given here. It was deduced on the above assumption, that half of the matter of radium D should be transformed in forty years. In a similar way, the total number of α particles expelled from radium C during the time radium D was being deposited must equal the number of α particles expelled from radium E during its life, supposing that there is only one change which gives rise to α rays. Assuming for the moment that the α ray activity, observed for the active deposit ten months old, decayed from that time according to an exponential law, it was calculated that the period of the change could not be longer than eighty years. If the α ray change has a period short compared with the β ray change, the α ray activity will finally decay at the same rate as the β ray activity. These two computations will agree, if it is supposed that the α ray activity increases to twice the value observed after an interval of ten months, and then decays with the time according to the period of the β ray change. This would fix the period of the α ray change at about one year. When the α ray activity reaches its maximum value, it is to be expected, on this view, that the ratio $\dfrac{\alpha}{\beta}$ should be the same as for the product radium C. This is, however, somewhat at variance with experiment; for the ten months old deposit has about the same ratio $\dfrac{\alpha}{\beta}$ as radium C, while on the above computation the ratio $\dfrac{\alpha}{\beta}$ should only be one half of that value. This difference may possibly be due to radium E undergoing a further change, giving rise to α rays, which has not so far been detected. In these calculations, it has been assumed that the α and β particles given out in these slow changes produce the same ionization as the corresponding particles from radium C. There is no doubt, however, that

this is not realized in practice. The β rays of radium D are slightly less penetrating than those of radium C, while the α rays of radium E have only about half the penetrating power of those of radium C. Our knowledge of the mechanism of absorption in matter is, however, too imperfect to correct for these differences with any certainty.

The above methods of calculation, though somewhat complicated, certainly serve to give the right order of magnitude of the periods of the two changes. It will be shown, too, that the calculated periods agree approximately with the amounts of radium D and E present in old samples of radium. The chief uncertainty in the methods of calculation lies in the difficulty of ascertaining the relative electrical effect produced by the α and β particles compared with those emitted from radium C.

The time T required for each transition product of radium to be half transformed is shown in the following table :—

Transition products of radium.	Time T to be half transformed.
RADIUM.	about 1000 years.
↓ EMANATION.	4 days.
↓ RADIUM A.	3 minutes.
↓ RADIUM B.	21 minutes.
↓ RADIUM C.	28 minutes.
↓ RADIUM D.	about 40 years.
↓ RADIUM E.	about one year.

Experiments with Old Radium.

Since the substance radium D is produced from radium at a constant rate, the amount present mixed with the radium will increase with its age. I had in my possession a small quantity of my first specimen of impure radium chloride, kindly presented to me by Professors Elster and Geitel four years ago. The amount of radium D present in it was tested in the following way :—The substance was dissolved in water and kept continuously boiling for a period of about six hours. Under these conditions the emanation is removed as rapidly as it is formed, and the β rays from the radium, due to the product radium C, practically disappear. A newly prepared specimen of radium bromide under these conditions retains only a fraction of 1 per cent. of its original β radiation.

The old radium, however, showed (immediately after this treatment) an activity measured by the β rays of about 8 per cent. of its original amount. The activity could not be reduced any lower by further boiling or aspiration of air through the solution. This residual β ray activity was due to the product radium D stored up in the radium. It could not have been due to β rays from radium C, since there was a distinct difference in penetrating power for the two kinds of β rays. The β ray activity due to radium D was thus about 9 per cent. of that due to radium C. Disregarding the differences in the absorption of the β rays, when the activity of the product D in radium reaches a maximum value, the β ray activity due to it should be the same as that due to C. Since D is half transformed in forty years, the amount present in the radium after four years should be about 7 per cent. of the maximum amount; *i. e.*, it should show a β ray activity of about 7 per cent. of that due to radium C. The observed and calculated values (7 and 9 per cent. respectively) are thus of the same order of magnitude. The amount of β rays from radium D present in pure radium bromide about one year old was about 2 per cent. of the total.

The amount of radium E present in old radium was measured by observations of the activity imparted to a bismuth disk left for several days in the solution. Radium E is not deposited to an appreciable extent on the bismuth from a water solution of radium bromide. If, however, a trace of sulphuric acid is added to the solution, the radium E is readily deposited on the bismuth. The addition of sulphuric acid to the radium solution practically effected a separation of radium D and E from the radium proper; for the latter was precipitated as sulphate and the products D and E remained in solution. After filtering, the solution contained a greater proportion of the products D and E and very little radium.

The ratio α/β for the old radium was found to be about twice that observed for radium C. This result is in agreement with the deductions made in the calculations of the periods of the changes; for it can be theoretically shown that the amounts of D and E in the radium continue to be approximately proportional to one another after five years' production, assuming the periods of the changes are forty years and one year respectively.

The amounts of radium D and E observed in the old radium are thus in good agreement with the results deduced from other data.

Variation of the Activity of Radium with Time.

It has long been known that the activity of freshly prepared radium increases at first with the time and reaches a maximum value after an interval of about one month. The results already considered show that there is a further slow increase of activity with the time. This is the case whether the activity is measured by the α or β rays. After a lapse of about 200 years the amount of the products radium D and E will have practically reached a maximum value. The same number of atoms of each of the products C and D will then break up per second. If each atom of these products in disintegrating throws off an equal number (probably one) of β particles, the number of β particles thrown off per second will be twice as great as from radium a few months old. The number will increase at first at the rate of about 2 per cent. a year.

Similar considerations apply to the α ray activity. Since, however, there are four other products of radium besides radium itself which expel α particles, the number of α particles emitted per second from old radium will not be more than 25 per cent. greater than the number from radium a few months old. The activity measured by the α rays will thus not increase more than 25 per cent., and probably still less, as the α particles from radium E probably produce less ionization than the α particles expelled from the other radium products. It is probable that half of the radium itself is transformed in about 1000 years. The activity of radium will consequently rise to a maximum after 200 years and then slowly die away with the time.

Products in Pitchblende.

The products radium D and E must be present in pitchblende in amounts proportional to the quantity of radium present, and should be capable of separation from the mineral by suitable chemical methods. The radioactive properties of these substances, if obtained in the pure state, is summarized below.

Radium D.—The product immediately after separation should emit only β (and probably γ) rays. The β ray activity should decay to half value in about forty years. In consequence of the change of D into E, the latter of which gives out α rays, the α ray activity will increase for a few years, pass through a maximum, and then decrease with the time and fall to half value in about forty years. Since the rate of change of D is about 25 times as fast as radium itself,

the activity on separation, measured by the number of electrons expelled per second, should be 25 times as great as from an equal weight of radium. The α ray activity produced in it should at any time be capable of separation by adding a bismuth plate to a solution of the substance.

Radium E.—The substance should emit only α rays, and its activity should fall to half value in about one year. Since its rate of change is about 1000 times as great as radium, the substance, weight for weight, should emit about 1000 times as many α particles as freshly prepared radium, and about 250 times as many as from radium about one month old. The activity measured by the electric method will probably be about 100 times as great as that of pure radium.

It is now necessary to consider the question whether the substances radium D and E have been previously separated from pitchblende, and are known by other names. In regard to radium D, there is some doubt whether it has been previously separated. It is possible that it is the radioactive constituent present in the radio-lead of Hoffmann, for he states that this substance emits a large amount of β rays. On the other hand, the radio-lead prepared by other observers lost its activity rapidly with the time.

In regard to radium E, I think there is little doubt that it is the radioactive constituent present in the so-called radio-tellurium of Marckwald *. It will be recalled that Marckwald obtained a deposit of radioactive matter on a bismuth plate introduced into a solution of pitchblende. This active bismuth gave out only α rays. The active matter was associated with tellurium and was, in consequence, called radio-tellurium. In later observations Marckwald has shown that tellurium is merely an impurity, and has devised a method of concentrating the radioactive matter.

The radioactive constituent in radio-tellurium and that in radium E are very analogous in chemical and radioactive properties, for both emit α rays, and both are deposited on a bismuth plate introduced into the active solution. In addition, I have found that the α rays from the two substances are identical in their power of penetration through aluminium. The radio-tellurium was obtained from Dr. Sthamer of Hamburg, in the form of a thin film of the active matter deposited on the surface of a polished bismuth plate. I was unable to detect any difference in the penetrating power of the rays from radium E and radio-tellurium, although the intensity of the radiation was reduced to a few per cent. by

* *Berichte d. D. Chem. Ges.* p. 2285 (1902).

aluminium screens. It is well known that the α rays from most of the radioactive products differ in penetrating power, and the identity of the α rays of radium E and radio-tellurium in this respect is a strong indication that the radioactive matter is the same in both cases. The agreement of the absorption of the rays, together with the similarity of the radiations and chemical behaviour, affords very strong evidence in favour of the identity of the two products. If this is the case the activity of the radio-tellurium must decay to half value in about one year.

It is natural here to consider the question whether the product radium E is also identical with the polonium discovered by Mme. Curie. Each acting constituent attaches itself to bismuth and emits only α rays. The test of pene-trating power of the rays cannot be applied, since the polonium, as commercially sold, is usually mixed with bismuth and not, as in the case of Marckwald's radio-tellurium, deposited on a thin film on the surface. The activity observed at the surface is thus due to α rays, which have already decreased in penetrating power due to their passage through different thicknesses of bismuth. The α rays from polonium in consequence appear to be more readily absorbed than those from radium E, but it is not unlikely that the difference observed may be due entirely to the different experimental conditions in the two cases. Mme. Curie states that the activity of polonium decays slowly with the time, and mentions that one specimen lost half its activity in eleven months. The rate of decay of activity is about the same as that deduced from the product radium E. Mme. Curie, in addition, observed that an active product could be separated from radium itself by precipitating bismuth added to the radium solution. This active matter present with the bismuth could be concentrated in exactly the same way as was employed for the polonium obtained directly from a solution of pitch-blende. Giesel long ago observed that bismuth was made active when placed in a radium solution, and considered that polonium was in reality "induced bismuth." The product removed on the bismuth must in both cases have been radium E, so that we have here direct evidence that polonium and radium E exhibit similar chemical properties.

In the original paper giving an account of the discovery of polonium, Mme. Curie states that the active matter could be concentrated to some extent by heating the active sulphide in a vacuum. The active sulphide was more volatile than the bismuth, and was deposited on portions of the glass tube between 300° C. and 350° C. On the other hand, I have

shown that radium E deposited on a platinum plate is not appreciably volatile until a temperature of nearly 1000° C. It is intended to examine this point still further in order to see if this difference of behaviour is only apparent or real.

The experiments as a whole are, I think, best explained on the view that polonium and radio-tellurium both contain the same radioactive constituent, which is the fifth disintegration product of radium. The most definite method of settling the matter is to compare the rates of decay of the activity of the three active substances, and experiments of this character are already in progress.

It would be of scientific value to isolate the product radium D from pitchblende, for in many respects it would be as useful scientifically as radium itself. Its activity in the pure state, measured by the β rays, would be about 25 times that of radium, and the rate of change of its activity is sufficiently slow to be neglegible in most experiments.

Experiments are in progress to see whether a simple method can be found for separation of radium D from pitchblende*.

McGill University, Montreal,
    Aug. 25, 1904.

LXIV. *On Saturation Currents in Ionization.* By GEORGE W. WALKER, *M.A., A.R.C.Sc., Fellow of Trinity College, Cambridge, Lecturer on Physics in the University of Glasgow*†.

THE theory and measurement of saturation currents forms the basis of much important work on the action of ionizing agents.

The fact that, when a current passes through an ionized gas between two plates, a stage is reached when a considerable increase of the potential-difference between the plates gives practically no increase of current, is explained on the simple

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* Mr. B. Boltwood, of New Haven, Conn., very kindly forwarded me, a few days ago, a specimen of radioactive lead, which he had separated from pitchblende four months before. This lead gave out an unusually large proportion of β compared with α rays, and the total amount of β rays from it was about the same as from the uranium present in the pitchblende from which it was separated. On dissolving the lead, some of the α ray activity was removed on a bismuth plate. I think that it is probable that the lead contains the product radium D. These results suggest that Hoffmann, whose earlier work on radio-lead was subjected to much criticism, was probably right in believing that he had separated a new radioactive constituent with the lead, the activity of which did not die away with the time.

   † Communicated by the Author.

theory of ionization by saying that all the ions reach the plates before they have had time to recombine. This view, although it agrees with the general feature, does not explain the numerous cases where the current continues to increase, although slowly, even when considerable potential-differences are used. There are cases in which it is unlikely that this can be ascribed to the action of the field in producing new ions in the gas, or in altering the rate of recombination.

Now if " saturation," practical or absolute, is obtained, the general equations † which represent the motion of ions, and which appear to have a wide range of applicability, ought to throw some light on this question ; but I have not seen any formal proof from the equations that " saturation " must be reached under given conditions.

For this purpose it is necessary to obtain exact solutions of the equations. In the general case these are too complicated to deal with ; but it seemed to me that consideration of even a particular case in which the equations could be integrated might reveal the conditions under which saturation could be obtained.

Let us take two infinite parallel plates at distances $2l$ apart, and at potentials $+V$ and $-V$.

Let n_1 = number of positive ions per unit volume at any point.

$\quad n_2$ =   ,,   negative ,,   ,,   ,,   ,,

$\quad e$ = charge of an ion.

$\quad R$ = group velocity of the ions per unit electric force, supposed to be the same for both positive and negative ions.

$\quad q$ = rate of production of ions per unit volume.

$\quad \alpha$ = coefficient of recombination.

$\quad X$ = electrical force at any point.

Then, if we measure x perpendicularly to the plates and take the origin as the point midway between the plates, we get

$$\frac{d\mathrm{X}}{dx} = 4\pi e(n_1 - n_2), \quad \ldots \ldots \quad (1)$$

$$\frac{dn_1\mathrm{X}}{dx} = \frac{1}{\mathrm{R}}(q - \alpha n_1 n_2), \quad \ldots \ldots \quad (2)$$

$$\frac{dn_2\mathrm{X}}{dx} = -\frac{1}{\mathrm{R}}(q - \alpha n_1 n_2). \quad \ldots \ldots \quad (3)$$

In these we suppose q, α, and R to be constant throughout the field.

* See J. J. Thomson, 'Conduction of Electricity through Gases,' ch. iii.

As an integral of these we readily obtain

$$(n_1 + n_2)e\mathrm{R}X = i, \quad \ldots \ldots \quad (4)$$

where i is a constant and is the current through the gas per unit area.

We also get

$$\frac{q}{4\pi e} + 2\mathrm{R}\left(1 - \frac{\alpha}{8\pi e\mathrm{R}}\right)n_1 n_2 = \mathrm{C}X^{2\left(\frac{\alpha}{8\pi e\mathrm{R}} - 1\right)} \quad . \quad . \quad (5)$$

where C is a constant.

Putting $X^2 = y$ and eliminating n_1 and n_2, we get

$$\frac{\mathrm{R}^2}{64\pi^2}\left(\frac{dy}{dx}\right)^2 = i^2 + \frac{qe\mathrm{R}}{2\pi\left(1 - \frac{\alpha}{8\pi e\mathrm{R}}\right)}y - \frac{2\mathrm{C}e^2\mathrm{R}}{1 - \frac{\alpha}{8\pi e\mathrm{R}}}y^{\frac{\alpha}{8\pi e\mathrm{R}}} \quad . \quad . \quad (6)$$

Now from symmetry $\dfrac{dy}{dx} = 0$ at $x = 0$; so that if $y = y_0$ at $x = 0$ we get

$$i^2 = \frac{2\mathrm{C}e^2\mathrm{R}}{1 - \frac{\alpha}{8\pi e\mathrm{R}}}y_0^{\frac{\alpha}{8\pi e\mathrm{R}}} - \frac{qe\mathrm{R}}{2\pi\left(1 - \frac{\alpha}{8\pi e\mathrm{R}}\right)}y_0, \quad \quad (7)$$

and

$$\frac{\mathrm{R}^2}{64\pi^2}\left(\frac{dy}{dx}\right)^2 = \frac{qe\mathrm{R}}{2\pi\left(1 - \frac{\alpha}{8\pi e\mathrm{R}}\right)}(y - y_0) - \frac{2\mathrm{C}e^2\mathrm{R}}{\left(1 - \frac{\alpha}{8\pi e\mathrm{R}}\right)}\left(y^{\frac{\alpha}{8\pi e\mathrm{R}}} - y_0^{\frac{\alpha}{8\pi e\mathrm{R}}}\right). \quad (8)$$

By giving $\dfrac{\alpha}{8\pi e\mathrm{R}}$ suitable values, (8) can be integrated without much difficulty. I selected $\dfrac{\alpha}{8\pi e\mathrm{R}} = 2$ as the easiest.

If the laws hold so far this would correspond to ordinary air at 8 atmospheres pressure, so that the case could be obtained experimentally.

It is unnecessary to suppose that $n_1 n_2$ vanishes at the plates. Various circumstances might prevent this, such as a small supply of ions from the plates themselves, or the presence of a thin layer of radioactive material. We shall therefore suppose that, at $x = \pm l$,

$$n_1 n_2 = \frac{\theta}{8\pi e\mathrm{R}} \quad \text{or} \quad \theta = \frac{\alpha}{2}(n_1 n_2)_l.$$

We thus get

$$i^2 = \frac{qe\mathrm{R}}{2\pi} y_0 \left\{ 1 - \left(1 - \frac{\theta}{q}\right)\frac{y_0}{y_1} \right\}, \quad \cdots \quad (9)$$

and

$$\left(\frac{dy}{dx}\right)^2 = \frac{32\pi qe}{\mathrm{R}y_1}\left\{\left(1-\frac{\theta}{q}\right)y + \left(1-\frac{\theta}{q}\right)y_0 - y_1\right\}\left\{y - y_0\right\} (10)$$

where $y = y_1$ at $x = l$.

The solution of (10) is, if $\dfrac{\theta}{q} < 1$,

$$y = -\frac{y_1}{2\left(1-\frac{\theta}{q}\right)} \frac{\left\{\left(1-\frac{2\theta}{q}\right)\cosh \mu x + \cosh \mu l\right\}}{\cosh \mu l} \quad . \quad (11)$$

where

$$y_0 = -\frac{y_1\left\{\left(1-\frac{\theta}{q}\right) - \frac{1}{2} + \frac{1}{2}\cosh \mu l\right\}}{\left(1-\frac{\theta}{q}\right)\cosh \mu l}$$

and

$$\mu^2 = \frac{32\pi qe}{\mathrm{R}y_1}\left(1-\frac{\theta}{q}\right) = \frac{32\pi qe}{\mathrm{R}X_1^2}\left(1-\frac{\theta}{q}\right).$$

Using these values in (9) we get

$$i = 2qel\frac{1}{\mu l}\frac{\left\{\cosh^2 \mu l - \left(1-\frac{2\theta}{q}\right)^2\right\}^{\frac{1}{2}}}{\cosh \mu l}. \quad \cdots \quad (12)$$

The potential is given by

$$\mathrm{V} = \left\{\frac{y_1}{2\left(1-\frac{\theta}{q}\right)\cosh \mu l}\right\}^{\frac{1}{2}} \int_0^l \left\{\left(1-\frac{2\theta}{q}\right)\cosh \mu x + \cosh \mu l\right\}^{\frac{1}{2}} dx. \quad (13)$$

The integration can be completed by means of elliptic functions, but not in a form very suitable for numerical purposes.

We may note in (12) that $2qel$ is the value of the saturation current on the simple theory. We shall denote by f the function

$$\frac{1}{\mu l}\frac{\left\{\cosh^2 \mu l - \left(1-\frac{2\theta}{q}\right)^2\right\}^{\frac{1}{2}}}{\cosh \mu l}$$

and shall first take $\dfrac{2\theta}{q}$ as very small. The following numerical table indicates the general character of the functions. They

are also shown in the curves. In the table the values of $V/X_1 l$ were obtained from suitably convergent expansions in terms of μl. I found that for the small values of $\dfrac{2\theta}{q}$ selected, no appreciable difference in the values of $V/X_1 l$ resulted.

μl	$\dfrac{1}{\mu l} = \dfrac{X_1}{l\sqrt{\frac{32\pi qe}{R}}}$	$\dfrac{V}{X_1 l}$	Diff. of Potential between Plates in Volts. Case (1).	Case (2).	Values of f. $\dfrac{2\theta}{q}=0.$	$\dfrac{2\theta}{q}=10^{-4}.$	$\dfrac{2\theta}{q}=10^{-3}.$	$\dfrac{2\theta}{q}=10^{-2}.$
0	∞	1	144	…	1	∞	∞	∞
·001	1000	·9999..	14·4	14400	·99999	14·17	44·7	141·0
·01	100	·9999..	1·44	1440	·99996	1·720	4·58	14·14
·1	10	·9992	·72	720	·996	1·01	1·09	1·714
·2	5	·996	·28	280	·986	·988	1·01	1·204
·5	2	·979	·13	130	·923	·926	·927	·957
1	1	·938	·055	55	·7608	·761	·762	·766
2	·5	·85	…	…	·4809	·4809	·481	·482
5	·2	·76	…	…	·200	·200	·200	·200
10	·1	·74	…	…	·100	·100	·100	·100
∞	0	·707	…	…	0	0	0	0

In case (1) I have taken $q=100$ per sec., and $R=60$ cms. per sec. for 1 electrostatic unit, these being probable values at 8 atmospheres for spontaneous ionization in air.
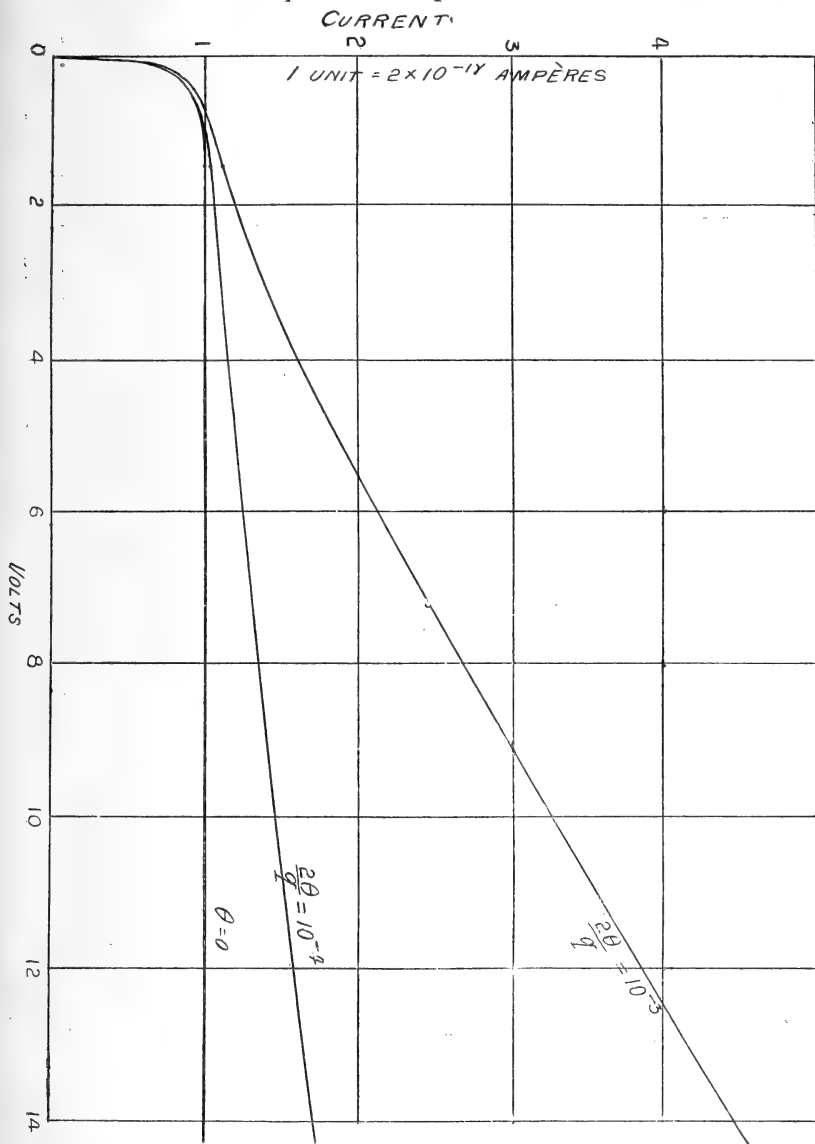
Fig. 1.

$e=3·5 \times 10^{-10}$ electrostatic units, and $l=1$ cm. The corresponding currents in amperes will be got by multiplying the values of f by 2×10^{-17}. (Fig. 1.)

2 Z 2

In case (2), using the same value of R, I took $q = 10^8$ which is about the value due to radium chloride of moderate strength. The corresponding values of the current in amperes are got by multiplying the values of f by 2×10^{-11}. The values of f show that if no ions are supplied by the plates, theoretical saturation is only obtained at an infinite P.D. Nevertheless the current attains to 99 per cent of the saturation value for ·72 volt between the plates when $q = 100$, and for 720 volts between the plates when $q = 10^8$. (Fig. 2.)

The effect of supposing that θ is not zero is very interesting. If $\dfrac{2\theta}{q}$ is small and constant as X_1 is varied, the initial portion of the curve showing the relation between P.D. and current is not sensibly altered, but instead of attaining saturation the current continues to increase as the P.D. increases. For very small values of X_1 we get from (12) and (13)

$$i = 2qeX_1 \left(\frac{R}{32\pi qe} \right)^{\frac{1}{2}} = \frac{1}{2} \left(\frac{qRe}{2\pi} \right)^{\frac{1}{2}} X_1$$

and

$$2V = 1 \cdot 414 X_1 \cdot l;$$

while for very large values of X_1 we get

$$i = 2qe \cdot \frac{2\theta^{\frac{1}{2}}}{q^{\frac{1}{2}}} \cdot X_1 \left(\frac{R}{32\pi qe} \right)^{\frac{1}{2}} = \left(\frac{\theta Re}{2\pi} \right)^{\frac{1}{2}} X_1$$

and

$$2V = X_1 \cdot l.$$

Thus for very large and very small P.D.'s the current is proportional to the P.D., but the slope of the curve showing the relation between current and P.D. at the higher voltages is $2 \left(\dfrac{\theta}{q} \right)^{\frac{1}{2}}$ of the slope for small voltages. We may further note that, other things being equal, the value X_1 required for a given fraction of the true saturation current is proportional to l.

The general agreement between these results and experiments on ionization, e. g. in flames [*], is exceedingly close, although of course the pressure in the case we are considering is much greater than in the experiments.

We have seen that $\theta = \dfrac{\alpha}{2} (n_1 n_2)_l$, so that $\dfrac{2\theta}{q} = \dfrac{\alpha (n_1 n_2)_l}{q}$. In order that θ should not be zero, positive ions must be supplied at the positive plate, and negative ions at the negative plate, by the plates themselves at a rate $(n_2)_l RX_1$.

[*] See Thomson, 'Conduction of Electricity,' pp. 197–203.

The quantity θ is constant for the purpose of integrating the equations, but the supply of ions by the plates may be
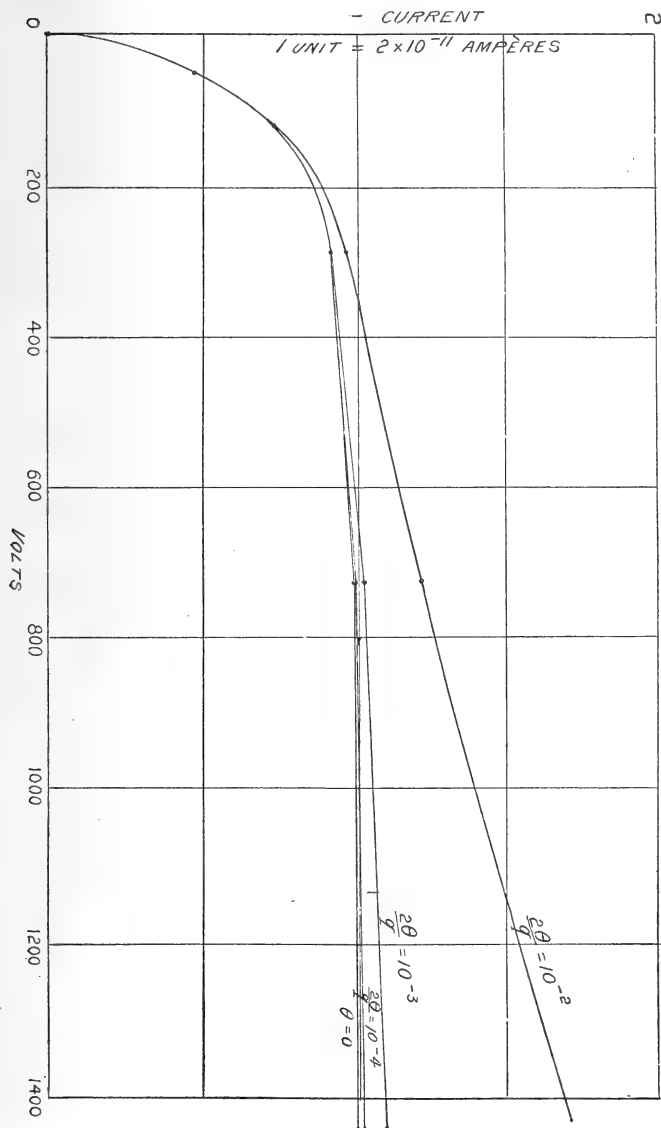
Fig. 2.

such that θ depends on the other constants of integration. In particular, it may depend on the value of X_1 at the plates. For instance, if the ions are supplied at a constant rate, then

for large values of X_1 both $(n_1)_l$ and $(n_2)_l$ vary inversely as X_1, so that θ will vary inversely as X_1^2 and we shall have a true saturation current. But it is conceivable that the supply may increase with X_1, so as to make θ constant, or even increase with X_1; and, as we have seen, even small values of $\frac{2\theta}{q}$ suffice to produce very profound modification of the curve *.

We have taken $\frac{\theta}{q}$ as small and less than unity, but cases may arise in which this is not so. If $\frac{\theta}{q}$ becomes greater than unity the solution of (10) has to be expressed in ordinary circular functions instead of hyperbolic functions. I do not propose to discuss this at present.

Although I have selected the case $\frac{\alpha}{8\pi e R} = 2$ for discussion, there are several other cases in which the integration can be effected, *e. g.* $\frac{\alpha}{8\pi e R} = \frac{1}{2}, \frac{1}{3}, \frac{1}{4}$.

I have not had time to examine these very completely, but as far as I have gone they indicate the same general features as in the case $\frac{\alpha}{8\pi e R} = 2$. Although the equations probably break down at exceedingly low pressures, we may take $\frac{\alpha}{8\pi e R}$ moderately small and obtain a solution in series. For large values of X_0, the force at $x = 0$, I find that approximately

$$i^2 = \frac{qeR}{2\pi}\left\{ \frac{\theta}{q} X_0^2 + \frac{8\pi qe}{R} l^2 - \frac{4}{3}\left(\frac{8\pi qe}{R}\right)^2 \frac{\alpha}{8\pi e R}\frac{l^4}{X_0^2}\right\}$$

and $2V = 2lX_0\left\{ 1 + \frac{1}{6}\frac{8\pi qe}{R}\frac{l^2}{X_0^2}\right\}.$

This indicates the same kind of effect, viz., if θ is zero the saturation current is $2qel$, while if θ is not zero a complete modification of the curve for large voltages may be produced.

These considerations show that the supposition of even a small supply of ions from the plates readily explains the observed facts. At the same time it raises a somewhat serious difficulty as to the interpretation to be put on the results

* Although $\frac{2\theta}{q}$ is small, it is obvious that when the current exceeds $2qel$, the rate of supply by the plates must make up the excess.

in cases where experiment indicates that true saturation is not obtained. For in order to settle the value of q and θ we require to know the form of the curve for a considerable range of voltages, and this is particularly difficult in cases of intense ionization. In other words, the failure to obtain saturation may be due to a small value of θ, in which case we are beyond the true saturation current, or it may be that θ is negligible and that the voltage is insufficient for saturation.

No doubt actual experiments will be complicated by inequality of the velocities of the positive and negative ions and of the rate of supply of these at positive and negative plates. This, however, only increases the difficulty I have mentioned.

A comparison of the curves I have drawn with actual experiments where saturation is not obtained, shows clearly the uncertainty thus introduced in estimating for instance q. The point seems to me one which merits considerable attention.

There are some other cases in which the equations can be integrated : for instance, when the ions are supplied entirely by the plates, and the positive and negative ions move with unequal velocities. I hope to consider some of these cases soon.

<div style="margin-left:2em">Physical Laboratory,
The University, Glasgow,
22nd Aug. 1904.</div>

LXV. *Notices respecting New Books.*

Radioactivity. By FREDK. SODDY, *M.A.* (The Electrician Printing and Publishing Co. Ltd.)

THE account which Professor Soddy gives in this book of the development of the science of Radioactivity up to the commencement of the present year is clear in style and full in treatment. Details of experimental work are given in many places ; a feature which will be appreciated by the many who, fascinated by the interest of the subject, are endeavouring to pursue investigations for themselves. We feel indeed that we would have been thankful to Professor Soddy had he given yet more practical details. All workers in this field should for instance be repeatedly warned against the risk of error arising from the subtle dispersal of emanation, or the obstinately persistent radioactivity of vessels or instruments once brought into contact with radium salts.

Nothing can more strikingly mark the rapid progress of this branch of Science than to note how much has been done since the appearance of so recent a work. Thus Paschen's investigations into the nature of the γ rays seem to suggest that in these rays we are dealing with something quite different from the X rays and,

relatively to the other rays from radium, of much more importance than was held hitherto to be the case. Again, we find throughout Professor Soddy's book the a particle spoken of as having the dimensions of the material atom, with nothing to suggest the quite different view put forward by Mr. Harold Wilson during the present year.

This, in common with other works dealing with the rapidly advancing science of Radioactivity, will require frequent re-editing. A solid and valuable body of experimental work, with which new views must ultimately be brought into harmony, is, however, carefully and ably described in this volume. J. J.

Astronomical and Historical Chronology in the battle of the Centuries. By WILLIAM LEIGHTON JORDAN, *F.R.G.S., F.S.S., &c.* (Longmans, Green & Co.)

FEW questions seem so simple and yet really are so complicated as questions of chronology. And here we are not alluding to the many different eras which have been used in different parts of the world and in different periods of the world's history, from which to express the date of any event we desire to chronicle or discuss. In all Christian countries for many centuries past (not, be it remembered, during the four or five immediately following that event) the birth of Christ, or the date at which it was supposed to have taken place, was the initial point from which all other events were dated, either before or after it, necessitating the use of the symbols B.C. for before, and A.D. for after the Nativity. It is now generally recognized that when this era was first introduced a mistake was made with regard to the year in which it took place, and that our Lord was really born probably in B.C. 5. That, however, is not the point which is especially discussed in the volume before us.

The assumed date of Christ's birth is generally supposed to have been about the end of the year which we now reckon as B.C. 1 (the interval between the recognized Christmas Day and the end of the year as settled by the Julian Calendar is too short to be worth taking into account), so that A.D. 1 was then commenced and completed on December 31 of that year. According to this one century was completed on Dec. 31, A.D. 100, and nineteen centuries on Dec. 31, A.D. 1900. The writer of this notice well remembers a letter being written from some unknown correspondent to the late Sir George Airy, then Astronomer-Royal, asking him to resolve a dispute when the twentieth century would commence, and the answer was that "a very little thought" would show that it would commence on Jan. 1, 1901. The book now before us takes a different view, and the Author does not forget to remind us that the German Emperor some time ago put forth a dictum that the twentieth century began on Jan. 1, 1900.

Mr. Jordan, recalling the fact that those medieval writers who first used Christian chronology reckoned not from the Birth,

but from the Incarnation, of Christ (*i. e.* the Annunciation, commonly called Lady Day preceding the Nativity), contends that the years should be taken as ordinal, not cardinal, and that the year usually called B.C. 1, should be considered to be the *first* year from the initial point of reckoning, so that on its completion one year had elapsed. Scientific or astronomical chronologists, when making calculations, are compelled to do this virtually by calling the year 0 which is ordinarily denominated B.C. 1, and when they have finished their calculations, restore the B.C. dates to those in the usual system, by which the year preceding A.D. 1 is B.C. 1, without an intermediate year 0, as numerical succession requires. This is why Mr. Jordan adopts for his title ' Astronomical and Historical Chronology,' which he thinks should be the same, and he dedicates his book to the librarians of Florence and Pisa, in the hope that they may find in medieval Italian literature further evidence of the correctness of his view with regard to the early usage of Christian chronology in this way. Although Dionysius Exiguus is said to have been a native of Scythia, and was probably Greek by nationality, he did his work at Rome (where he died about the middle of the sixth century), and his system of chronology was first used in Italy.

As might be expected, in the course of his elaborate discussion of the question (taken up, we may mention, at the commencement, according to his view, of the present century) the Author furnishes us with much subsidiary information with regard to the application of the Metonic Cycle and other chronological data. W. T. L.

Die Telegraphie Ohne Draht. *Von* AUGUSTO RIGHI *und* BERNHARD DESSAU. Mit 258 eingedruckten Abbildungen. Braunschweig: F. Vieweg und Sohn. 1903. Pp. xi + 481.

WE had occasion recently to review this work in its Italian edition, and as the German issue of it is an exact replica of the Italian work, there is little need to do more than merely draw the attention of our readers to the existence of the German version. Like all Messrs. Vieweg's publications, it is printed and illustrated in the best style. Many of our readers unacquainted with Italian will, no doubt, be glad to know that a German version is available.

Jelineks Psychrometer-Tafeln. *Erweitert und vermehrt von* J. HANN. *Neu herausgegeben und mit Hygrometer-Tafeln versehen von* J. M. PERNTER. Fünfte erweiterte Auflage. Leipzig: W. Engelmann. 1903. Pp. xiii + 108.

THIS enlarged set of hygrometric tables should prove very useful to meteorologists. The general principles of the wet and dry bulb hygrometer, and of de Saussure's hair hygrometer, are dealt with in the introduction, in which the method of using the various tables is also clearly explained, and illustrated by several examples worked out in detail. Numerous references are given to the literature of the subject.

A Text-Book of Organic Chemistry. By WILLIAM A. NOYES, *Professor of Chemistry in the Rose Polytechnic Institute.* Henry Holt & Company, New York. 1903. Pp. xvii + 534.

THE task of writing a satisfactory elementary text-book of organic chemistry is, in view of the extraordinary rapid accumulation of new facts in that domain of science, an extremely difficult one, and we must confess that in our opinion the author has succeeded in accomplishing it admirably. The introductory two chapters, on physical measurements, are particularly good. In the general arrangement of the subject-matter, the book differs from others of a similar kind in the rejection of the old division into "fatty" and "aromatic" compounds. A very useful feature is the list of laboratory exercises given at the end of each chapter.

La Moderna Teoria dei Fenomeni Fisici (Radioattività, Ioni, Elettroni). Di AUGUSTO RIGHI. Bologna: Nicola Zanichelli. 1904. (Attualità Scientifiche—No. 3.) Pp. viii + 135.

PROFESSOR RIGHI is to be congratulated on having produced so fascinating an account of the new phase in physical science brought about by the introduction of the electronic theory. Not only the serious scientific worker, but the general reader will find this little volume delightful reading. In non-technical yet strictly scientific language, the author traces the gradual growth of the electronic idea, and describes the remarkable series of experimental researches which have gradually led to the general adoption of the electronic theory. Electrolytic ions and electrons; electrons and luminous phenomena; nature of cathode rays; ions in gases and solids; radioactivity; mass, velocity, and charge of ions and electrons; electrons and the constitution of matter—such is the list of subjects dealt with by Prof. Righi in his interesting little book, the value of which to the serious student will be enhanced by the complete bibliography given at the end of the book.

Essais Industriels des Machines Electriques et des Groupes Electrogènes. Par F. LOPPÉ. Paris: Gauthier-Villars. 1904. Pp. 283.

THIS work is a fine specimen of that lucidity and elegance of style for which the best writers of French text-books are noted. The testing of electrical machinery and of "generating sets" has become so important a matter for the modern engineer that a book specially devoted to this subject is sure to appeal to a very wide circle of readers; and in the work under review its distinguished author has succeeded in giving an interesting and useful summary of the large amount of experimental work carried out within recent years in connexion with the testing of electrical machinery.

It is now well known that many of the earlier methods of dynamo and motor testing, and of separating the various losses, were based on assumptions which subsequent researches have shown to be untenable. A clear idea of the nature of the assumptions underlying any particular test is of extreme importance to the engineer, and in M. Loppé's book he will find a systematic and critical account of all the best-known methods relating to the testing of

continuous-current machines, alternators, and direct-coupled generating sets, and a useful chapter on the methods approved by the American Institute of Electrical Engineers, the Verband Deutscher Elektrotechniker, and l'Association Française des Propriétaires d'Appareils à Vapeur. The Appendix contains various specimen test-sheets and tables.

In some respects the book is not quite up-to-date, and the sentence at the foot of p. 44 regarding the calorimetric method of determining efficiencies, regarding which the author says that " en pratique cette méthode est inapplicable," is a strange commentary on Mr. Threlfall's recent paper on " The Testing of Electric Generators by Air Calorimetry," read before the Institution of Electrical Engineers. The names of English engineers are frequently misspelt ; thus, we read of " M. Swenburn," " Sylvanus Thomson," &c. These, however, are only minor blemishes in a really useful book.

Traité d'Analyse des Substances Minérales. Par ADOLPHE CARNOT, *Membre de l'Institut.* Tome Second : Métalloïdes. Paris : Ch. Dunod. 1904. Pp. 822.

THIS is the second volume of a comprehensive treatise on the analysis of mineral substances, and deals with metalloids ; this term has, however, to be given a somewhat elastic interpretation, the author including within the present volume titanium, tungsten, molybdenum, vanadium, &c. The method of treatment, which is uniform throughout, consists in giving the occurrence in nature and properties of the element under consideration, and of its more important compounds ; then follows a detailed and careful study of the most useful and reliable methods for detecting and quantitatively estimating the element, and for separating it from elements previously considered.

A very important and highly commendable feature of the book is the amount of attention given to matters of technical importance to engineers, agriculturists, and others engaged in various branches of industry in which some knowledge of chemistry and chemical analysis is essential. As an example of the thoroughness which the author brings to bear on his task, we may mention that in the section devoted to carbon the various methods of determining the calorific values of solid, liquid, and gaseous fuels are fully considered, and an illustrated description is given of Mahler's calorimetric bomb. Similar thoroughness characterizes the other sections of the book, and renders it an important work of reference not only for the professional chemist, but for many others who, in the course of their professional work, have occasion to seek more detailed information on various subjects which may more properly be said to belong to the province of the technological chemist.

In addition to its thoroughness, the book possesses all those admirable qualities for which French writers of high-class textbooks have long been renowned, and which should secure for it a welcome place in every chemical library.

LXVI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 416.]

May 25th, 1904.—J. E. Marr, Sc.D., F.R.S., President,
in the Chair.

THE following communications were read :—

1. ' On the Occurrence of a Limestone with Upper Gault Fossils
at Barnwell, near Cambridge.' By William George Fearnsides, Esq.,
M.A., F.G.S.

The section in the great Gault-pit worked by the Cambridge
Brick Company at Barnwell is as follows :—

Thickness in feet.

5. Surface-soil with gravel and Chalk-Marl, disturbed ... 15 to 17
4. Dull leaden clay, almost devoid of determinable fossils,
   but with a few phosphate-nodules, etc. 39
3. Compact, well-jointed, homogeneous clay, with large
   ammonites of the *rostratus*- or *Bouchardianus*-type . 3
2. Hard calcareous bed with *Inoceramus, Schlœnbachia
   varicosa,* and *Terebratula biplicata* .... 0 to 1
1. Blue, well-laminated clay, with fossil-fragments and
   pale phosphate-nodules .... 4 seen

The limestone is variable in thickness, and is largely made up of
comminuted shells of *Inoceramus.* It occurs in a series of flattened
lenticles, a few yards in diameter and about a foot thick. It
contains abundant phosphate-nodules of at least three types—green,
pale, and dark-brown in colour. Foraminifera are abundant, as also
fragments of lamellibranchs, brachiopods, small gastropods, echinoids,
and crustacea. A fibrous material, possibly chitin, chips of quartz,
a little orthoclase, and glauconite are also recognized microscopically.
The fauna is not markedly different from that of the underlying
clay. A list is given which shows that this fauna has been recorded
from the Upper Gault of Folkestone, and agrees most closely with
that from Bed IX of Mr. Hilton Price's paper on that locality. As
these fossils are obtained 40 feet below the upper surface of the Gault
seen in the section, it is clear that the whole of the Upper Gault of
Cambridge was not used up in the making of the 'Cambridge
Greensand'; and this fact, together with the northward thinning
of the Gault as it passes into the Red Chalk, necessitates a
modification in the view commonly held as to the origin of this
'Greensand' deposit.

2. ' On the Age of the Llyn-Padarn Dykes.' By James Vincent
Elsden, Esq., B.Sc., F.G.S.

The paper produces evidence which seems to suggest that the
bulk of the greenstone-dykes of this area belong to an earlier
period of eruption than has been generally assigned to them; and
there is proof that some of them may even be older than the

quartz-felsite of the Llyn-Padarn ridge.   The greater part, if
not actually of Bala age, seem to have been intruded before the
great post-Bala crush-movements, which produced the folding of
the Lower Cambrian rocks of Llanberis, had entirely ceased.   At
the same time, the evidence does not exclude the possibility that
some of the intrusions may be of later date.   The evidence on
which these conclusions rest is based mainly on the signs which
the intrusions exhibit of having been considerably modified by
earth-pressures, more especially in those portions which protrude
into Cambrian strata.   Petrographical considerations, also, make
it impossible to separate these rocks from the diabase-sills of
Bala age occurring farther to the south and south-west of this
area ; and there is a strong presumption that they represent the
last residuum of the magma from which the Bala sills were
derived.

The north-western portions of the dykes, enclosed in the older
rocks of the Llyn-Padarn ridge, are comparatively free from
dynamic metamorphism ; but when traced into the more yielding
Cambrian grits and slates, they become structurally deformed
and often so highly sheared as to be hardly recognizable as parts
of the same dyke.   It is suggested that such highly-sheared green-
stones as occur in the ridge are of still older date.   One section is
described, in which a sheared greenstone is pierced by a tongue
of felsite about 2 inches wide and 2 feet long.   The felsite is
undistinguishable from that of the rest of the ridge and on the
borders of the greenstone.   Full petrographical descriptions of the
minerals of the rocks in their altered and unaltered state are
given : the minerals being taken in the order of their consolidation,
and the rocks considered in the 'dynamic' or crush-zone of the
sediments and in the 'static' or pressure-zone of the Llyn-Padarn
ridge.   These minerals are apatite ; iron-ores altered to sphene
and leucoxene, and to a mineral which is apparently perowskite ;
felspars belonging to the albite-anorthite series of one generation
undergoing 'albitization,' and the formation of felspar-mosaic ; two
pyroxenes, one possibly rhombic and the other like malacolite,
granulitized and associated with secondary albite in the crush-
zones, or passing into amphiboles and chlorites : original amphiboles
rare, but common as actinolite, tremolite, and asbestos alteration-
products of pyroxene ; biotite uncommon ; chlorite ; quartz ;
epidote ; and calcite.   In the least-altered rocks the minerals are
comparatively unchanged ; then there is, first of all, molecular
rearrangement under pressure without movement ; next, myloniti-
zation and recrystallization ; and lastly, the whole rock becomes
cataclastic, with partial or complete obliteration of its original
structure.   The gradual appearance of these features towards
the east is proof that the deforming agency operated from that
direction.

June 8th.—J. E. Marr, Sc.D., F.R.S., President,
in the Chair.

The following communications were read :—

1. 'The Palæontological Sequence in the Carboniferous Lime-
stone of the Bristol Area.' By Arthur Vaughan, Esq., B.A., B.Sc.,
F.G.S.

2. 'On a small *Plesiosaurus*-Skeleton from the White Lias of
Westbury-on-Severn.' By Wintour Frederick Gwinnell, Esq., F.G.S.

3. 'The Evidence for a Non-Sequence between the Keuper and
Rhætic Series in North-West Gloucestershire and Worcestershire.'
By Linsdall Richardson, Esq., F.G.S.

The section at Wainlode Cliff shows a transition in the 'Bone-
Bed,' from a thin pyritic stratum of an inch or so in thickness and
crowded with fish-remains, to a micaceous sandstone-bed, usually
devoid of such remains and about a foot thick, but containing Strick-
land's *Pullastra arenicola*. This sandstone is seen in many Worcester-
shire sections, and may be called the 'bone-bed-equivalent.' Thus
as the bed which is full of vertebrate remains, or the Bone-Bed
(Bed 15 of the author's sections), can be traced in a single section
laterally into a sandstone-bed devoid of those remains, the contem-
poraneity of the two developments is considered satisfactorily
established. Particular stress is laid upon the fact that above this
main 'Bone-Bed' the component deposits of the Rhætic are remark-
ably persistent, while below it such persistency is not found. Black
shales are generally present below the Bone-Bed or its equivalent in
Worcestershire, but in places there comes in a sandstone between
them and the 'Tea-Green Marls.' At Dunhampstead the Rhætic
rocks are thicker than at any other locality in Worcestershire. At
Denny Hill, near Gloucester, the 'Bone-Bed' rests directly on the
'Tea-Green Marls'; there is no infra-Bone-Bed deposit of Rhætic
date. At Garden Cliff, however, a comparatively thick accumulation
is seen in that position. The anticlinal and synclinal areas esta-
blished in the Mid- and North Cotteswolds by Mr. S. S. Buck-
man are referred to ; and it is found that the greatest thicknesses
of the Rhætic rocks under the Bone-Bed coincide with synclines, and
the least thicknesses with anticlines. The Moreton and Birdlip
anticlines are especially mentioned, as also the syncline of Cleeve
Hill and that between Painswick and Stroud. Thus Dunhampstead,
where the Rhætic deposits below the 'Bone-Bed' are thicker than
anywhere else in Worcestershire, is situated on a continuation of the
Cleeve-Hill synclinal axis ; Denny Hill, where the 'Bone-Bed' rests
directly upon the 'Tea-Green Marls,' is near the westward con-
tinuation of the Birdlip anticline ; and Garden Cliff, where the infra-
Bone-Bed deposit is thickest, is situated on a continuation of the
synclinal axis which runs near Painswick. Thus the earth-pressures
recognized in later times were probably at work at the close of the
Keuper Period. As the area, once covered by the waters of the
Keuper sea and the diminished representatives of that sea in the
form of lakes, gradually sank, the Rhætic ocean slowly encroached

upon the land-surface, flowing up the depressions in the undulating expanse of marls, and successive overlaps of the several infra-Bone-Bed deposits resulted : the greatest overlap apparently taking place during the formation of the Bone-Bed. At those localities where the distribution of the infra-Bone-Bed deposits indicates elevation of the Keuper Marls in immediate pre-Rhætic times, it is noticeable that there is also a non-sequence at the base of the Lias.

June 22nd.—J. E. Marr, Sc.D., F.R.S., President,
in the Chair.

The following communications were read :—

1. 'The Igneous Rocks of Pontesford Hill (Shropshire).' By Prof. William S. Boulton, B.Sc., Assoc.R.C.S., F.G.S.

This paper is confined to a description of the characters and sequence of the rocks within the limits of Pontesford Hill, and no attempt is made to correlate them with those of the Uriconian areas. The hill is a 'plagioclinal ridge,' bounded on all sides by faults ; it is made up entirely of igneous rocks, but some of the fine tuffs and volcanic grits show unmistakable signs of deposition in water. There are two distinct groups of igneous rocks : a bedded group, consisting of rhyolites and acid tuffs, with andesites and andesitic tuffs ; and an intrusive group of olivine-dolerites. The general strike of the bedded rocks is north-north-easterly and south-south-westerly, parallel to that of the neighbouring Longmyndian rocks ; the average dip is about 80° east-south-eastward, but at the extreme south-east of the hill the rhyolite and associated breccias dip in the opposite direction (west-north-westward) at about the same angle. The northern end of the hill consists of rhyolite (the 'northern rhyolite'), about 1000 feet thick, a pale pink and purple rhyolite with much epidote, chlorite, and secondary quartz, showing vesicular, spherulitic, pyromeridal, and banded structures. Macroscopic and microscopic descriptions of the rocks are given, and the origin of the spherulitic and nodular structures is fully discussed. In many cases, though certainly not in all, the nodules appear to have begun as a vesicle, often irregular in shape, and sometimes with crescentiform spaces round the main cavity, and separated from it by similarly-shaped portions of the glass. The spherulitic fibres appear to develop, not from a central point outward, but locally from vesicles or other cavities, crystals, etc., coalescing finally to form larger and longer growths. The spherulitic type of devitrification is not all of the same age, for fibrous growths traverse small and earlier-formed spherulites, which have been dissolved out and replaced by quartz. The andesitic group is made up of felsitic-looking, gritty pink and green tuffs, passing up into, and inter-bedded with, andesitic glassy (palagonitic), and crystal tuffs, hälleflintas, and lavas ; the thickness is about 1600 feet. A thickness of about 150 feet of rhyolite-breccias (glassy and crystal tuffs) and grits succeeds ; and this is followed by the south-eastern rhyolite, about 250 feet thick, a dark red or purple, coarsely-vesicular, well-banded rock, often with light-green and white

amygdules. The andesites consist of oligoclase and malacolite, embedded in a hyalopilitic groundmass containing palagonite, in which ilmenite, leucoxene, and magnetite are embedded. A table of the silica-percentages and specific gravities of the bedded rocks shows that a gap occurs between the 'northern rhyolite' and the more acid of the andesite-tuffs that immediately follow; this, together with a discordance in strike, may indicate a break in volcanic history, a disturbed junction, or that this rhyolite is intrusive. From this point onward, the tuffs and lavas form a continuous series, despite the difference in the average silica-percentage of the andesite-group and the rhyolite-breccias. The tuffs thin to the north-eastward, their lapilli diminish in size, and they become more gritty and washed in aspect in the same direction; facts which all point to the inference that the volcanic vent may have been to the west of the hill. The intrusive rocks are basic, and often amygdaloidal; they are granular or ophitic, and compare in composition with such olivine-dolerites as those of Rowley, the Clee Hills, and Little Wenlock, while they differ considerably from the intrusive dolerites of North Wales.

2. 'The Tertiary Fossils of Somaliland, as represented in the British Museum (Natural History).' By Richard Bullen Newton, Esq., F.G.S.

3. 'The Caernarvon Earthquake of June 19th, 1903, and its Accessory Shocks.' By Charles Davison, Sc.D., F.G.S.

The Caernarvon earthquake of June 19th, 1903, was the strongest earthquake indigenous to the county for more than five centuries. Its disturbed area contained about 25,000 square miles, and included nearly the whole of Wales, the North-West of England, the Isle of Man, and several of the eastern counties of Ireland. The centre of the innermost isoseismal (intensity 7) was situated beneath the sea, about 4 miles west of Pen-y-groes, and the longer axis of the isoseismal ran from N. 40° E. to S. 40° W. It is concluded, from the seismic evidence, that the earthquake was caused by a slip of about 16 miles in length along a fault running in the above direction, hading north-westward, and passing either through Clynnog or a mile or two either to the north-west or south-east. In the former case the fault-line might be submarine; and it is pointed out that, if the fault which runs in a south-westerly direction from Aber to Dinlle (on the coast of Caernarvon Bay) were continued underneath the sea to the neighbourhood of Nevin, it would occupy the position assigned to the parent-fault by the discussion of the earthquake-phenomena.

The principal shock was preceded by an earth-sound, and followed by at least five shocks, originating apparently at the north-western extremity of the principal focus. In addition, six slight shocks and two earth-sounds were recorded by single observers; and, if these be included in the earthquake-series, it follows that seismic action was gradually withdrawn from the extremities of the focus and ultimately confined to its central region.

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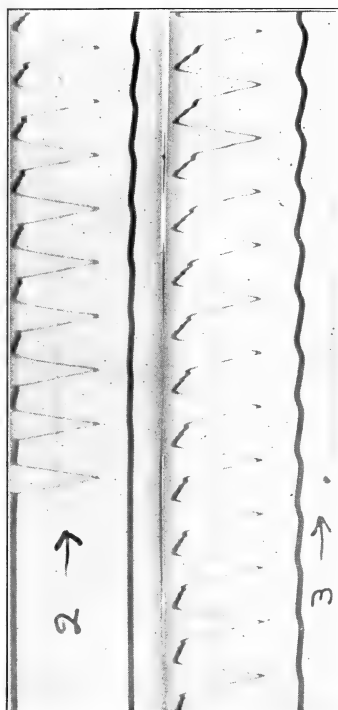
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Fig. 2.
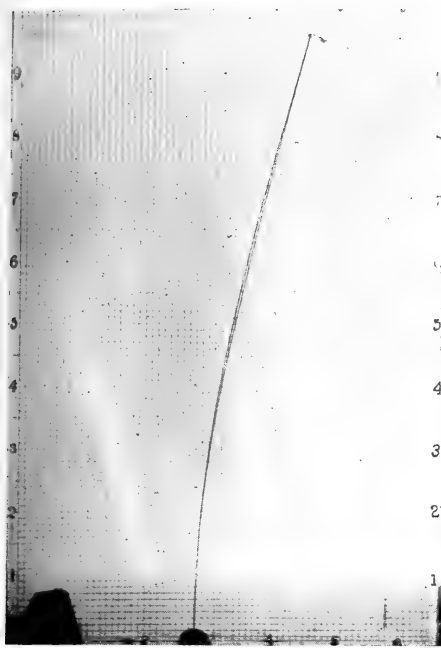
Fig. 3.
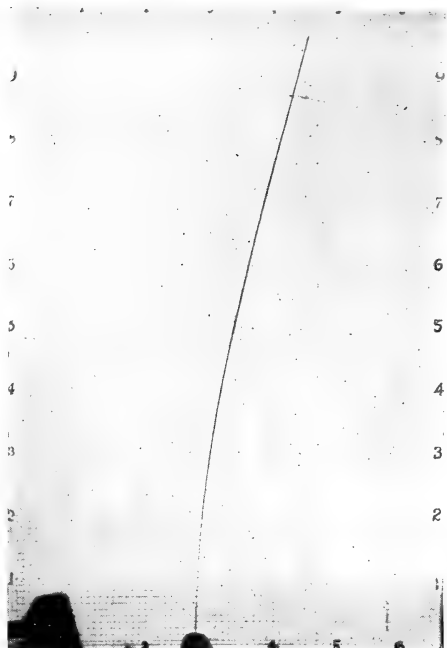
Fig. 4.
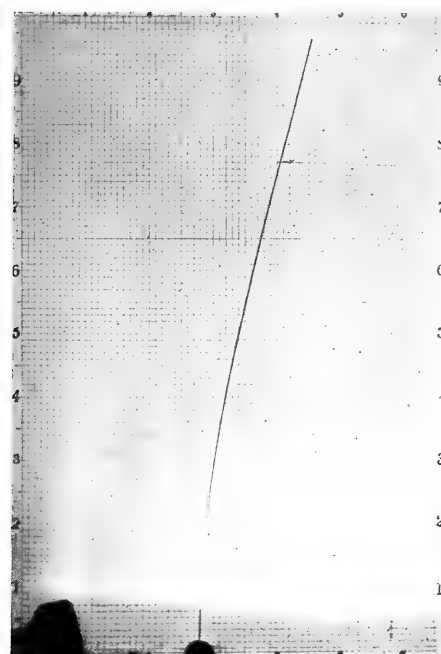
Fig. 5.
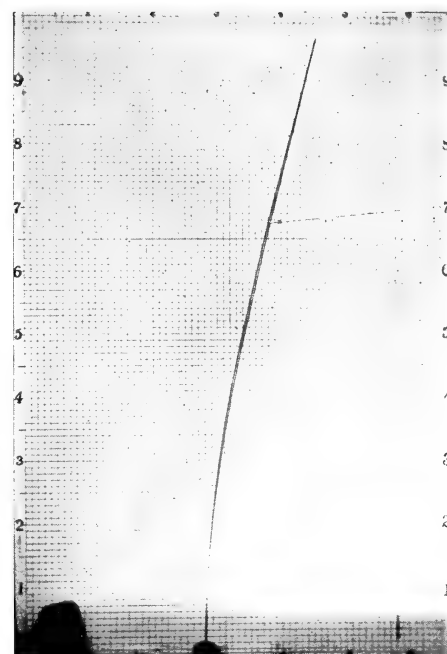
LXVII. *On the Secondary Radiation caused by the β and γ Rays of Radium.* By A. S. Eve, *M.A., McGill University* [*].

THE experiments described in this paper were suggested by an investigation made by Townsend [†] on the secondary Röntgen rays. A summary of his results is given by J. J. Thomson (pp. 261–268) in 'Conduction of Electricity through Gases.' It was hoped that analogous work with the penetrating rays of radium would throw further light on the relationship between Röntgen and γ rays. It will be convenient to state Townsend's method and results at the outset.

Secondary Radiation.	Rays through Air.	Rays through Aluminium ·25 mm.
Air.................	2	1·0
Aluminium	6	3·5
Glass	7·5	3·0
Lead	24	6·0
Paraffin (Solid)...	30	15·5
Brass	66	2·5
Zinc	68	3·0
Copper	70	2·5

The figures denote scale-divisions of the electrometer in 10 seconds. The bulb employed was placed in a lead box. The Röntgen rays passed through a hole in the lead and struck the radiator; the secondary radiation was measured by the ionization current between a charged cylinder of wire gauze and an axial electrode connected with a quadrant

[*] Communicated by Prof. E. Rutherford, F.R.S.

[†] J. S. Townsend, Proc. Camb. Phil. Soc. x. p. 217 (1899).

electrometer.   The secondary rays could be intercepted by an aluminium screen, so as to test their absorption.

Fig. 1.

Plan
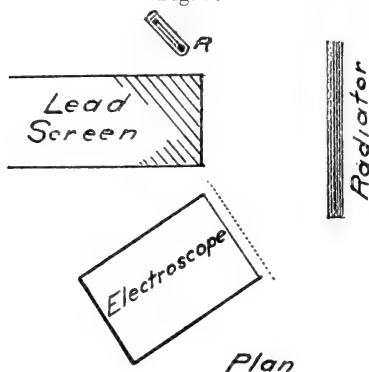
Apparatus.—In the present experiment (fig. 1) an electroscope of square section was used ; one face was removed and replaced by aluminium foil, ·05 mm. thick.   The radium (25 mgs.) was placed within a small glass test-tube inside a second test-tube.   The glass therefore cut off the α rays and some of the low velocity β rays.   The electroscope was screened to a great extent from the radium by two blocks of lead, each 4·5 cms. thick.   Thick plates of various substances, used as radiators, were placed at a distance of 20 cms. from the radium, and 12 cms. from the thin face of the electroscope.   Screens of wood, aluminium, glass, brass, and lead could be placed in front of the electroscope in order to measure the degree of penetration of the secondary rays.   The apparatus was subsequently modified (fig. 2), to determine the

Fig. 2.
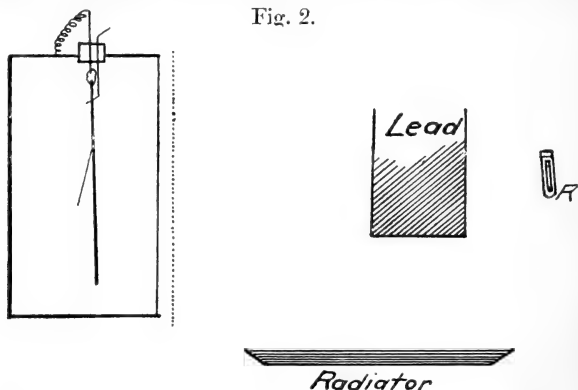
Radiator

secondary radiation from liquids.   The primary rays were screened from the electroscope by the same lead blocks as before, but placed horizontally, and raised from the table.

The liquids were poured into a tray beneath the blocks, and the secondary rays, passing obliquely upwards, were measured by the electroscope. Plates of various solid substances were substituted for the liquids, and the magnitudes of the secondary radiations from the liquids were interpolated from the known values of the radiations from the solids.

The rate of collapse of the gold leaf of the electroscope was measured by a microscope with a micrometer eyepiece.

Results.—The secondary radiation, when air is the radiator, is very small. The number of scale-divisions indicated by the electroscope, due to natural leak, to penetrating γ rays, and to secondary radiation from air and surrounding bodies, was subtracted from the number of scale-divisions when a thick lead plate was used as radiator. This difference was taken as a measure of the secondary radiation from lead due to the β and γ rays of radium. An aluminium screen ·85 mm. thick, or a brass screen ·04 mm. thick, was then placed in front of the electroscope in order to measure the absorption of the secondary rays; and similar measurements were made for various secondary radiators.

The following table gives the results in scale-divisions per

TABLE I.—β and γ rays.

Radiator.	Density.	Secondary Radiation.	Sec. Rad. Density.	Aluminium ·085 cm. λ'.	Brass ·004 cm. λ'.
Mercury	13·6	147	10·8		
Lead	11·4	141	12·4	18·5	95
Copper	8·8	79	9·0	20	107
Brass	8·4	81	9 6	21	116
Iron (wrought)	7·8	75	9·6	20	116
Tin...............	7·4	73	9·9	20·3	
Zinc	7·0	79	11·3		
Granite	2·7	54	20·0	12·4	
Slate	2·6	53	20·4	12·1	89
Aluminium ...	2·6	42	16·1	24	114
Glass	2·5	44	17·6	24	122
Cement	2·4	47	19·6	13·5	
Brick............	2·2	49	22·3	13·0	78
Ebonite........	1·1	32	29·1	26	168
Water	1·0	24	24·0	21	173
Ice	·92	23	28·2		
Paraffin solid.	·9	17	18·8	21	155
„ liquid.	·85	16	18·8		
Mahogany......	·56	21·4	38·2	23	164
Paper	·4 ?	21·0	52	22	113
Millboard	·4 ?	19·4	48	20·5	147
Papier-mâché .	...	21·9			
Basswood	·36	20·7	57	22	147
Pine	·35	21·8	62	21	145
X-ray screen...	...	75·2	...	23·6	

minute.   The secondary radiation divided by the density, and the coefficients of absorption of the secondary radiations by aluminium and brass, are also stated.

The results are shown graphically in fig. 3, where the

Fig. 3.
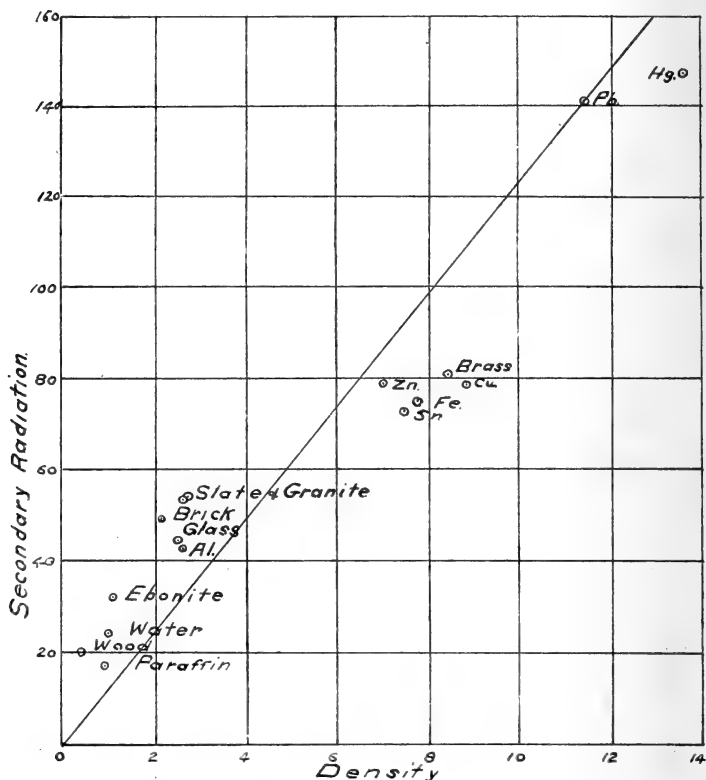
densities are denoted by abscissæ and the secondary radiation by ordinates.

Some experiments were made in order to ascertain the depth from which secondary radiation was effective at the surface.   In fig. 4 are shown the secondary radiations due to successive layers of paper.   The radiation was nearly proportional to the number of sheets until 10 or 12 sheets of foolscap (each ·06 mm. thick) were used.   After that the rate of increase of secondary radiation fell away rapidly, and 36 sheets gave the same result as a much larger number. Good radiators, such as brick or lead, causing strongly penetrating secondary radiation, could be detected behind

55 sheets of paper. But the secondary radiation from paper alone comes from a depth ranging to 2 or 3 millimetres.

Fig. 4.
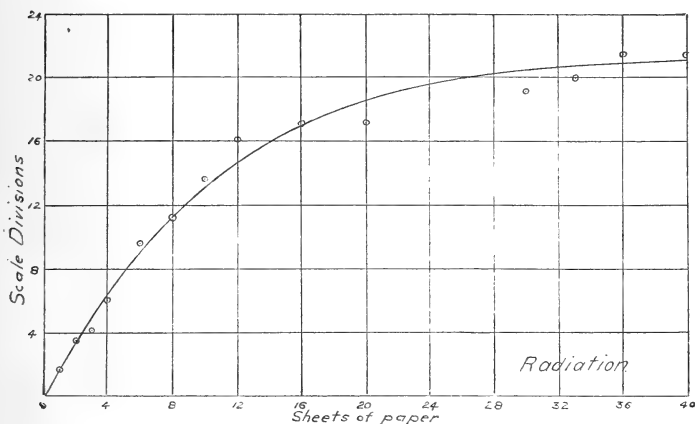
Similar experiments with glass and aluminium gave an effective depth of 3 mm., but here again lead or brick could be detected through a greater thickness. A plate of lead produced a measurable effect from behind 2·8 mm. of brass; but if a brass plate were placed behind, there was no increase of radiation. The state of the surface, whether wet or dry, dirty or clean, rough or polished, was immaterial to the results. A layer of stout filter-paper well soaked in water, placed upon a lead plate, reduced the radiation from 141 to 120 scale-divisions. A screen of barium platino-cyanide, as used for showing the shadows thrown by Röntgen rays, gave rise to secondary radiation nearly four times as great as that from an uncoated cardboard of equal thickness. But the coefficient of absorption, 23·6, was only a little greater than from paper or millboard, so that these secondary rays are readily absorbed.

Iron filings gave very little less radiation than solid wrought iron, and about the same amount as cast iron.

Some experiments were made with γ rays alone, cutting off the β rays with layers of lead placed between the radium and the radiating plates. The secondary radiation was decreased thus :—

Lead interceptor.	Secondary radiation from lead.
0	141
6·3 mm.	27
12·7 mm.	21

On plotting the results for density and the γ secondary radiation, it was found that the substances held the same relative position as for β and γ rays jointly. The change appears to be one of intensity, not of kind. The coefficients of absorption by aluminium and brass were also calculated, and their values averaged 17 per cent. more than those from the collective β and γ rays. It is clear that the secondary radiations from β and from γ rays are of the same character. The secondary radiation from the γ rays alone is less intense and less penetrating, but some of this difference is due to the loss of intensity by the γ rays in passing through 6 mm. of lead.

From the above results it appears that the β rays caused about 85 per cent. of the secondary radiation, and the γ rays 15 per cent. This was confirmed by using a strong magnetic field whereby the β rays were deflexed, and then absorbed by lead, so that the γ rays only remained.

It is now possible to compare Townsend's results with Röntgen rays with those for γ rays.

Table II.—Secondary Radiations.

Radiator.	β and γ Rays.	γ Rays.	Röntgen Rays.
Lead	100	100	100
Copper	57	61	291
Brass	58	59	263
Zinc	57	...	282
Aluminium	30	30	25
Glass	31	35	31
Paraffin	12	20	125

In the above table lead has been selected as the standard for each column.

It will be seen that the Röntgen rays give results widely divergent from the order expected from considerations of density. The radiation from lead is actually less than that from paraffin. It seemed possible that a hard bulb might give results differing from those found by Townsend. As a test case the secondary radiations from lead and brass were compared. The results from brass were a little larger than than those from lead, so that Townsend's order was confirmed, for hard as well as soft rays, in the case of these two substances.

As Townsend used an aluminium screen, ·24 mm. thick, it

is easy to calculate the coefficient of absorption in aluminium from secondary radiation caused by Röntgen rays. The values for β and γ rays and for γ rays alone are placed by the side for comparison with those from Röntgen rays. The results from γ rays are not very accurate, as the quantities to be measured were small when the screen was used.

TABLE III.—λ' for Aluminium, Secondary Rays.

Radiator.	β and γ.	γ.	Röntgen.
Lead	19	28	55
Copper	20	23	133
Brass	21	27	131
Zinc	21	23	169
Aluminium	24	32	22
Glass	24	...	37
Paraffin...........	21	...	26

This table further accentuates the difference between the secondary radiations from γ and from Röntgen rays, the latter having much less penetrating power, particularly in the case of zinc, brass, and copper, whilst lead behaves in a manner apparently anomalous.

In order to test the character of the secondary radiations due to radium, layers of paper were placed in front of the electroscope, lead being used as radiator. The results are shown graphically in fig. 5. The logarithms of the ordinates

Fig. 5.
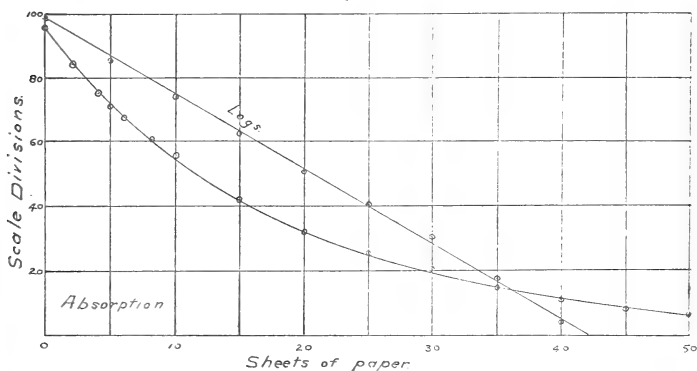
when plotted give a straight line, so that the curve has a simple exponential equation $I = I_0 \epsilon^{-\lambda' x}$ where $\lambda' = 9$.

It appears that in this case the secondary rays are more homogeneous than the rays which cause them, since the coefficient of absorption is not constant for the primary β or γ rays.

A comparison of the absorption by different screens in the case of secondary radiation from lead due to β and γ rays may now be made :—

TABLE IV.

Screen.	λ'.	λ'/Density.
Lead	132·6	11·6
Brass	95	11·3
Aluminium	18·5	7·1
Glass	23·5	9·4
Wood (Bass)......	2·7	6·8
Paper..	9·0	22·5

The order is still roughly that of density, but the absorption is not proportional to it.

Experiments were made to determine the nature of the secondary radiation from surfaces when the rays passed through the substances from the other side. The electroscope was placed on a thick lead block beneath which was the radium. The various plates were placed horizontally, and in front of the electroscope, so that the rays struck the lower surfaces ; the secondary radiations from the upper surfaces were measured by the electroscope. The difficulty of this method was found to be that the secondary radiation from the air exposed to the full rays was larger than the secondary radiation from an interposed plate which cut down the intensity of the primary rays. The effect produced depended upon the thickness of the plate as well as upon its density. Thus a thin layer of wood gave more radiation than a plate of metal of equal thickness ; not because the wood radiated better, but because the metal cut down the primary and secondary rays more effectively. It was not found possible to make a satisfactory comparison of the values from different materials.

Equal radiations were obtained from 50 mm. of wood, 5·5 mm. of slate, and 11 mm. of zinc. So also 3 mm. of brass gave the same amount as 15 mm. of water in a papier-mâché tray.

Successive layers of paper were placed in front of the

electroscope.   The results are shown in fig 6 for a number
of sheets ranging up to 100 with a total thickness of 6 mm.

Fig. 6.
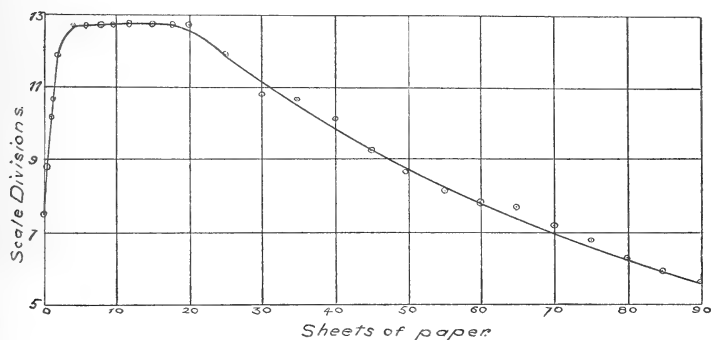
The first few sheets increased the radiation, until a maximum
was reached for about 12 sheets.   The values were almost
steady between 9 and 20 sheets.   After that the curve fell,
partly because the primary rays were cut down, and partly
because the secondary radiation cannot penetrate more than
about 36 sheets (fig. 4).

It will be seen later that theoretical considerations would
lead one to expect an equation to this curve having the form
of the difference of two simple exponentials, and the curve
appears to resemble this well-known type.

In the case of zinc, the rise and fall to a fairly steady
value took place for layers ranging up to a thickness of
1·6 mm.

Results.

The secondary radiation from various substances follows
approximately the same order as their densities (Table I.).
The coefficients of absorption of secondary radiation by various
substances also follow roughly that order (Table IV.).   Good
radiators are therefore good absorbers.   Neither the secondary
radiations nor the coefficients of absorption are proportional
to the densities.   The secondary radiation does not come
from the surface merely, but from a total depth varying from
about 1·5 mm. in the case of lead to about 3 mm. for glass,
aluminium, or paper.   It is mainly independent of the state
of the surface.   Almost the same amount of radiation is
obtained from solid iron as from iron filings ; from liquid
and from solid paraffin ; from ice and water ; from paper,
millboard, papier-mâché, mahogany, pine, and basswood.

On reference to Table I. it will be seen that the most

penetrating secondary rays are given by granite, slate, brick, and cement.

If three centimetres of water be placed in a thin tray on a brick or slate slab, the secondary radiation will be decreased by placing a thin sheet of lead at the bottom of the water. The lead radiates more than the slate or brick, but its rays are less penetrating; and because it screens more than it causes there is a net loss.

A similar experiment may be made with glass or wood raised above slate. A sheet of lead placed below the glass or wood will decrease the total radiations.

The total radiation from a number of layers of different substances may therefore be dependent on the order in which they are arranged.

The different values obtained for the coefficients of absorption by aluminium, when various substances are used as radiators, point to the fact that the absorptions of these rays by the air in the electroscope will be different. For example, iron gives secondary radiation 75 and brick 49; but λ' for iron is 20 and for brick 13. Therefore the radiation from brick will pass through the electroscope with less absorption than that from iron. The energy absorbed by the air in the two cases will be in the ratio of 13 to 20. The corrected value from brick relative to iron will be $49 \times \dfrac{20}{13}$ or 75·4.

If similar corrections are made with ebonite as standard, and applied to Table I., we can arrange the results as follows:—

Substance.	Secondary Radiation.	Sec. Rad. / Density.
Copper	103	11·7
Brass	100	11·9
Iron	98	12·6
Tin	93·5	12·6
Aluminium	40	15·4
Lead	198	17·3
Glass	48	19·2
Paraffin	21	23
Ebonite..........	32	29
Water	30	30
Mahogany	24	37
Granite..........	113	42
Slate	114	44
Brick	98	47
Millboard	24·6	61
Paper	25	62
Basswood	27	75
Pine	27	77

There seems a tendency to form groups which may be more than fanciful. In any case the chief effect of the correction is to accentuate the high radiation from lead, granite, and slate.

The error caused by the thin aluminium face of the electroscope has a balancing effect in the opposite direction. Although this screen may reduce the recorded radiations below the actual values, the relative error will not exceed 2 per cent., except in the case of the more penetrating rays, such as those from granite or slate.

Theory.—The β rays are known to consist of negatively charged particles moving with different velocities. With these rays are present the γ rays of greater penetration, probably of the type of Röntgen rays. The large range of velocities of the β rays causes a wide variation in their degree of penetration. It is therefore impossible to assign any definite values for the initial coefficients of absorption of most substances exposed to the β rays. This difficulty has prevented the reduction of the preceding results to any definite mathematical theory. For example, with aluminium, using the primary β rays we have :—

Thickness in mm.	λ.
0 to ·85	6·5
·85 to 2·5	5·7
2·5 to 4·1	1·6

The theory which agrees most nearly with the results is that the secondary radiation set up in a thin layer is proportional to the density and to the rate of absorption of the primary rays. If the amount absorbed in thickness dx be dI, then the secondary radiation is $k\rho dI$, where ρ is the density and k is a constant. The amount which reaches the surface will be $k\rho dI\epsilon^{-\lambda'x}$, where λ' is the coefficient of absorption of secondary radiation. Now $I = I_0\epsilon^{-\lambda x}$, where λ is the coefficient of absorption for primary radiation, and I_0 is the surface intensity of the primary rays. Therefore the total secondary radiation is :—

$$\int_0^\infty k\rho\lambda\epsilon^{-(\lambda+\lambda')x}\,dx = \frac{k\rho\lambda}{\lambda+\lambda'}.$$

The ratio of the secondary radiation to $\dfrac{\lambda\rho}{\lambda+\lambda'}$ was found to be fairly constant for lead, glass, wood, and paper, but brass and aluminium showed large deviation. The attempt to prove this theory was therefore abandoned, as it was impossible to assign definite values to λ.

McClelland has shown that the initial values of λ for γ rays are also variable, so that these rays do not offer a suitable test.

The secondary radiation from a light substance appears to be more homogeneous in character than the primary rays by which it is caused (fig. 5).

In the case of paper screens of thickness x the intensity of the secondary radiation is proportional to

$$\epsilon^{-\lambda' x} \quad . \quad . \quad . \quad . \quad . \quad (\text{fig. 5}).$$

When the primary rays pass through a substance of thickness a, it will be shown later that the secondary radiation is roughly proportional to

$$\epsilon^{-a\lambda} - \epsilon^{-a(2\lambda + \lambda')}. \quad . \quad . \quad . \quad (\text{fig. 6}).$$

And when the secondary rays come from the surface first struck by the primary rays, the secondary radiation is roughly proportional to

$$1 - \epsilon^{-a(\lambda + \lambda')} \quad . \quad . \quad . \quad . \quad (\text{fig. 4}).$$

There is a general agreement between the equations and the types of curves.

The radiations due to the rays jointly, and the γ rays alone, appear to be similar in character, the latter being less intense and of a less penetrating type. The secondary radiations caused by Röntgen rays in various substances follow an order relative to density different from the secondary radiations caused by β and γ rays jointly, or by γ rays alone. The penetrating power of the secondary Röntgen rays from various substances is also more irregular and in most cases more feeble.

When the primary rays strike the lower surface of a plate of thickness a, the intensity in the substance at a distance x from the upper surface is now $I_0 \epsilon^{-\lambda(a-x)}$.

Proceeding as above, the total radiation at the upper surface will be

$$k\rho\lambda\epsilon^{-a\lambda} \int_0^a \epsilon^{-(\lambda + \lambda')x},$$

or

$$k\rho\lambda/(\lambda + \lambda') \, . \, (\epsilon^{-a\lambda} - \epsilon^{-a(2\lambda + \lambda')}).$$

A maximum point is given by

$$a(\lambda + \lambda') = \log_\epsilon(2\lambda + \lambda')/\lambda.$$

The curve in fig. 6 is of a type whose equation is the difference of two simple exponentials as above.

The variation in the initial value of λ prevents an exact verification of the equation with the curve.

Similar calculations for the curve in figure 4, when the secondary rays come from the surface which the primary rays first strike, lead us to expect a curve given by

$$y = k\rho \, . \, \lambda/(\lambda + \lambda') \, . \, (1 - \epsilon^{-a(\lambda + \lambda')})$$

where a is the thickness of the substance. The curve obtained has the general character required by the equation.

PART II.

Magnetic Effects.

In order to determine the nature of the secondary radiation caused by γ rays, a small electroscope was mounted on a lead platform (1·2 cm. thick) on a lead cylinder 10 cm. high and 10·7 cm. in diameter.

Fig. 7.
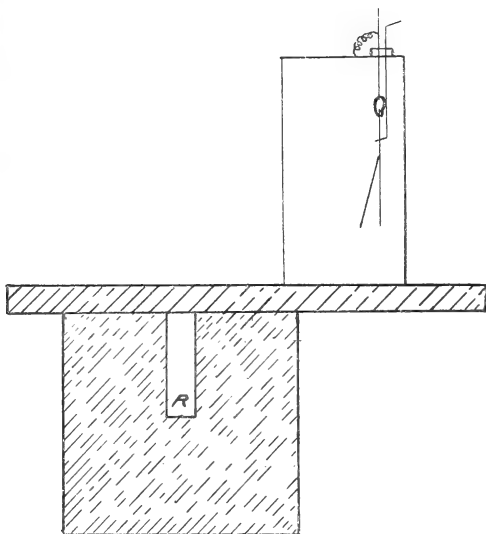
Radium (25 mgs.) was placed at the bottom of a hole reaching to the centre of the cylinder. On applying a strong magnetic field, as at right angles to the plane of the paper (fig. 7), so as to bend the rays from the platform into the electroscope, it was found that the rate of discharge was nearly doubled, rising from 9·2 to 18·2 scale-divisions per minute. It is therefore clear that either some of the γ or of

the γ secondary rays consist of negatively electrified particles in motion.   To discern between these rays, thin screens of lead were placed close in front of the thin aluminium foil (·02 mm.) which formed that face of the zinc electroscope nearest the radium.   About ·2 mm. of lead reduced the effect to half value, and 1 or 2 millimetres removed it.   It is plain that the rays deflected were of the nature of secondary radiation with low velocity and small penetrating power.

The results obtained from an increasing series of lead screens in front of the aluminium foil are shown in fig. 8.

Fig. 8.
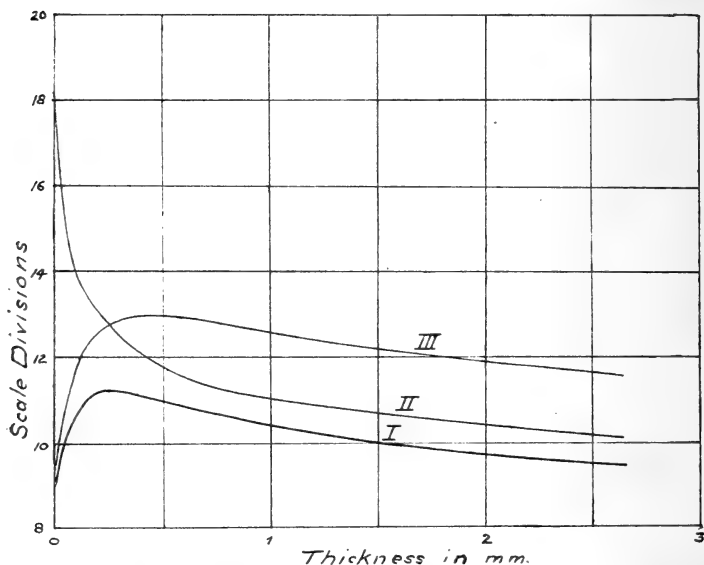
The screens were ·13 mm. thick.   Curve I. represents the scale-divisions per minute for various thicknesses when there is no magnetic field.   The scale-divisions are due to natural leak, secondary radiations, and γ rays passing through the electroscope.   A few screens increased the ionization currents: this was due to the secondary radiation from the lead screens caused by the primary rays.   This result is similar to Becquerel's increased photographic effect with γ secondary rays.   After the maximum was reached, more lead screens decreased the ionization in the electroscope in a manner already explained, and as in fig. 6.

Curve II. shows the results when the magnetic field bends the rays towards the electroscope.   The initial high value is

due to the secondary rays, from the top of the platform, bent into the electroscope.

The rapid fall of the curve shows that the bent rays consist of low velocity particles, easily absorbed. When the magnetic field was increased in strength the initial value was less, because some of the rays were curved right on to the platform, and absorbed. In this case, too, the curve fell less rapidly, so that the rays remaining were more penetrating and had higher velocities. The particles are therefore projected with various velocities from the lead block.

Curve III. gives the results when the magnetic field is reversed. The added lead screens again produce an increase of secondary radiation as in Curve I. After passing the maximum, the height of the curve is not materially reduced, because the outside rays are bent away from the electroscope and cannot be cut down by the lead.

It may be noted that thin screens placed on the platform (fig. 7) increase the secondary radiation, both when there is no magnetic field and when the rays are bent into the electroscope.

Substance.	Thickness in mm.	No Field.	Rays bent in.
......	...	8·8	17·6
Lead	1·5	9·3	15·0
Lead	5·0	8·1	13·7
Copper	1·0	9·4	16·7
Aluminium	·85	10·0	17·6
Glass	2·0	10·2	17·1
Ebonite	5·0	10·6	20·0
Basswood	2·0	10·9	18·7

The explanation rests in the rise to a maximum shown in Curves I. and III. fig. 8, and in fig. 6. The increased values are in the inverse order of density. The secondary radiation from the lead will pass through a thin substance, and the secondary radiation from the added layer caused by the γ primary rays will reinforce the effect.

Discussion of Results.

Becquerel has shown that the β secondary rays can be deflected by a magnet.

Curie and Sagnac proved that Röntgen rays striking a metal plate cause it to emit negative electricity and to acquire a positive charge ; Dorn has proved that such rays can be deflected by a magnet.

In the present paper it is shown that the electroscope method will detect the readily deviable rays caused by the γ rays, and that such rays are absorbed by a few millimetres of lead.

It is noteworthy that β, γ, and Röntgen rays all give rise to a secondary radiation consisting of moving particles having a negative charge.

As cathode rays give rise to Röntgen rays, and the latter can cause rays of a cathode type, the process appears reversible. It is probable therefore that Röntgen, β, and γ rays all produce secondary radiations of the Röntgen type as well as those of the β type.

In a recent communication to the *Annalen der Physik* (Nos. 6, 7, 1904) Paschen has given an account of some experiments which he considers to be evidence in favour of the view that the γ rays consist of negatively charged particles moving with higher velocities than the β rays. He claims that the γ rays are of the type of cathode rays, and not of Röntgen rays.

In the first experiment he enclosed radium in an envelope of lead thick enough to absorb the β rays. He found that the lead, if well insulated in a vacuum, acquired a positive charge, and he justly concluded that negatively charged particles were projected from the surface of the lead. He further claimed that these negative particles constituted the γ rays or a part of such rays.

Now in the experiment described in this paper (figs. 7 and 8) it has been shown that the particles projected from a thick mass of lead, containing radium at its centre, are completely absorbed by one or two millimetres of lead, and are readily deflected in a magnetic field ; whereas the γ rays will penetrate several centimetres of lead, and have never yet been deviated in a more powerful magnetic field.

It is clear, therefore, that Paschen was not justified in his conclusion that the γ rays consist of negatively charged particles, but that he was really dealing with a secondary radiation caused by the γ rays, and proceeding from the outer layers of the lead envelope.

Direct experiments by Becquerel, Rutherford, Villard, and others have failed to detect any curvature of the γ rays in a magnetic field. There is at present no evidence that the γ rays consist of negatively charged particles. On the other hand, the β rays can be deflected completely away from the γ rays. This abrupt discontinuity in the magnetic spectrum indicates that γ rays are not merely a group of those β rays which have the highest velocity.

We may conclude that the γ rays either consist of particles practically devoid of electric charge, or are of the type of Röntgen rays, or have a special character of their own hitherto unknown.

The dissimilarity between Röntgen and γ rays in connexion with the ionization of gases and vapours, and in the character of the secondary radiations, is probably due to the fact that the Röntgen pulses are more broad than those which constitute the γ rays. The differences are decreased when harder bulbs are used, and the resulting pulses are more thin.

Theoretical reasoning still supports the view that the expulsion of the β particles must be accompanied by pulses similar to those set up on the stoppage of the cathode particles; and, until there is definite evidence to the contrary, it is reasonable to suppose that these pulses, analogous to the Röntgen rays, constitute the γ rays.

In conclusion the writer has pleasure in acknowledging the kind help and advice received from Professor Rutherford.

McGill University, Montreal.
    27th Aug., 1904.

LXVIII. *The Electric Origin of Gravitation and Terrestrial Magnetism.* By WILLIAM SUTHERLAND[*].

THE conception that the inertia of matter and that of electricity must be identical makes it probable that gravitation has an electric origin. As cohesion and rigidity have been shown to be essentially electric properties of matter (Phil. Mag. Dec. 1902 ; May 1904), the investigation of gravitation from a similar point of view invites attention. In a short paper read before the Royal Society of Victoria in 1877, on a connexion of causation amongst electricity, magnetism, and gravitation, my late brother Alexander argued that the inverse square law of force indicated such a connexion. With this idea as a starting point we may proceed as follows :—Suppose matter to be formed of pairs of electrons $\sharp\flat$ whose inertia constitutes the inertia of matter. Such a pair of electrons, or molecule of electricity, when forming a component of the æther, I have proposed to call a neutron. Let us write down the force between two such doublets in a more general form than that usually assumed. Suppose them to be at a distance r apart which is great in comparison with the distance between $\sharp$ and $\flat$ in each pair. Let the charge of $\sharp$ be e_1, and of $\flat$ be e_2. Let the attraction

between $\sharp$ in the first pair and $\flat$ in the second be $e_1 e_2/r^2$, and *vice versa*. Let the repulsion between $\sharp$ in the first and $\sharp$ in the second be $(1-b)e_1^2/r^2$, and the repulsion between $\flat$ in the first and $\flat$ in the second be $(1-c)e_2^2/r^2$. The fractions b and c will be shown to be small and of opposite sign. Probably the law of attraction between $\sharp$ in one doublet and $\flat$ in another is more strictly $(1+\delta)e_1 e_2/r^2$, where δ is of the order b^2 or c^2, but we neglect δ for the present. The total attraction between the two doublets is

$$\{-e_1^2(1-b)-e_2^2(1-c)+2e_1 e_2\}/r^2$$
$$= \{-(e_1-e_2)^2+be_1^2+ce_2^2\}/r^2. \quad . \quad . \quad (1)$$

Now by **J. J.** Thomson's evaluation of the inertia i of an electron of charge e in electrostatic units spread over the surface of a sphere of radius a, we have $i=2e^2/3a\mathrm{V}^2$, where V is the velocity of light. For other distributions of the charge e the coefficient $2/3$ must be altered. Let us then write

$$i_1 = \frac{2}{3}\frac{e_1^2}{a_1 \mathrm{V}_1^2}, \quad i_2 = \frac{2}{3}\frac{e_2^2}{a_2 \mathrm{V}_2^2}, \quad . \quad . \quad . \quad . \quad (2)$$

and then for the attraction between two doublets we write (1) in the form

$$9\{-(i_1 a_1 \mathrm{V}_1^2/e_1 - i_2 a_2 \mathrm{V}_2^2/e_2)^2 \\ + b i_1^2 a_1^2 \mathrm{V}_1^4/e_1^2 + c i_2^2 a_2^2 \mathrm{V}_2^4/e_2^2\}/4r^2. \quad . \quad . \quad (3)$$

The first point to notice about this expression is that, if b and c are 0, the force must be a repulsion and could not account for gravitation. This fact gives the clue to the most convenient simplification to be made in (3), namely, that of assuming that the repulsive term $(i_1 a_1 \mathrm{V}_1^2/e_1 - i_2 a_2 \mathrm{V}_2^2/e_2)^2$ vanishes. We may also try the effect of assuming $\mathrm{V}_1 = \mathrm{V}_2 = \mathrm{V}$, and we then have $e_1 = e_2$, and may denote each by e. We have also $i_1 a_1 = i_2 a_2$, and may denote each by ia. The attraction (3) now becomes

$$9(b+c)i^2 a^2 \mathrm{V}^4/4e^2 r^2. \quad . \quad . \quad . \quad . \quad (4)$$

If now there are n_1 doublets in one atom of mass m_1, and n_2 in another of mass m_2 at distance r, the total attraction between the atoms is $n_1 n_2$ times (4).

But $2n_1 i = m_1$ and $2n_2 i = m_2$, so that the attraction is

$$9(b+c)m_1 m_2 a^2 \mathrm{V}^4/16e^2 r^2. \quad . \quad . \quad . \quad (5)$$

If G is the constant of gravitation (C.G.S.) then (5) expresses the electric origin of gravitation on condition that

$$\mathrm{G} = 9(b+c)a^2 \mathrm{V}^4/16e^2. \quad . \quad . \quad . \quad . \quad (6)$$

Now $G = 666 \times 10^{-10}$ (Burgess, Physical Review, 1902), $V = 3 \times 10^{10}$, $e = 3 \times 10^{-10}$, and $a = 2 \times 10^{-13}$ (Electric Origin of Rigidity, section 6). This value of a is deduced from the velocity and energy of a negative electron in the cathode stream. For an electron forming part of an atom the value may be different. Provisionally assuming that a has the same value in an electron of matter as in a free electron we find from (6) that

$$b + c = 32 \times 10^{-44}. \quad \ldots \quad \ldots \quad (7)$$

As we shall see that b and c are of opposite sign and nearly equal, $b + c$ is the small difference of the numerical magnitudes of b and c. Gravitation then can be traced to an electric origin, if between two electric doublets there is an outstanding electric attraction which is of the order 10^{-43} times the force between two of the electrons, one in each doublet. Ordinary experimental evidence of a difference of this order of magnitude has hitherto been out of the question. We turn then to phenomena on the same large scale as universal gravitation in astronomy, to find whether it is possible to get further evidence of this difference, terrestrial magnetism holding out the greatest promise. Now in " A Possible Cause of the Earth's Magnetism and a Theory of its Variations," and in " The Cause of the Earth's Magnetism " (Terrestrial Magnetism and Atmospheric Electricity, v. 1900, and viii. 1903), I have shown that the Earth's primary magnetic field can be accounted for by spherical distributions of positive and negative electricity within the Earth and revolving with it, the negative being on the whole nearer to the surface. In the second of these papers it was shown that if each two atoms of the Earth's substance contain the electron pair ♯♭ in accordance with Helmholtz's theory of chemical valence, and if the total electricity of each kind is supposed to form a uniform sphere of nearly the same radius as the Earth, the negative sphere need have a radius exceeding that of the positive sphere by only 10^{-8} cm., that is by about the diameter of an ordinary molecule. In that paper the origin of this small difference in the radii was not investigated, but was merely commented upon as implying a small tendency of each ♭ in a molecular doublet to take up a position farther from the centre of the Earth than ♯.

But on the present occasion we are not confining our attention to electrons only as they appear in electrolysis and the theory of chemical valence, but are prepared to take account of the much larger number of the electrons forming the substance of an atom. As the inertia of the negative

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electron in the cathode rays was found by J. J. Thomson to be from 1/500 to 1/1000 of that of a hydrogen atom, we may provisionally suppose the atom of hydrogen to contain from 250 to 500 electric doublets, whereas in electrolytic theory we assign to the hydrogen atom only one negative electron to make the hydrogen ion. It is possible, then, that the Earth's magnetism may be due to either the ionic electric charges of matter or to the electricity which forms matter. Possibly the ionic charges are only a special manifestation of the polarity which we assume to be innate in matter if we consider its atoms to be made up of electric doublets.

The fundamental equation for our theory of the Earth's magnetism gives the magnetic potential Ψ at a point r, θ, due to a sphere of radius R revolving round a central axis with angular velocity Ω and charged with electricity of surface density σ, thus

$$\Psi = 4\pi R^4 \Omega \sigma \cos \theta / 3r^2. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

For a sphere of radius R having a uniform body charge of negative electricity of density ρ the magnetic potential is

$$\frac{4}{3} \pi \Omega \frac{\cos \theta}{r^2} \int_0^R \rho R^4 dR. \quad . \quad . \quad . \quad . \quad (9)$$

This is obtained on the assumption that $e_1 e_2 / r^2$ expresses the whole law of electric force. If we take account of repulsions $(1-b)e^2/r^2$ and $(1-c)e^2/r^2$, we have to give a more complex meaning than hitherto to the electric force at a point, and therefore also to the magnetic force due to moving electricity. In the theory of magnetism due to electric convection, magnetic force at a place is taken to be proportional to the electric force exerted at that place by the moving electricity. Now the electric force of an electron e' on another e'' at distance r has three different values according to the natures of e' and e''.

If the æther contains electric doublets which give it its electric properties, and also by their rotation give it its magnetic properties, the magnetic force due to the rotation of ♯ of amount e with the Earth at the ♯ of a doublet A in the æther outside the Earth will be $1-b$ times as large as at the ♭ in A. The mean magnetic force at A due to the rotation of ♯ in the Earth is $1-b/2$ times that which would act if e^2/r^2 were the universal law. Similarly for ♭ in the Earth the magnetic force is $1-c/2$ times its value for the one law e^2/r^2. Now the Earth's magnetic field is directed in the same way as if it were caused by the rotation of negative electricity with the Earth. Hence c is algebraically less than b.

The Earth having opposite electric charges each of density ρ, their magnetic potential outside the Earth is by (9)

$$\Psi = \frac{b-c}{2} \frac{4}{3} \pi \Omega \frac{\cos \theta}{r^2} \int_0^R \rho R^4 dR. \quad . \quad . \quad (10)$$

Let ρ have an average value independent of R, then this is

$$\Psi = 2(b-c)\pi\Omega\rho R^5 \cos \theta/15r^2. \quad . \quad . \quad (11)$$

The average electric density ρ may be estimated either for the valency charges of the Earth's atoms or for the electricity forming the atoms. We are concerned now with the latter. If D is the mean density of matter in the Earth and h is the mass of an atom of hydrogen, then the inertia of an electron is of the order $h/1000$, and the number of electrons $\flat$ per unit volume is of the order $500D/h$, and so ρ is $500eD/h$.

For the average magnetic potential Ψ in electromagnetic units round a parallel of latitude $\pi/2 - \theta$ in 1880 von Bezold has given

$$\Psi = \cdot 330R \cos \theta. \quad . \quad . \quad . \quad . \quad (12)$$

Hence with V for the velocity of light we have from (11) and (12), putting $r = R$,

$$b - c = \frac{\cdot 330 \times 15Vh}{1000\pi\Omega R^2 De}. \quad . \quad . \quad . \quad (13)$$

But h/e is a fundamental electric constant having the value 345×10^{-17} and $R = 637 \times 10^6$ cm., $\Omega = 2\pi/24 \times 60 \times 60$,
$$D = 5\cdot 7, \quad V = 3 \times 10^{10}.$$

These give

$$b - c = 9\cdot 7 \times 10^{-22}, \quad . \quad . \quad . \quad . \quad (14)$$

and (7) is

$$b + c = 32 \times 10^{-44}.$$

It is noteworthy that $b + c$ is of the same order as $(b-c)^2$ nearly. As we have

$$\left. \begin{array}{l} b = 4\cdot 85 \times 10^{-22} + 16 \times 10^{-44} \\ c = -4\cdot 85 \times 10^{-22} + 16 \times 10^{-44} \end{array} \right\}, \quad . \quad . \quad (15)$$

we are invited to write with β for $2\cdot 4 \times 10^{-22}$

$$\left. \begin{array}{l} b = 2\beta + \beta^2 \\ c = -2\beta + \beta^2 \end{array} \right\}. \quad . \quad . \quad . \quad (16)$$

This makes terrestrial magnetism a first order effect due to the small fraction β, and gravitation a second order effect. This simple result invites speculation as to the origin of β.

We can write $1-b$ and $1-c$ in the forms

$$2-(1+\beta)^2 \quad \text{and} \quad 2-(1-\beta)^2. \quad . \quad . \quad . \quad (17)$$

It seems to me that we may regard the electrons as attracting or repelling one another by means of inward or outward momentum radiated with the velocity V of light, so that the force of attraction or repulsion is proportional to V^2. If something, say the æther, is radiating outwards and inwards in all directions from and to an electron with velocity V, then the mean squared relative velocity of the æther from any two electrons is V^2+V^2, similarly to the known result for the kinetic theory of gases. Now suppose this mean squared relative velocity is an absolute constant of the æther. If a positive electron imparts an additional velocity v to the æther radiating from or to it, two positive electrons can affect one another only as though each had a squared velocity $2V^2-(V+v)^2$ available for mutual action. If each negative electron imparts an additional velocity $-v$ to the æther leaving or entering it, subject to the condition that for the æther the squared relative velocity has the constant value $2V^2$, then the negative electrons have each $2V^2-(V-v)^2$ available for the communication of momentum to one another. These take the place of the V^2 which would be available if v were 0; hence the rate of communication of momentum is altered by v in the ratio $2V^2-(V+v)^2 : V^2 = 2-(1+v/V)^2$ for ♯, and in the ratio $2-(1-v/V)^2 : 1$ for ♭. Our fraction β stands thus for v/V. Probably this velocity v exists only when the electrons are coupled up as doublets, the positive electron pouring out æther like a source, while the negative absorbs it like a sink at the same rate.

For the communication of momentum between ♯ in one doublet and ♭ in another, we may make two assumptions as to the available squared velocity ; first we may make it

$$2V^2-(V+v-v)^2 = V^2,$$

a result corresponding to the law e^2/r^2 assumed at the start of this investigation : second, we may take the available amount to be

$$2V^2-(V+v)(V-v) = V^2+v^2,$$

which would correspond to a law of attraction $(1+\beta^2)e^2/r^2$ between ♯ and ♭ in place of our original e^2/r^2. This second law seems preferable, for it gives for the total available squared velocity in the four electrons of two doublets the sum

$$2V^2-(V+v)^2+2V^2-(V-v)^2$$
$$+2\{2V^2-(V^2-v^2)\} = 4V^2, \quad . \quad (18)$$

Moreover, if we revise the previous working, using the law of attraction $(1+\beta)e_1e_2/r^2$ between ♯ and ♭ in place of e_1e_2/r^2, the only effect would be to replace (7), which is equivalent to $2\beta^2 = 32 \times 10^{-44}$, by the relation $4\beta^2 = 32 \times 10^{-44}$, which makes the values of β deduced from gravitation and from terrestrial magnetism agree more closely than before, but at present we are concerned rather with agreement in the order of magnitude of the two than with strict concordance in absolute values. As β is $2\cdot4 \times 10^{-22}$, the value of v is $7\cdot2 \times 10^{-12}$.

An important point to be considered now is the non-existence of a field of electric force at the surface of the Earth comparable in importance with that of its magnetic field. Let E be the amount of positive or negative electricity forming the matter of the Earth, then, according to the preceding principles, if free electricity obeyed the laws of electricity in doublets, the attraction on a positive charge Q at the surface of the Earth would be

$$EQ\{-(1-2\beta-\beta^2)+1+\beta^2\}/r^2 = 2(\beta+\beta^2)EQ/r^2. \quad (19)$$

With the data supplied for finding ρ in (11) we can estimate E to be of the order 10^{45}, and so the above force becomes of the order 10^6Q dynes. This is quite incompatible with experience. Hence we infer that when a charge of electricity like Q becomes separated from its complement $-Q$, the electrons forming it must lose their power of giving out and taking in æther with any velocity comparable with $7\cdot2 \times 10^{-12}$ found above for v. In the case of free electricity then $\beta=0$. And β vanishes not only as regards Q, but also as regards E and $-E$ in their relation to Q. In breaking up doublets to form the two free charges Q and $-Q$, we apparently destroy the circulation of the æther which these doublets maintained, and so we withdraw their electrons from participation in the effects of the circulation maintained by other doublets. For, if we assume that in $2-(1+\beta)^2$ the factor $1+\beta$ for Q reduces to 1, while for E the factor $1+\beta$ is retained, and similarly for $-E$ the factor $1-\beta$, the force given by (19) becomes

$$EQ\{-(1-\beta)+1+\beta\}/r^2 = 2\beta EQ/r^2,$$

which is still incompatible with experience.

Thus to account for the absence of a powerful electric field accompanying the Earth's magnetic field, we must consider that the force between a free electron and any other electron, whether free or bound, at distance r is e^2/r^2

Gravitation and the Earth's magnetism are by this specula-
tion traced to the circulation of something (probably the
æther), such circulation being maintained by the doublets.
Free electricity not maintaining this particular circulation
does not partake of its effects. The theory here given for
the Earth's magnetic field may be applied to calculate that
of any heavenly body whose mass, radius, and velocity of
rotation round its axis are known. If the principles dis-
cussed in this paper are sound, they confirm the principle
that there is essentially but one form of inertia in the
universe.

Melbourne, August 1904.

LXIX. *The Determination of the Size of Molecules from the
Kinetic Theory of Gases.* By J. H. JEANS, *M.A.,
Fellow of Trinity College, Cambridge* *.

§ 1. THERE are five ways in which it is possible, in theory,
to determine the size of molecules. These are
from observations :—

    (i.) On the viscosity of gases.
    (ii.)   ,,   conduction of heat by gases.
    (iii.)   ,,   coefficient of diffusion of gases.
    (iv.)   ,,   deviations from Boyle's law.
    (v.)   ,,   volume occupied by matter in the solid
                     and liquid states.

Of these the last can hardly be regarded as of any value
except as fixing an upper limit to the size of the molecules.
For we can make no definite inference from observations of
this kind except on the supposition that the molecules are
packed as closely as is physically possible, and this suppo-
sition we know to be erroneous. At the same time, since we
know in which direction the error lies, the observations in
question are of interest as fixing a limit in one direction.

The quantities which can be deduced from these five classes
of observations depend, however, not only upon the size of
the molecules but also upon the constant N, which measures
the number of molecules per c.c. in a gas at normal tempe-
rature and pressure. If, for the moment, we regard all
molecules as spheres of diameter σ, we find that any of the
first three kinds of observations enable us to determine $N\sigma^2$,
while either of the last two give $N\sigma^3$.

Clearly, if we take one set of observations of either class,

* Communicated by the Author. Read before the British Association
at Cambridge, August 23, 1904.

we can eliminate N, and so determine σ. Conversely, of course, we could, if we wished, eliminate σ and determine N.

§ 2. Let us begin by considering the determination of N. The best observations in the first three classes are probably those on the viscosity of air. The best in the last two classes are those on the deviations of air from Boyle's Law.

As regards observations on the viscosity of air, the coefficient of viscosity at normal temperature and pressure is given in Landolt and Börnstein's tables as ·0001714, this being a mean value derived from the consideration of a large number of experiments. Meyer ('Kinetic Theory of Gases,' p. 190), also considering a great number of experiments, takes the value ·000172. The usual formula for the coefficient of viscosity derived from the Kinetic Theory of Gases is

$$\kappa = ·350 \rho \bar{c} l, \quad . \quad . \quad . \quad . \quad . \quad \text{(i.)}$$

where l is the mean free path, $\bar{c}$ the mean velocity, and ρ the density of the gas.

We may take the values of ρ and $\bar{c}$ at normal temperature and pressure to be

$$\rho = ·001293$$
$$\bar{c} = 45100 \text{ cm. per sec.}$$

The value of l, as will be proved in the paper immediately following this, must be taken to be

$$\frac{1·255 . .}{\sqrt{2\pi N \sigma^2}},$$

differing from Maxwell's value by the presence of the factor 1·255 .. in the numerator.

Substituting these values in formula (i.), we obtain

$$·0001714 = ·350 \times ·0001293 \times 48500 \times \frac{1·255}{\sqrt{2\pi N \sigma^2}},$$

from which we derive

$$N\sigma^2 = 3306 \text{ sq. cms.} \quad . \quad . \quad . \quad . \quad . \quad \text{(ii.)}$$

Turning to deviations from Boyle's Law, Van der Waals [*] deduces from Regnault's experiments the value

$$b = ·00198,$$

where b is the usual b of Van der Waals' equation referred to normal temperature and presssure, the value of b being

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[*] 'Continuity of the Liquid and Gaseous States,' p. 400 (English trans.).

$\frac{2\pi}{3} N\sigma^3$. For the same quantity Rose-Innes [*] deduces from some work of Callendar the value

$$b = \cdot 00209.$$

If we take b to be the mean of these, we obtain

$$\frac{2\pi}{3} N\sigma^3 = \cdot 002035,$$

so that

$$N\sigma^3 = \cdot 00097 \text{ cubic cm.} \quad . \quad . \quad . \quad . \quad \text{(iii.)}$$

From relations (ii.) and (iii.) we obtain

$$N = \frac{(N\sigma^2)^3}{(N\sigma^3)^2} = 4 \cdot 92 \times 10^{19} \quad . \quad . \quad . \quad . \quad \text{(iv.)}$$

This is the value of N deduced by the kinetic theory from observations on a single gas, air. From calculations on a variety of other gases, I have found values of N differing slightly from the foregoing, the average value being perceptibly lower—about $4 \cdot 6 \times 10^{19}$.

§ 3. There is, however, no question that the value of N deduced from the kinetic theory is not likely to be accurate. A glance at equation (iv.) will show that before the kinetic theory can arrive at a value of N, it has to double the errors of observation, already large, in the determination of b, and to treble those in the determination of the coefficient of viscosity.

A better way of determining N is by electrical methods, since N can be determined at once from the ionic charge. By this method there is no multiplication of observational errors, and, moreover, there is no assumption necessary as to the *shape* of the molecules. For the ionic charge we have the following determinations :—

J. J. Thomson	....	$3 \cdot 4 \times 10^{-10}$.
H. A. Wilson	....	$3 \cdot 1 \times 10^{-10}$.
J. S. Townsend	....	$3 \cdot 0 \times 10^{-10}$.

The value for the ionic charge deduced from the value $N = 4 \cdot 6 \times 10^{19}$ given above would be about

$$2 \cdot 7 \times 10^{-10}.$$

If we determine N from the three above observations, we obtain respectively

$$N = 3 \cdot 6 \times 10^{19}, \ 4 \cdot 0 \times 10^{19}, \ 4 \cdot 1 \times 10^{19}.$$

[*] Phil. Mag. v. p. 48.

§ 4. As regards the determination of σ, it is at once clear that the kinetic theory stands in a better position. From the relations (ii.) and (iii.), we find for air,

$$\sigma = \frac{(N\sigma^3)}{(N\sigma^2)} = 2 \cdot 7 \times 10^{-8} \text{ cms.}$$

Here we notice there is no multiplication of errors, so that we should expect the error of the result to be only one-half or one-third of the error in N.

This is the best determination of which the kinetic theory, by itself, is capable. If, however, we call in outside assistance from electrical observations, the case is much improved.

§ 5. Let us agree to adopt the value

$$N = 4 \times 10^{19}$$

from the electrical observations already mentioned, then we can use the formulæ

$$\kappa = \cdot 350 \rho \bar{c} \frac{1 \cdot 25}{\sqrt{2\pi N \sigma^2}}, \quad . \quad . \quad . \quad . \quad \text{(v.)}$$

$$b = \tfrac{2}{3}\pi N \sigma^3,$$

for separate determinations of σ. It is clear that in these determinations of σ the error of observation in κ is reduced to one-half, that in b to one-third, and those in N to one-half and one-third respectively, in the two cases. The error in the determination of σ ought, therefore, to be only about one-sixth of that in our previous determination of N.

We have already found that for air,

$$N\sigma^2 = 3306 \text{ sq. cm.}$$

If we take $N = 4 \times 10^{19}$, we find

$$\sigma = 2 \cdot 86.$$

If m is the molecular weight of any gas, then ρ and $\bar{c}$, referred to normal temperature and pressure, are proportional respectively to m and $m^{-\frac{1}{2}}$, so that as regards κ and m, σ is proportional to

$$\kappa^{-\frac{1}{2}} m^{\frac{1}{4}}.$$

From the value of σ already obtained for air, we can deduce the values for other gases, the results being given in the following table. The values of κ are taken, some from Landolt and Börnstein's tables, and some from recent observations.

Values of σ deduced from Viscosity.

Gas.	κ.	$\sigma \times 10^8$.
Hydrogen	·0000864	2·05
Helium	·000165	1·81
Water-vapour	·000093	3·39
Carbon monoxide...	·0001628	2·90
Ethylene	·0000944	3·77
Nitrogen	·0001647	2·90
Air....................	·0001714	2·86
Nitric oxide	·000168	2·82
Oxygen	·0001873	2·81
Argon	·000208	2·79
Carbon dioxide......	·0001431	3·47
Nitrous oxide	·0001381	3·54
Ethyl chloride	·000094	4·68
Chlorine	·000128	4·11

§ 6. As regards conduction of heat, the formula, as will be explained in the next paper, is

$$\theta = 1\cdot6027 \kappa c_v,$$

where c_v is the specific heat at constant volume, and κ is simply an abbreviation for the right-hand member of equation (v.). This leads to the following table of values*.

Values of σ deduced from Conduction of Heat.

Gas.	θ (mean).	$\sigma \times 10^8$.
Hydrogen............	·000353	1·99
Nitrogen	·0000512	2·74
Ethylene	·0000395	3·88
Carbon monoxide...	·0000498	2·74
Air.....................	·0000519	2·72
Nitric oxide	·0000451	2·81
Oxygen	·0000551	2·58
Carbon dioxide......	·0000316	3·58
Nitrous oxide	·0000351	3·48

§ 7. As regards the calculation of σ from coefficients of diffusion, I have been content to examine only the four gases

* I have taken Regnault's values for c_v, and the value used for θ is the mean of three sets of experiments by Winkelmann, Graetz, and Wüllner, given in Landolt and Börnstein's Tables, and in Meyer's 'Kinetic Theory of Gases,' English Translation, pp. 293, 295.

of which Maxwell has calculated the coefficients of self-diffusion from Loschmidt's experiments. These are given in Lord Kelvin's Baltimore Lectures *.

The formula for the coefficient of self-diffusion, corrected as explained in the paper following, is

$$D = 1.34\frac{\kappa}{\rho}.$$

From the four values of D quoted by Lord Kelvin, I have calculated the following values for σ.

Values of σ deduced from Coefficients of Diffusion.

Gas.	D.	σ.
Hydrogen	1·31	2·03
Oxygen	·189	2·71
Carbon monoxide...	·174	2·92
Carbon dioxide......	·109	3·27

§ 8. This completes the series of determinations of σ from free-path phenomena. For the values obtained from observed deviations from Boyle's Law, we have the formula

$$b = \tfrac{2}{3}\pi N\sigma^3.$$

In the following table the values of b are taken either from Van der Waals' discussion of Regnault's experiments, or from Rose-Innes' discussion of Callendar's experiments. Wherever possible, a mean of two sets has been taken :—

Values of σ deduced from deviations from Boyle's Law.

Gas.	b (mean).	$\sigma \times 10^s$.
Hydrogen	·00074	2·05
Nitrogen	·00255	3·12
Air.........	·00203	2·90
Carbon dioxide......	·00228	3·00

* Page 295, or Phil. Mag. iv. p. 194.

§ 9. Collected together for the sake of reference the numbers stand as follows :—

Values of $\sigma \times 10^8$.

Gas.	Viscosity.	Conduction of Heat.	Diffusion.	Boyle's Law.	Mean.
Hydrogen	2·05	1·99	2·03	2·05	2·03
Helium	1·81	...	...	...	1·81
Water-vapour ...	3·39	...	...	...	3·39
Carbon monoxide	2·90	2·74	2·92	...	2·86
Ethylene	3·77	3·88	...	...	3·81
Nitrogen	2·90	2·74	...	3·12	2·91
Air..................	2·86	2·72	...	2·90	2·84
Nitric oxide	2·82	2·81	...	...	2·82
Oxygen	2·81	2·58	2·71	...	2·73
Argon	2·79	...	...	..	2·79
Carbon dioxide...	3·47	3·58	3·27	3·00	3·36
Nitrous oxide ...	3·54	3·48	...	...	3·52
Ethyl chloride ...	4·68	...	...	...	4·68
Chlorine	4·11	...	...	...	4·11

In calculating the mean values of the last column, I have assigned double weight to viscosity determinations, as these are probably more reliable than the rest.

As regards the magnitude of error to be expected in the results, some indication is given by the amount of difference between the different determinations of σ for the same gas. It will be seen that there is no entry in which the error differs by as much as 10 per cent. from the mean value given in the last column, except in the case of one entry for carbon dioxide.

It would, of course, be possible to arrive at this result in any table, by an appropriate choice of the calculations tabulated. It may, therefore, be well to mention that every calculation which I have started has been carried through, and the result tabulated, with one single exception to be explained later. The particular gases worked at have been selected partly on account of their importance, and partly on account of the amount of experimental evidence available. The single gas for which the results have not been tabulated is mercury-vapour. I found that viscosity experiments led to a value $\sigma = 4·8 \times 10^{-8}$; that this result differed widely from that obtained from the conduction of heat; and finally that it was directly contradicted by calculations from the density of the solid. The density of solid mercury is given by Mendeleef

as 14·39, from which it follows that the diameter of the mercury molecule cannot exceed $2·6 \times 10^{-8}$. On finding that the determinations of the viscosity of mercury are regarded as uncertain by Meyer (Kinetic Theory, pp. 197, 295), I thought it legitimate to omit mercury-vapour from the table.

§ 10. The values given in the table are in good agreement with the upper limit set by the density in the solid and liquid states. The density of solid hydrogen at $-256°$ C. is about 850 times that of the gas, from which the upper limit for σ is found to be $3·47 \times 10^{-8}$. For oxygen the similar quantity is $3·0 \times 10^{-8}$. The density of solid nitrogen is 1114 times that of the gas, leading to the limit $\sigma = 3·5 \times 10^{-8}$, while from the density of solid argon ($1·396$) we can deduce the limit $\sigma = 3·56 \times 10^{-8}$.

The maximum density of water occurs in the liquid state at $4°$ C., and this leads to the limit $\sigma = 3·05 \times 10^{-8}$, a limit which is *below* the value $3·39 \times 10^{-8}$ given in the table above. The same occurrence, then, as was experienced with mercury-vapour finds place again in the case of water-vapour. It may be that the error affects all vapours, and that it is not legitimate, even as an approximation, to treat a vapour as a perfect gas (*cf.* Meyer, Kinetic Theory of Gases, p. 221 *et seq.*).

On the other hand, it is certainly illegitimate to treat molecules as elastic spheres, and the fact that we have assumed all molecules to be spheres must have introduced a large possibility of error. So long as we are dealing only with free path and collision formulæ, the error will affect all quantities about equally—we can, in fact, define σ as the diameter of a sphere such that spheres of this diameter would undergo the same number of collisions as take place in the actual gas. But in considering the solid state, we are no longer dealing with free paths and collisions, so that if we continue to regard molecules as spheres a new definition of σ becomes necessary. In fact, we define σ now as the diameter of a sphere which occupies the same space as the molecule, and the more the molecules differ from the spherical shape, the more this value of σ will differ from the former value.

We cannot therefore be surprised at some difference between the values obtained by the consideration of the solid state, and those obtained by the other methods we have used.

The paper following may be regarded as an appendix to the present paper, explaining the calculation of the formulæ which have been used in the present paper.

LXX. *The Persistence of Molecular Velocities in the Kinetic Theory of Gases.* By J. H. JEANS, *M.A., Fellow of Trinity College, Cambridge* *.

§ 1. IN the Kinetic Theory investigation of the phenomena of viscosity, conduction, and diffusion of gases, it is usual to assume that a collision may be supposed, so to speak, to wipe out the whole previous history of a molecule, so that the " expectation " of any quantity which can be associated with a molecule is the mean value of that quantity at the point at which the last collision occurred. It will be shown in the present paper that this assumption cannot be justified. If a molecule now at P is supposed to have last collided at Q, it will be shown that on tracing back the history of the molecule, the probability is that it will be found to have come initially from a point beyond Q in the direction away from P, so that the " expectation " of any quantity to be associated with the molecule will not be that appropriate to the point Q, but will be appropriate to some point beyond Q.

We may begin with a theorem due to Maxwell †. If two molecules collide with velocities such that their centre of gravity is at rest before collision, and therefore of course after collision also, then all directions are equally probable

Fig. 1.
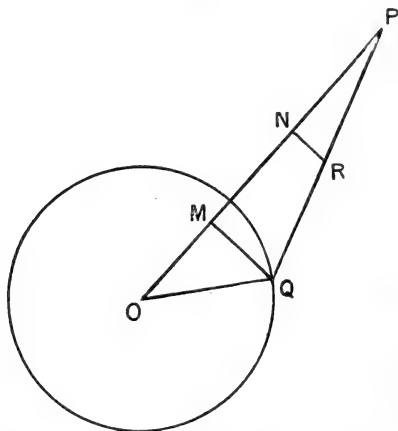
for the velocity of either after collision. From this it follows that if two molecules collide in any way, the " expectation " of the velocity of either is exactly equal to the velocity of the centre of gravity of the two. In figure 1, let OP and

OQ represent the two velocities before collision, so that PQ represents the relative velocity. If we suppose the two molecules to be of equal mass, the velocity of the centre of gravity will be represented by OR, where R is the middle point of PQ. Thus if the two molecules have velocities represented by OQ, OP before collision, the expectation of velocity of the former after collision is represented by OR.

If the magnitude, but not the direction, of the velocity of the second molecule is known, we can find the expectation of the final velocity of the first molecule by averaging the components of the velocity OR over all possible directions for OQ. We must not take all directions for OQ to be equally probable, for the number of collisions occurring between molecules having specified resultant velocities is proportional to the relative velocity of the two molecules.

It is at once clear on averaging the components of the velocity OR, that the averaged components perpendicular to OP vanish. We are therefore left with the single component in the direction OP of which the average value is

$$\frac{\int_0^\pi ON . PQ \sin\theta\, d\theta}{\int_0^\pi PQ \sin\theta\, d\theta}, \qquad . \quad . \quad . \quad . \quad \text{(i.)}$$

where θ is the angle POQ.

To evaluate this fraction, let us write $OP = a$, $OQ = b$, $PR = r$, so that $r^2 = a^2 + b^2 - 2ab \cos\theta$, and

$$ON = \tfrac{1}{2}(OP + OM)$$
$$= \tfrac{1}{2}(a + b \cos\theta)$$
$$= \frac{1}{4a}(3a^2 + b^2 - r^2).$$

We also have $r\,dr = ab \sin\theta\, d\theta$, so that the fraction (i.) becomes

$$\frac{\int (3a^2 + b^2 - r^2) r^2\, dr}{\int 4ar^2\, dr},$$

the limits of integration being from $r = a + b$ to $r = a \sim b$.

When $a > b$, the fraction is found to be equal to

$$\frac{15a^4 + b^4}{10a(3a^2 + b^2)},$$

and when $a < b$, it is equal to

$$\frac{a(5b^2 + 3a^2)}{5(3b^2 + a^2)}.$$

The essential point to be noticed is that these expressions are necessarily positive for all values of a and b, so that whatever the velocity of the second molecule may be, the expectation of the velocity of the first molecule after collision is definitely in the same direction as its velocity before collision. If α is the expectation of velocity in the same direction as a, the formula just found leads to the following table of values :—

$$\frac{b}{a} = \quad 0, \quad \tfrac{1}{2}, \quad 1, \quad 1\tfrac{1}{2}, \quad 2, \quad 3, \quad \infty.$$

$$\frac{\alpha}{a} = \cdot500, \quad \cdot473, \quad \cdot400, \quad \cdot368, \quad \cdot354, \quad \cdot343, \quad \cdot333.$$

We have now proved the tendency for the original velocity of a molecule to persist after collision. If we define the ratio $\dfrac{\alpha}{a}$ to be the " persistence," the table just given shows that the persistence is measured by a quantity which varies from $\tfrac{1}{3}$ to $\tfrac{1}{2}$ of the original velocity, according to the velocity of the colliding molecule.

§ 2. By averaging over all possible velocities for the colliding molecule, we can obtain the mean value of this persistence averaged over all collisions.

The proportion of collisions for which the greater velocity stands to the less in a ratio between k and $k + dk$ ($k > 1$) is easily found to be

$$\frac{5k(3k^2 + 1)}{\sqrt{2}(1 + k^2)^{7/2}} \, dk; \quad . \quad . \quad . \quad . \quad . \quad \text{(ii.)}$$

and the values of the persistence for the two molecules taking part in such a collision are respectively

$$\frac{15k^4 + 1}{10k^2(3k^2 + 1)} \quad \text{and} \quad \frac{5k^2 + 3}{5(3k^2 + 1)}.$$

The mean persistence for the two molecules, *i. e.* the mean of the two foregoing expressions, is

$$\frac{25k^4 + 6k^2 + 1}{20k^2(3k^2 + 1)}.$$

Multiplying this by expression (ii.) and integrating from $k = 1$ to $k = \infty$, we find for the mean persistence of all velocities

after collision,

$$\int_1^\infty \frac{(25k^4 + 6k^2 + 1)}{\sqrt{z}(1 + k^2)^{7/2}}\, dk,$$

of which the value is found to be

$$\frac{1}{4}\left(1 + \frac{1}{\sqrt{2}}\log_e(\sqrt{2} + 1)\right),$$

or ·406.

§ 3. If we denote this quantity by θ, we see that, roughly speaking, a molecule which has travelled a distance ζ in a given direction since its last collision, may be expected to have travelled a distance $\theta\zeta$ on its previous free-path, $\theta^2\zeta$ on the free-path preceding this, and so on. It may therefore be supposed to have travelled a total distance

$$\zeta + \theta\zeta + \theta^2\zeta + \dots \qquad \dots \qquad \text{(iii.)}$$

in this direction, and the value of this series is $\dfrac{\zeta}{1 - \theta}$.

§ 4. Thus, in the diffusion of gases, we must regard the "free-path" of the usual formula as of length $(1 - \theta)^{-1}$ times that contemplated by the simple theory. Putting $\theta = ·406$ we find that $(1 - \theta)^{-1} = 1·684$.

§ 5. In the problems of viscosity and conduction the case stands differently. Diffusion may be regarded as a transport of qualities, and velocity and conduction as a transport of quantities. There is the essential difference that the qualities associated with a molecule—its chemical composition in the present instance—cannot be affected by collision, whereas the quantities associated with a molecule—quantities of energy and momentum respectively when dealing with conduction and viscosity—are altered by collisions. As a simple hypothesis, we may suppose that at each collision, half of the excess of either quantity is imparted to the colliding molecule. We then replace expression (iii.) by

$$\zeta + \tfrac{1}{2}\theta\zeta + \tfrac{1}{4}\theta^2\zeta + \dots.$$

of which the value is $\dfrac{\zeta}{1 - \tfrac{1}{2}\theta}$. The simple free-path must therefore be multiplied by a factor $(1 - \tfrac{1}{2}\theta)^{-1}$, of which the value, on taking $\theta = ·406$, is found to be $1·2547$.

This correcting factor leads to the formulæ used in the preceding paper.

LXXI. *A Contribution to the Theories on Osmosis, Solubility, and Narcosis.* *By* Professor I. TRAUBE, *Berlin* *.

IN a series of very able researches† E. Overton has determined, by plasmolytic and other methods, the velocity with which chemical compounds diffuse into the protoplast.

Among the compounds which penetrate the cell-walls *rapidly* are the monovalent alcohols, aldehydes, ketones, aldoximes, ketoximes, mono- di- and trihalogen carbohydrates, nitroalkyls, alkyl cyanides, the neutral esters of the inorganic and of many organic acids, aniline, pyridine, and the greater number of the alkaloids when in a free state, and the natural colouring-matters, *i. e.* those soluble in alcohol.

The bivalent alcohols and the amides of the monovalent acids diffuse *less rapidly* ; whilst glycerol, urea, thio-urea, and erytritol diffuse more slowly still. The hexavalent alcohols, hexoses, and amido-acids scarcely diffuse at all ; while the cells are *completely impervious* to the salts of the strong inorganic acids, and to inorganic acids and bases. It is always observed that permeability increases in homologous series, and also by the substitution of a hydrogen-atom by methyl, of methyl by ethyl, and so on. Thus there can be no question of any such sieving action as M. Traube supposes the membranes to exercise.

Starting from a well-known experiment by Nernst‡, and from the circumstance that, in general, compounds penetrate membranes the more rapidly, the more soluble they are in such substances as fats, cholesterine, lecithine, &c. (*i. e.* the more *lipoid-soluble* they are), Overton evolves the theory *that the magnitude of the distribution coefficient between fat, &c., on the one hand, and water on the other, determines the velocity of the osmosis.* He assumes that, in the first instance, a dissolution takes place in the fatty substance of the membrane at a velocity proportionate to this coefficient, and that thereupon the substance is passed on from the membrane to the interior of the cell.

Again, Overton—and, independently, Hans Meyer§—draws attention to the notable circumstance, *that all the reliable narcotics, anæsthetics, and antipyretics are rapidly diffusing substances, and from this they deduce a theory, according to which the efficacy of a narcotic depends principally on its lipolidic solubility.* These experiments, and above all Overton's

* Communicated by Prof. W. J. Pope, F.R.S.
† Overton, *Vierteljahresschr. d. Naturf. Gesellsch. Zürich,* xl. p. 1 (1895), and xliv. p. 88 (1899) ; *Zeitschr. physik. Chem.* xxii. p. 189 (1897).
‡ Nernst, *Zeitschr. physik. Chem.* vi. p. 37 (1890).
§ Overton, *Studien über die Narkose,* Jena, Fischer (1901).

theories, have very just1y met with general notice and
attention ; and indeed these theories shed light on a number
of extremely important physiological, biological, pharmaco-
logical, and medico-therapeutic problems, so that it is scarcely
surprising that even certain weak points in the theory have
been hitherto overlooked.

And yet, interesting as are Overton's experiments, and
ingenious as his theories appear, they are nevertheless erro-
neous in so far as they concern osmotic velocity, as I shall now
proceed to prove.

The weightiest objections to Overton's theory are as
follows *:—

The fact that osmotic velocity goes parallel with lipoid-
solubility is doubtless worthy of notice. But if—as Overton
assumes—there must be dissolution into the lipoid substance
of the cell-wall before the substance can pass on further,
then even the most rapid osmosis would be a slow procedure.
But even admitting dissolution into the lipoid substance as
comprehensible, it is impossible to understand why the
lipoid substance should not hold this dissolved substance all
the more firmly, the more forcibly it has attracted it; *i. e.*,
why the substance should be passed on to the interior of the
cell in direct proportion to the degree of its solubility. It is
above all impossible to understand how water can penetrate
membranes more rapidly than any other substance, since in
its case there can be no question of great solubility in lipoid
substances. It is even demonstrable in the case of kidney
secretions and other secretory phenomena, that among other
substances salts penetrate into the interior of the cell, a fact
irreconcilable with Overton's theory ; further, the experi-
ments which have been made with diaphragms point to the
conclusion that it is *not* the content of lipoid which determines
the sequence of osmotic velocity.

Though these negative objections are weighty enough, the
observations that now follow establish still more conclusively
the fallacy of Overton's and H. Meyer's theories, in so far as
they concern the causes of osmotic velocity.

My own long-continued researches on the constants of capil-
larity of substances—especially solutions †—have led to the
following simple result:—*The greater the osmotic velocity of a
substance soluble in water, the more this substance reduces the
capillarity constant of water. Substances that cannot penetrate*

* *Note during the time of correction*: My ideas concerning these
objections need a certain restriction ; compare my papers in Pflüger's
Archiv d. Physiologie, Nov. or Dec. 1904.

† Compare especially *Ber. d. d. chem. Ges.* xvii. p. 2294 (1884) ; *Journ.
prakt. Chem.* xxxi. p. 177 (1885), and *Ann. Chem. Pharm.* cclxv. p. 27.

membranes (*with regard to which the membranes are semi-permeable*) *increase the constant * of capillarity of water.*

My former publications †, as well as researches not yet published, show, *that among hundreds of combinations examined by Overton plasmolytically and by myself as to capillarity, there is* NOT ONE *in which capillary and osmotic phenomena do not correspond.*

The curves on p. 707 illustrate the Constants of Capillarity of some characteristic substances dissolved in water.

Considering the close connexion between *capillarity*, *surface-tension*, and *internal pressure*, we are fully justified in picturing to ourselves the osmotic process in the manner illustrated by the accompanying figure.

Let M be a membrane, and A and B two liquids. The surfaces of both liquids will be drawn inwards with a force which is determined by the internal pressure or by the surface-tension. If the surface-tension of A be greater than that of B, B will penetrate through the membrane, or have the power to do so.
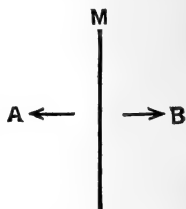
The difference of the surface-tensions determines the direction and velocity of the osmosis.

The internal pressure and the surface-tension of salt-solutions and of cane-sugar, urea, &c., are greater than those of water. Consequently, the production of semi-permeable membranes for these solutions presents no serious difficulties. But almost all not-salt-like substances diminish the capillarity and surface-tension of water on dissolution. In my previous papers ‡, substances such as albumen, gelatine, dextrine, sugars, urea, tartaric acid, oxalic acid, &c., have already been specified as exercising only *a small influence on capillarity* (kapillar inaktiv); none of these substances appreciably lowers the capillary height of water, even in concentrated solutions. Substances such as the alcohols, ethers, esters, fatty acids, &c., have *a strong influence on capillarity* (kapillar aktiv). The higher members of homologous series, if soluble, often lower the capillary height of water very considerably, whilst compounds such as ethylene alcohol, glycerol, &c., occupy a medium position between the active and the inactive substances as regards capillarity (compare the curves).

* The product of capillary height and density is the constant of surface-tension chiefly important in our case. But for substances with a strong influence on capillarity, especially in diluted solutions, the capillary height may be taken into account without fear of any grave inaccuracy.

† Cf. *Ann. Chem. Pharm.* cclxv. p. 27.

‡ I. Traube, *Ber. d. d. chem. Ges.* xvii. p. 2294 (1884), and Bodländer and Traube, *Ber. d. d. chem. Ges.* xix. p. 1871 (1886).
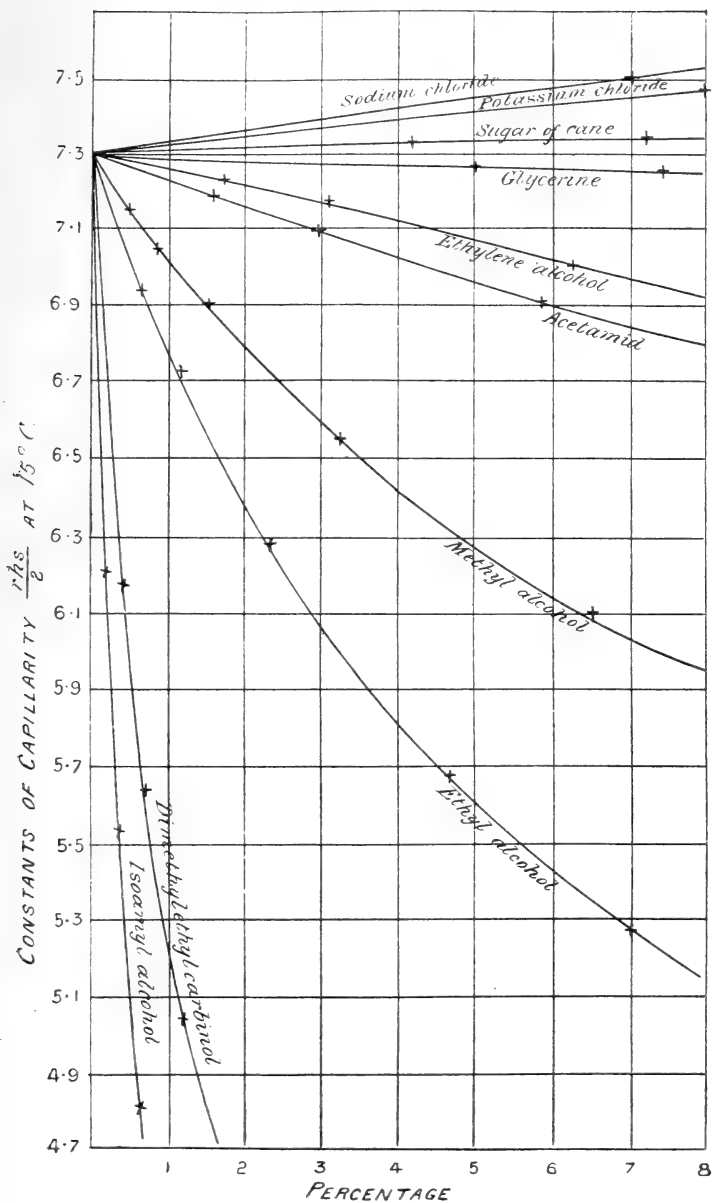
It is evident that osmotic velocity and surface-tension run parallel. *Hence the difference of the surface-tensions* (der Oberflächendruck *) *is the motive force in osmotic phenomena; it is to this difference that osmotic pressure is due.*

The origin of osmotic pressure is thus at last brought to light, and proved to be of the nature which the author, in company with Lothar Meyer †, has maintained from the beginning of the discussion, in spite of much unfriendly criticism. The migration of water into sugar solutions, as also the pressure which is thereby set up, are accounted for by the difference between the surface-tensions, and by nothing else. It is now evident that the assumption, which regards the sugar-molecule as alone responsible for the osmotic pressure, was not justified ‡. *It must, however, be emphasized that the difference between the surface-tensions cannot be considered as equal to the osmotic pressure, for this new pressure is quite different for isosmotic solutions*; it is in the first instance only the measure of the velocity with which the osmotic pressure rises, and this osmotic pressure is only a counterforce.

From the foregoing it is evident that the idea of a "semi-permeable membrane" is anything but an absolute one. A membrane which is semipermeable to aqueous solutions of salts, sugar, and at most such substances as urea, &c., will not be so to aqueous solutions of ethyl or amyl alcohol. And it may be assumed (but this must nevertheless be established by separate experiments) that a membrane impervious, for instance, to sodium chlorate from an aqueous solution, becomes pervious to the same, if a substance which lowers the surface-tension considerably is added to the water. This I intend to determine by experiment, and expect it to prove of the greatest importance, from both the physiological and the biological point of view.

The theory here stated is capable of wide generalization. It leads to a new conception of the phenomena of *diffusion* and *dissolution*.

Thus let the membrane separating an aqueous salt solution from pure water be eliminated; the phenomena of diffusion will then take place. According to the prevailing

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* Compare Pflüger's *Archiv d. Physiol.* Nov. or Dec. 1904.

† Lothar Meyer, *Zeitschr. physik. Chem.* v. p. 23 (1890), and I. Traube, *Ann. Phys. u. Chem.* lxii. p. 490 (1897).

‡ Perhaps a similar fate may be in store for certain other hypotheses concerning solutions, and the sceptics be justified in the end! Compare I. Traube, *Grundriss Physik. Chem. Enke,* Stuttgart, 1904, p. 194, and this Journal, viii. p. 158 (1904),

theory of diffusion, the salt particles or the ions, in virtue of their "osmotic pressure," migrate into the pure solvent. According to the conception of osmotic phenomena set forth here, it is not the particles of the salt solution that migrate into the pure solvent; but, on the contrary, *it is the pure solvent which, by virtue of its low surface-tension, migrates into the salt solution.* This conception of the phenomena of diffusion leads to significant physiological consequences [*].

Further, if two different liquids, capable of mutual dissolution, be brought into contact, or if a solid substance be in contact with a solvent, *the " solution-tension " will depend chiefly on the difference between the surface-tensions.*

It is now clear why a much greater quantity of ether or amyl alcohol dissolves in water, than of water in ether or amyl alcohol. Further, as Hulet's [†] experiments have demonstrated, a coarse powder does not act in the same way as a fine powder when treated with solvent.

On the basis of this theory of solubility, it is to be anticipated that when a liquid or a solid substance dissolves in a solvent, the surface-tension of the solution will never fall below that of the dissolving substance. For if, through the increasing concentration of the solution, the surface-tension of the solution should approach nearer and nearer to that of the pure solvent, a point will finally be reached at which the surface-tensions of solution and dissolving substance are equal. *And then the solution will be saturated.*

If, for instance, methyl and ethyl alcohol, in whatever proportion, are not " miscible " with water, but are *soluble* in it, it must be concluded that the constants of capillarity of all the solutions should be without exception greater than those of the pure alcohols : this is actually the case. On the other hand, the fact that amyl alcohol is only sparingly soluble in water must be attributed to the amyl alcohol lowering the capillary height of water to such an extent, that the saturated solution has very nearly the capillary height of pure amyl alcohol.

The following Table illustrates these contentions. t is the temperature of observation, h_1 the capillary height of the saturated aqueous solution, h_2 that of the organic substance saturated with water. The capillary height of water in the tubes used was 92·5 mm. at 18° C. From these data may be calculated the absolute constant, which does not concern us here.

* Compare Pflüger's *Archiv d. Physiol.* Nov. or Dec. 1904.
† Hulet, *Zeitschr. physik. Chem.* xxxvii. p. 385 (1901).

	$t.$	$h_1.$	$h_2.$
	°	mm.	mm.
Isobutyl alcohol	20	31·5	33·9
Isoamyl alcohol	,,	33·3	33·7
Ethyl ether	,,	<34·5	29·6
Aniline	,,	55·0	50·5
Methyl acetate.........	18	34·7	33·5
Ethyl acetate	,,	<38·5	34·5
Propyl acetate	,,	<46·9	34·9
Isobutyl acetate	,,	<45·2	34·25
Isoamyl acetate	,,	<44·4	34·9
Ethyl alcohol + benzene + water	,,	37·5	37·3
Isobutyl alcohol + aniline + water	,,	45·5	44·5
Chloroform	19	82·5	27·95
Ethyl iodide	,,	87·0	18·5
Ethylene chloride...................................	,,	80·25	32·55

As the numbers given for the esters and other compounds (chloroform, ethylene chloride, &c.) show, the capillary height of the solution is not always equal to that of the dissolved substance ; but, except for the small difference in the case of isobutyl alcohol, it is never lower ; and even this is no exception, if we take the real constant of surface-tension, *i. e.* the product of capillary height and density instead of the capillary height. On the other hand, the capillary heights of the ester solutions, particularly of those of comparatively high molecular weight, have probably been read at considerably above their true position, since by reason of the low dissolving tension of these esters, and the consequent incessant evaporation, constant rising goes on in the superficial layer.

Thus we see that solubility, solution-tension, and surface-tension are all in the closest mutual relationship.

The highest attainable value of the surface-tension of a saturated solution is that of the dissolved substance. Consequently the surface-tension of the dissolved substance determines the shape of the curve of the surface-tension of solutions.

If we apply the interpolation formula

$$\gamma = a + bq \quad \text{or} \quad \gamma = a + bq + cq^2,$$

where γ is the surface-tension of an aqueous solution containing a percentage of water q, then a will stand fairly approximately for the surface-tension of the pure substance, if the content of water is $q = 0$. This explains the fact, mentioned in former publications of mine, that the solutions of salts and of the greater number of solid organic substances are inactive as regards capillarity, *i. e.*, do not appreciably lower the capillary height of water. The reason is, *that solid substances have a higher surface-tension than liquids.*

Since the constants of capillarity for isomeric substances differ but little, and since moreover the capillarity of the saturated solution depends chiefly on the constants for the pure substance, the more soluble substance must generally have the higher constant of capillarity. And this is actually the case. As, for instance, with propyl alcohol and isopropyl alcohol; isoamyl alcohol and dimethylethyl carbinol; propyl aldehyde and acetone; butyric acid and isobutyric acid.

The connexion between *solution-tension* and *capillarity* is clearly more close than that between solubility and capillarity. Methyl and ethyl alcohols are both equally soluble in water; but the solution-tension of methyl alcohol is considerably greater than that of ethyl alcohol. The two can be compared quantitatively.

In the course of former investigations I was able to find empirically an extremely simple law,—since corroborated by others [*]. It is that

Equal equivalents of substances belonging to homologous series (ordinary alcohols, fatty acids, esters, &c.) exercising a strong influence on capillarity, lower the capillary height of water in the proportion $1 : 3 : 3^2 : 3^3 \ldots$

Some new values may be here quoted in illustration of the law, supplementing those which I have previously given. The specimens (supplied by Kahlbaum) were, when necessary, shaken up with soda, and repeatedly fractionated over anhydrous sodium sulphate.

	Concentration in equivalents of the aqueous solution.	Capillary height at 18° C.
Water	....	92·5 mm.
Methyl acetate	1 normal	58·1 ,,
Ethyl acetate	$\frac{1}{3}$,,	58·0 ,,
Propyl acetate	$\frac{1}{9}$,,	57·7 ,,
Isobutyl acetate	$\frac{1}{27}$,,	58·8 ,,
Isoamyl acetate	$\frac{1}{81}$,,	59·9 ,,
Isobutyric acid	$\frac{1}{3}$,,	57·2 ,,

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[*] I. Traube, *Ann. Chem. Pharm.* cclxv. p. 27, and Forch, Wied, *Ann. Phys.* lxviii. p. 810 (1899).

The example of isobutyric acid shows that even the fatty acids react as do the isomeric esters *.

If three molecules of methyl alcohol reduce the surface-tension of water as much as one molecule of ethyl alcohol, we are justified in concluding that the tendency to enlarge the surface of water is three times less in the case of methyl alcohol than in the case of its nearest homologue. Hence the tendency of methyl alcohol to separate from the solution may be considered as three times less than the corresponding tendency of ethyl alcohol. Or, in other words, *the solution-tension of the homologous substances exercising a strong influence on capillarity increases with increasing molecular weight in the proportion* $1 : 3 : 3^2 \ldots$.

Now, if on such an aqueous solution of different alcohols or esters, &c., we dispose a layer of a liquid insoluble in water, such as benzene, the amount of dissolved substance of which this liquid will deprive the water will be greater in exact proportion as the solution-tension is less. *Distribution coefficients and solution-tensions—and hence also surface-tension and osmotic velocity—are therefore proportional magnitudes in first approximation.*

Now we can understand that diosmotic velocity and solubility in fats &c. run parallel, without being *obliged* to accept Overton's hypothesis that solubility in fats is the cause of the penetration of substances in the cell-walls.

By help of a simple capillary method, a method much to be recommended for the determination of solubilities, I have determined the distribution-coefficients of the above esters for benzene, carbon tetrachloride, and carbon bisulphide on the one hand, and for water on the other hand. Equal volumes of the organic solvents and of the aqueous solution were shaken up at 19°–20°, and then the original aqueous solution diluted with water until the constant of capillarity rose to the same value as it attained after agitation with benzene.

In determining the solubility of such substances as these in water, it is extremely advisable to shake up with a surplus of the substance to be dissolved, and then to go on diluting this saturated solution until the constant of capillarity is equal to that of a second solution of known concentration. So simple and so sensitive are these methods, that they can be used even in cases where other methods fail.

The following Table gives the distribution coefficients which I have thus determined :—

* Compare further examples, *Ann. Chem. Pharm.* cclxv. p. 271.

	Concentration of the aqueous solution in equivalents.		Distribution Coefficients.					
			$\dfrac{C_6H_6}{H_2O}$	Quotient.	$\dfrac{CCl_4}{H_2O}$	Quotient.	$\dfrac{CS_2}{H_2O}$	Quotient.
Methyl acetate ...	1	normal	2·94		2·56		......	
	2·06	,,	3·77		......		......	
	3·14	,, (saturated)	3·88		3·50		1·81	
Ethyl acetate ...	0·33	,,	10·2	3·0	9·0	3·2	......	2·9
	0·90	,, (saturated)	10·8		10·10		5·27	
Propyl acetate ...	0·11	,,	33·3	3·2	......	4·1	......	3·8
	0·22	,, (saturated)	34·8		39·0		20·0	

Slight corrections were necessary in the cases of carbon tetrachloride and benzene. For it was observed that when pure water was shaken up with carbon tetrachloride, the capillary height of the water was immediately reduced by $\frac{1}{2}$ millim., and when shaken with benzene by 2·2 millim.

That the law should hold good less accurately for the distribution coefficient than for capillarity constants is not surprising. For to begin with, the distribution-coefficient probably varies because of partial association, especially of methyl acetate in benzene ; and, secondly, the above statements show that there cannot be absolute proportionality between distribution-coefficients and constants of capillarity This follows, for instance, from a comparison of ethyl acetate with isobutyric acid. The capillarity constants are the same, but the coefficient of isobutyric acid in the system CS_2/H_2O is $=0\cdot80$.

Overton and Hans Meyer have noted the interesting fact that the efficaceous narcotics, anæsthetics, and antipyretics all belong to those combinations that penetrate thin membranes rapidly. Rapid penetration into the cell seems to be the most essential condition for enabling a narcotic to exercise its paralysing and other effects on the interior of certain cells. But now we have found that a near relation exists between osmotic velocity and surface-tension, and therefore we can expect that surface-tension and narcotic power run parallel. That is really the case, in general, even if we compare narcotics which differ materially in their chemical constitution *.

If we compare narcotizing substances of the same homologous series, the relation becomes still more remarkable.

We have found (p. 711) a very simple law to be valid for the surface-tension of such homologous substances, and we will

* Compare Pflüger's *Archiv*, Nov. or Dec., 1904.

examine whether the same law holds good for the narcotic action of those substances.

By the cerebral narcosis of tadpoles Overton[*] has determined that the equivalent quantities of dissolved alcohols given in the table under the headings have exactly the same narcotic effect. Under 2 are to be found the relative equivalent quantities of the same alcohols that—according to Joffroy and Serveaux's experiments [†]—prove fatal to rabbits on intravenous injection.

Very fine are those experiments carried out in several parallel series on the development of fertilized eggs of the sea-urchin by Dr. H. Fühner [‡] in the biological stations of Heligoland and Villefranche; by quantitative methods,

	JOFFROY & SERVEAUX. On Rabbits.		OVERTON. On Tadpoles.		FÜHNER. On Sea-Urchin Eggs.	
		Quotients.	Molec. per litre.	Quotients.		Quotients.
Methyl alcohol ...	1·267	3·1,	0·57	2·0	0·719	1·8
Ethyl alcohol ...	0·408	4·5	0·29	2·6	0·408	3·0
n-Propyl alcohol	0·090		0·11	3·0	0·136	3·0
n-Butyl alcohol...	......	2 9,	0·038		0·045	
Isopropyl alcohol	......		0·13	3·0	......	
Isobutyl alcohol .	0·031	3·1	0·045	2·0	......	
Isoamyl alcohol .	0·010		0·023		0·020	
n-Heptyl alcohol	......		......	...	0·00170	3·3
n-Oktyl alcohol...	......		......	...	0·00051	
Capryl alcohol ...	......		0·0004		0·0008	

	OVERTON. On Tadpoles.	
	Molec. per litre.	Quotients.
Acetone	0·26	3·0
Methyl ethyl ketone..............	0·09	3·0
Diethyl ketone	0·029	
Methyl acetate	0·08	2·7
Ethyl acetate......................	0·03	2·9
Propyl acetate	0·0105	
Isobutyl acetate	0·057	3·0
Isoamyl acetate...................	0·019	

[*] Overton, *Studien über die Narkose*, Jena, 1901.

[†] Joffroy and Serveaux, *Arch. de médec. expér. et d'Anatom. pathol.* vii. p. 569 (1895).

[‡] H. Fühner, *Arch. für exper. Pathol. u. Pharmakol.* li. p. 1, 1903, and lii. p. 69, 1904.

Dr. Fühner determined the check to development which the fertilized eggs underwent in equivalent solutions of the different alcohols.

It is quite evident that the narcotic action of homologous substances (such as alcohols, esters, &c.) *with increasing molecular weight is as* $1 : 3 : 3^2 : 3^3$. . . . The validity of this law may justly be looked upon as an excellent corroboration of my theories *.

I do not oppose Overton's and L. Meyer's theory, in so far as it represents the action of narcotics as depending on lipoid-solubility. From the relation which I have shown to exist between surface-tension and distribution-coefficient, in connexion with the fact that the narcotics exercise their chief action in cells containing much lipoid substance, it follows that after penetration into the cell, dissolution in the lipoid substance takes place. But unfortunately not only does the narcotic dissolve into the lipoid substance, but some of the lipoid substance dissolves in the narcotic ; and this circumstance may very likely be the cause of the disagreeable secondary effects exerted more or less by all narcotics. And if this be so, there seems little hope of ever finding a perfectly innocuous narcotic.

I venture to hope that the above observations, setting forth as they do the close relations between Osmosis, Diffusion, Solubility, and Capillarity, may lay claim to some interest from the chemico-physical point of view. I would emphasize, however, that the chief importance of the research lies on physiological, biological, pharmacological, and diagnostic grounds. The chief result is the substitution of the difference of the surface-tensions (den Oberflächendruck) for the osmotic pressure, and in my paper, which will be published in this year in Pflüger's *Archiv* (" Der Oberflächendruck und seine Bedeutung für den Organismus ") I prove that by the introduction of this new force in physiology a great many phenomena will be easily understood which were believed to be explained only by vital forces. I beg to call attention to that paper because several questions are treated (*e. g.* the theory of catalysis, the effect of the cell-walls and of medicaments on the velocity of osmosis, the doctrine of toxines, &c.) which are perhaps of some general interest.

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* It seems to follow from Overton's and Fühner's experiments, viz.: that the narcotic law, in contrast with the capillary law, is not valid for the transition from normal to iso-combinations. From this we should have to conclude that the slight change in constitution brings about differentiation of the narcotic action.

LXXII. *Relative Motion of Earth and Æther.*
By A. A. Michelson [*].

IN the *Physikalische Zeitschrift* [†] a method is proposed by W. Wien for deciding the important question of the trainement of the æther by the earth in its motion through space, by measuring the velocity of light in one direction— that is, without reflecting it back from the distant station.

The essentials in the proposed method are two Foucault mirrors, or two Fizeau wheels (one at each station) revolving at the same speed. The control for this synchronism is to be furnished by the "Konstanz des hindurchgehendes Lichtes oder in bekannten stroboskopischen Methoden."

The flaw in the proposed method—as was pointed out by Simon Newcomb as long ago as 1880—lies in the fact that the effect which it is proposed to measure is exactly the same as the effect on the light which is to furnish the test of synchronism.

In November 1887 I proposed a method differing in no essential respect from the foregoing, except that the control of synchronism was to be furnished by electrical methods. This was before the celebrated work of Hertz showed that electrical impulses differed in no essential from light. This identity constitutes the same objection to this plan [‡].

The possibility of a mechanical method of control was pointed out in a paper on the "Velocity of Light," in the Philosophical Magazine, March 1902. This is based on some experiments made in 1899, which showed that the vibrations of a tuning-fork could be transmitted over a mile of piano-wire with but little diminution of amplitude [§].

PART II.

Suppose it were possible to transmit two pencils of light in opposite directions around the earth parallel to the equator, returning the pencils to the starting-point. If the rotation of the earth does not entrain the æther, it is clear that one of the two pencils will be accelerated and the other retarded (relatively to the observing apparatus) by a quantity proportional to the velocity of the earth's surface, and to the length of the parallel of latitude at the place ; so that a

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[‡] Possibly a spirally wound wire—which transmits electrical oscillations with a velocity less than that of light—would be differently affected, and thus furnish a solution of the problem.

[§] Perhaps, however, even mechanical impulses would be affected by the earth's motion in such a way as to neutralize the expected effect.

measurement of the difference of time required for the two pencils to traverse the circuit would furnish a quantitative test of the entrainement.

But it is not necessary that the path should encircle the globe, for there would still be a difference in time for any position of the circuit.

This difference is given by the formula

$$T = \frac{2}{V^2} \int v \cos \theta \, ds,$$

where V is the velocity of light, v the velocity of the earth's surface at the element of path ds, and θ the angle between v and ds.

If the circuit be horizontal, and x and y denote distances east and west and. north and south respectively, and ϕ the latitude of the origin, and R the radius of the earth, then for small values of y/R we have approximately

$$T = \frac{2v_0}{V^2} \int \left(\cos \phi - \frac{y}{R} \sin \phi \right) dx.$$

The integral being taken round the circuit the first term vanishes, and if $A = \int y \, dx =$ area of the circuit,

$$T = \frac{2v_0 A}{V^2 R} \sin \phi.$$

The corresponding difference of path for equal times expressed in light-waves of length λ is

$$\Delta = \frac{2v_0 A}{V R \lambda} \sin \phi.$$

Thus, for latitude $45°$ $\sin \phi = \sqrt{1/2}$, $\frac{v_0}{R} = \frac{2\pi}{T}$; the velocity of light is 3×10^8 in the same units, and the length of a light-wave is 5×10^{-7}; which approximate values substituted in the preceding formula give

$$\Delta = 7 \times 10^{-7} A.$$

Thus if the circuit be one kilometre square

$$\Delta = 0 \cdot 7.$$

The system of interference-fringes produced by the superposition of the two pencils—one of which has traversed the circuit clockwise, and the other counterclockwise—would be shifted through seven-tenths of the distance between the fringes, in the direction corresponding to a retardation of

the clockwise pencil, if the experiment were tried in the Northern hemisphere.

The observation of interference-fringes produced by pencils which traversed a path 60×20 metres * presented so little difficulty, that it seems quite feasible to proceed to much greater distances.

In the case considered, the length of the path would be four kilometres. If this length were doubled the area enclosed would be quadrupled and the expected displacement would be 2·8 fringes.

A difficulty in the measurement of this displacement lies in the fact that it cannot be reversed (as was the case in the experiment † where the entire apparatus was rotated). A fiducial mark is, however, furnished by the image produced by one of the two pencils.

Thus let light, starting from a slit and rendered parallel by a collimator, fall on a glass plate the upper half of which is heavily silvered while the lower half is clear or lightly silvered.

The light transmitted by the lower half is reflected round the circuit, returning to the glass plate through which it passes to the observing telescope—while the reflected part traverses the circuit in the opposite sense, returning to the glass plate where it is reflected to the observing telescope, interfering with the former pencil.

Observing by reflexion from the upper half, the image of the slit is seen, and the cross-hair of the eyepiece is made to divide this image symmetrically. The upper half is now covered and the lower half clear.

The system of interference-fringes should have its central or achromatic fringe bisected by the cross-hair if the æther rotates with the earth. If the æther does not partake of the earth's motion of rotation, the central fringe will be displaced from the cross-hair by the amount calculated from the formula.

A control is furnished by introducing another pair of mirrors in the path so as to make the area of the circuit so much smaller that the displacement would be negligible.

The attempt to apply the same principle to the revolution of the earth about the sun is less promising. The formula for the displacement from noon to midnight is in this case

$$\Delta = \frac{4\mathrm{A}v}{\lambda \mathrm{R V}} \sin \phi \cos \delta,$$

where A, λ, ϕ, and V have the same meaning as before, and

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* Am. Journ. Sci. vol. iii. 1897.   † Phil. Mag. Dec. 1887.

v is the velocity of the earth in its orbit, R the radius of earth's orbit, and δ the sun's declination.

If A = 10 × 10 kilometres,

$v/V = 10^{-4}$, $R = 1 \cdot 5 \times 10^{11}$, $\lambda = 5 \times 10^{-7}$,

$\sin \phi = 0 \cdot 7$, $\cos \delta = 1$,   then $\Delta = 0 \cdot 37$.

To obtain this displacement would require a circuit 40 kilometres in length.

LXXIII. *On the Absorption of α Rays, and on the Classification of the α Rays from Radium.* By Professor W. H. Bragg, *M.A., University of Adelaide* [*].

[Plate XVIII.]

BOTH the α and the β rays of radioactive substances are absorbed by their passage through matter; and in each case the rate of absorption depends almost entirely upon the density of the matter and not upon its nature. Apart from this striking similarity, there are important differences between the phenomena of the absorption of the two classes of rays. In a paper read at the Dunedin meeting of the Australasian Association for the Advancement of Science, in January 1904, I endeavoured to make the contrast clear, and to show that a sufficient explanation could be based on the hypothesis that the β rays are liable to deflexion through collision, whereas the α rays are not. Both kinds of rays suffer a continual diminution in speed through the expenditure of energy on ionization; but in the case of the α rays this is the only cause to which their so-called "absorption" is due. This hypothesis has many theoretical considerations in its favour, and I showed in the paper referred to that it provides a good explanation of many known facts. I have recently made a direct attempt to test its truth; and the results of the experiments are satisfactory. Moreover they show that it is possible to divide the α rays into classes: certainly two, and probably four. All the rays of the same class have the same initial velocity. Before setting out the experimental results, it will be well to recapitulate some theoretical considerations.

The β ray is an electron of high speed; and, as experiment shows, it may pierce millions of atoms without suffering appreciable deviation. It is well known that an electron flying past a similar electron which is stationary undergoes a deflexion which depends in part upon the relative velocity.

* Communicated by the Author.

If the encounter be not too close, both the deflexion and the amount of energy given by the moving to the stationary electron are inversely proportional to the energy of the former. The actual deflexion which an electron is likely to suffer in going through an atom, must not of course be calculated on the supposition that the atom consists merely of so many electrons, taken as at rest : the positive charge must be brought in, and the fact that the atom's electrons are also in motion. Indeed, it may be calculated that the more simple but defective supposition is unable to explain fully the penetrative power which the electron certainly possesses. Nevertheless, it is clear that the β rays are liable to deflexion through close encounters with the electrons of the atoms ; and therefore the distance to which any given electron is likely to penetrate before it encounters a serious deflexion is a matter of chance. This, of course, brings in an exponential law. Such a law was, for example, obtained by Lenard in his original experiments on the cathode rays, which penetrated a thin aluminium window in the wall of the tube in which they were formed. By the aid of stops he isolated a small pencil of rays whose intensity, after they had traversed a certain distance of air, he measured by aid of a phosphorescent screen. If such a jet of electrons be projected into the air, some will go far without serious encounter with the electrons of the air molecules ; some will be deflected at an early date from their original directions. The general effect will be that of a stream whose borders become ill-defined, which weakens as it goes, and is surrounded by a haze of scattered electrons. At a certain distance from the source all definition is gone, and the force of the stream is spent.

There is a second cause of the gradual "absorption" of a stream of β rays. Occasionally an electron in passing through an atom goes so near to one of the electrons of the atom as to tear it from its place, and so to cause ionization. In doing so, it expends some of its energy. It is easy to calculate from the data that Durack has given as to the number of new ions made in each centimetre, and Townsend as to the amount of energy required to make a new pair of ions, that the β ray must slow down considerably towards the end of its course ; and the slower it goes, the more liable it is to deflexion.

On the other hand there is, in the case of the α ray, only one cause of "absorption." The α ray is a very effective ionizer, and rapidly spends its energy on the process. It is of course far more likely than the β ray to ionize an atom

through which it is passing, because it contains some thousands of electrons and ionizing collision is so much the more probable. But a collision between an electron of the flying atom of the α ray and an electron of the atom traversed, can have very little effect on the motion of the α atom as a whole. All the electrons of the α ray which do not go very close to electrons of the stationary atom, are practically undeflected; and probably even one which does, is steadied by its connexions with its fellows.

Deductions from this hypothesis harmonize very well with observed effects. In the first place, penetration of matter must be inversely proportional to the density of the matter. In the second place, there should be no scattered or secondary radiation as in the case of the β electrons, and this is found to be true. Again, since the α rays are undeviable by collision, a stream of them should be as sharply defined after traversing a thin material plate as it was before; which Becquerel found to be the case. Again, α rays emitted at a certain speed should reach a certain distance from the parent body which is proportional to the initial energy; so that an ionization chamber should be unaffected if just out of range, and much affected if just within it, as Madame Curie found. Also the α particle should penetrate much further than the β particle of the same initial velocity, since the former only loses its motion on account of the expenditure of energy on ionization, whereas the latter is liable also to deflexion by collision. This appears to be true from the experiments of Lenard and Rutherford. It is also easy to see that even if the α particle is uncharged when it leaves the parent body, it must immediately become positive, since in traversing an atom it is just as likely to lose one of its own electrons as to take one away from the atom traversed. This agrees with Rutherford's statement made in his Bakerian Lecture. It may very well be, however, that the loss of a second electron is much more unlikely than the loss of the first.

On the other hand, it is clear that on this hypothesis no exponential law can express the phenomena of the absorption of the α rays, and that if an exponential law is found to be approximately realized, the hypothesis must furnish some explanation of the fact. This test it can, I think, satisfy.

In the first place, however, it must be pointed out that in Rutherford's experiment, wherein he showed that the absorption curve was approximately exponential, the radioactive material was scattered over the floor of the ionization chamber. But if this hypothesis is correct, it is simpler to investigate the matter in a rather different way. Suppose a narrow

pencil of α rays to be emitted from the material, and confined by suitable lead stops as in the diagram. Let the pencil cross an ionization chamber AB, A being a sheet of gauze, B a metal plate. Let B be connected to an electrometer and a saturating potential applied. Let us now calculate the ionization. Suppose the α rays to be all emitted with the same velocity. Let a be their range in air, h the distance from the surface of the radioactive material to the gauze, ρ the ratio of the density of the material to the density of the air, and let x be measured down into the material from its surface. Remembering that when an α particle comes from a depth x in the material, it only has a path $a - \rho x$ in the air, we see that all the α particles belonging to the pencil and coming from a depth $(a-h)/\rho$ will enter the chamber. If the depth of the chamber be δh, and if it be assumed that each particle makes ions whose number is proportional to the distance traversed, then we may put the ionization equal to $n(a-h)\delta h/\rho$, where n is a constant. We are here supposing that the ionization does not depend on the speed, and this is reasonable; for Durack found that each β particle moving at a speed approaching that of light made a new pair of ions in every 6 cm., whereas the slower particle of the Lenard ray made a new pair in every 2·3 cm., the air traversed being in each case at a pressure of 1 mm. The speed of the α ray varies between far narrower limits than these.
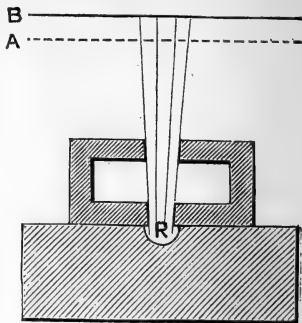
If the depth of the chamber be b, the ionization is

$$\int_{b}^{h+b} n\frac{a-h}{\rho}\,dh = \frac{nb}{\rho}\left(a-h-\frac{b}{2}\right).$$

This supposes that part of the stream is strong enough to cross the chamber. If not the expression becomes

$$\int_{h}^{a} n\frac{a-h}{\rho}\,dh = \frac{n(a-h)^2}{2\rho}.$$

Thus, if the ionization is measured, and a curve plotted showing its relation to h, the curve should in the former case be a straight line whose slope is nb/ρ, and in the latter a parabola.

It should be observed that in this form of experiment a

certain cone of **α** rays is tested at various points, and the inverse square law does not enter into the question.

With the assistance of Mr. R. Kleeman 1 have carried out a set of experiments of this kind. Since the results obtained by Rutherford and Madame Curie would lead us to expect a simpler result in the case of polonium than radium, it would have been preferable to have employed the former, but the latter was alone available.

In the case when all the rays are initially of uniform velocity, the curve obtained ought to show, when the radium is out of range of the ionization chamber, an effect due entirely to β and γ rays, which should slowly increase as the distance diminishes. When the **α** rays can just penetrate, there should be a somewhat sudden appearance of the ionization, and, for a short distance of the approach, equal to the depth of the chamber, the curve should be a parabola. Afterwards it should become a straight line.

This is exactly realized; and so far the hypothesis is verified. But a further effect appears. As the radium is gradually brought nearer to the chamber, the straight line suddenly changes its direction; and indeed there appear to be two or three such changes. Thus the curve is really a rectilinear polygon, with the corners rounded off.

Moreover, the slope of the last side, representing effects close to the radium, is nearly four times the slope of the first side; whilst it seems probable that the slopes of two intermediate sides are two and three times that of the first, respectively.

For all this there is a ready explanation. The atom passes through several changes, and it is supposed that at four of these an α atom is expelled. Probably the **α** particles due to one change are all projected with the same speed. We ought therefore to expect four different streams of α particles, differing from each other only in initial energy. If the radium and its products are in equilibrium, the number of α particles due to each change is the same. Thus, if a_1 is the range of one stream, a_2 of another, and so on, the ionization should, when two streams reach the chamber, be

$$\frac{nb}{\rho}\left(a_1 - h - \frac{b}{2}\right) + \frac{nb}{\rho}\left(a_2 - h - \frac{b}{2}\right)$$

or

$$\frac{nb}{\rho}(a_1 + a_2 - 2h - b).$$

Thus the slope of the curve should in this case be $\dfrac{2nb}{\rho}$, whereas if only one stream enters it should be nb/ρ. When

three reach it, the slope should be $3nb/\rho$, and when four $4nb/\rho$.

In the diagram (Pl. XVIII.) the curve A represents the results obtained with an ionization chamber of depth 5 mm.; the ordinates represent distances from radium to gauze, and the abscissæ the leak per second, as shown on an arbitrary scale by a Kelvin and White electrometer. The cone of rays had an angle of about 20°; 5 mmg. of radium bromide were used, only a small portion of which was exposed. The initial parabola and two straight lines joined by a short curve are well shown. The curve B was obtained with a smaller cone, and plotted more closely; it shows the straight line character up to short distances from the radium. Traces of more than one corner are to be seen in it. With the view of searching more closely for these corners, observations were made as shown in curve E. Previous results seemed to show that there were three streams of nearly equal initial velocities, all finishing at about 3·5 to 4·5 cm. from the source. Hence the ionization chamber was made very shallow, only 2 mm. in depth, and readings were taken for every half millimetre of approach. The results are given in the following table, and plotted in curve E. They are at least partially successful, for there is clearly a corner at Q, and probably others at R and S. Also the slopes of PQ, QR, RS, ST, as drawn in the diagram, are in the proportion of 16, 34, 45, 65, *i. e.*, nearly as 1, 2, 3, 4.

Distance from radium to gauze.	Leak per second.	Distance from radium to gauze.	Leak per second.
4·76	2·62	4·01	4·25
4·71	2·68	3·96	4·43
4·66	2·75	3·91	4·60
4·61	2·80	3·86	4·76
4·56	2·86	3·81	4·96
4·51	2·93	3·76	5·23
4·46	3·02	3·71	5·44
4·41	3·15	3·66	5·66
4·36	3·26	3·61	5·94
4·31	3·41	3·56	6·17
4·26	3·56	3·51	6·40
4·21	3·68	3·46	6·70
4·16	3·79	3·41	6·96
4·11	3·92	3·36	7·26
4·06	4·06		

Since radium freed from its emanation exhibits at first only one disintegration process, the curve obtained from such

radium should be of simpler character. This was found to be the case. The results are plotted in curve D_1, and reduced, as regards abscissæ, to curve D_2. The cone used was in this case rather wide. It is clear that the most energetic α particles are almost, perhaps entirely, absent, and the first breakdown of the radium atom is responsible for the α particle of perhaps the least range of the four.

A thin layer traversed by the α particle should reduce the ranges of all of them by the same distance. This effect is shown in curve C, which shows the result of interposing a thin film of goldbeaters' skin. With this exception the arrangements were the same as in the case of curve A. The effect is simply to reduce all the ordinates by the same quantity.

It should be added that the β and γ rays were not eliminated, but were found by frequent trial to be of small and practically constant effect at all ranges. Also the meshes of the gauze did not interfere, for when the gauze was hung by long silk cords and set swinging in its own plane, the general effects were exactly the same.

It thus appears that there are several classes, perhaps four, of α rays, which may be distinguished from each other by their difference in initial energy. The slowest are probably due to the first act of disintegration, and this is in accordance with Rutherford's experiments. Also the results go to show that the α particles are never deflected, but are "absorbed" only because they spend their energy on ionization.

Finally, we may calculate the ionization that should on this hypothesis be produced in a chamber such as Rutherford used. If the film of radium be supposed very thin ; if an obliquity factor $\cos \theta$ be introduced, and if the chamber be deep enough to absorb all the rays, the ionization should be approximately :—

$$\int_0^{\cos^{-1}\frac{\rho d}{a}} 2\pi \sin \theta \cos \theta \, (a - \rho d \sec \theta) d\theta = \pi (a - \rho d)^2 / a,$$

where d is the thickness of metal traversed, and ρ is the ratio of the densities of metal and air. Thus the curve for simple substances like uranium and polonium should be parabolic with respect to d, and this is nearly the case. For radium the curve should be much more complicated, and might well approach the exponential form.

LXXIV. *On the Ionization Curves of Radium.* *By* W. H.
  BRAGG, *M.A., Professor of Mathematics and Physics in the
  University of Adelaide, and* R. KLEEMAN *.

IN a paper " On the Absorption of α Rays, and on the
  Classification of the α Rays of Radium," contributed by
one of us to this Magazine †, a description was given of a
method of obtaining curves which represented the ionizing
effects of α rays at various distances from their source. It
was shown, also, that the general form of the curves supported
the hypothesis that the α particle passes, in its flight, through
the matter which it traverses, without any appreciable de-
viation, and only loses its extraordinary velocity when it has
expended on ionization the ionizing powers which it possesses
by virtue of that velocity. Such a fast moving atom must
become positive, as the α atom is known to do, and as perhaps
the emanation atom does when it recoils after expelling an
α particle. In an experiment on the ionization curve of
de-emanated radium, 5 mmg. of radium bromide were dis-
solved in a little water in a test-tube. Air was bubbled
through the solution for many hours. The liquid was then
evaporated, and the bottom of the tube on which the radium
was deposited was cut off, and placed in the testing apparatus.
Although the fragment of glass was of very irregular form,
and the curve obtained was distorted by the want of regu-
larity, yet after a few days an effect appeared which could be
ascribed with certainty to the fact that the radium layer was
thin, and that α rays from even the lowest stratum of it were
effective in the ionization chamber. The curve, in fact, had
a vertical portion : the interpretation of this being that for
some distance of approach of the radium towards the chamber
there was no increase in the ionization. A few words of
explanation will make this clear.

In the paper referred to, it was shown that when a shallow
ionization chamber is gradually brought closer to a source of
α rays of uniform initial velocity, the rays being limited by
stops to a narrow cone, and the chamber being wide enough
to take in the whole cone at all distances, then the ionization
increases in proportion to the amount of approach. For as
the distance is diminished the curve includes α rays from
deeper and deeper strata in the radium. Thus, if ordinates
represent distances from radium to chamber, and abscissæ the
leaks shown by a quadrant-electrometer in consequence of

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the ionization currents, then the curve of radioactive material of sufficient depth is simply a straight line such as AB. The α rays are supposed to be all of one kind, *i. e.* of one initial velocity on their expulsion from the parent atom, though not, of course, of uniform velocity when they emerge into the space above the radium. But if the layer is thin, then the ionization curve must take some such form as APM, where PM is the range which the α particles from the lowest layer possess when they emerge. If t is the thickness of the layer and ρ the ratio of its density to that of air, then ρt is equal to the difference between AO and PM. Thus part of the ionization curve is vertical.
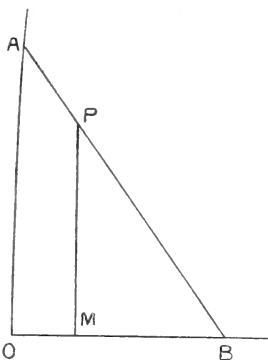
Since this effect was found in the case of the irregular fragment of glass tube on which radium was deposited, it seemed to us probable that layers prepared with greater care would furnish valuable information. This we have found to be the case. The curves are curious and interesting. They fully bear out, so far as we have been able to judge, the hypothesis advanced in the paper already cited. They afford a novel method of examining the phenomena of radioactive change, and a confirmation of the theories and results described by Rutherford and Soddy. They have also brought to light a fact which we believe to have been hitherto unobserved. It is, that the α particle is a more efficient ionizer towards the extreme end of its course.

As has been already stated, the curve of a thin layer emitting one set of α rays must be of the form ABLO. But if there are four sets, as Rutherford and Soddy have shown, there should be four such figures, and the ionization curve should be of the form ABCDEFGH. Assuming the lowest part of the curve to represent the α rays emitted at the first change, as was shown to be probable in the paper cited above, then the other three ought to be nearly equal in width. For they represent the emanation and two induced activities, the latter two following so quickly on the emanation that, unless
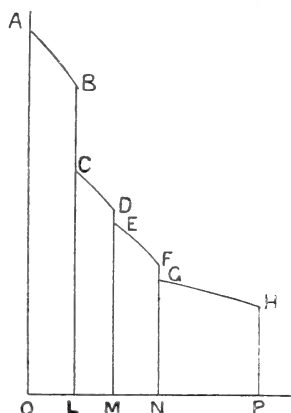
there is a violent disturbance of radioactive equilibrium, they must emit streams containing nearly equal numbers of particles. They may, however, be much narrower than the quadrilateral GHPN which belongs to the first change. The width of a quadrilateral represents, of course, the number of particles in the stream, relative to the other streams: the longer upright side represents the range in air of particles from the top layer of radioactive material, the shorter upright side the range of the particles from the bottom layer. The sides AB, CD, EF should therefore have the same slope.

In an actual experiment the corners must be rounded off, because the ionization chamber must have an appreciable depth. Moreover, the cones must have an appreciable width. We were, in fact, obliged to use rather wide cones, in order to obtain convenient deflexions in the Kelvin and White electrometer. We bound together various little bundles of tubes of thin copper, and placed them vertically over the flat silver dishes on which we evaporated a few drops of solution of radium bromide. Fig. 1 shows the general arrangement. AB is

Fig. 1.
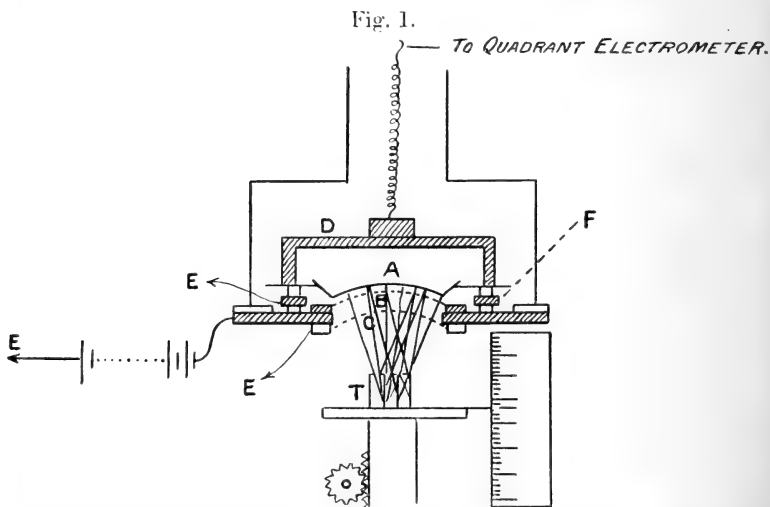
Arrangement of experiment:—Spherical ionization chamber. Conductors shaded, insulators plain. The gauze C made with B an external ionization chamber, which prevented any external ionization from entering the measuring chamber AB. It was only used occasionally, and seemed to be unnecessary.

the ionization chamber, A being thin aluminium, B a sheet of gauze. The lead plate to which the latter was soldered was raised to a positive potential of 250 to 400 volts. A was in

metallic connexion through the walls of the leaden cover D
with the electrometer.   A and D were insulated from an
earthed brass ring F on which they rested, and F was insulated
from the lead plate.   No leakage from the high-potential plate
could therefore reach the electrometer.   The chamber was
sometimes flat, sometimes dome-shaped, as will be explained
later.   A platform supporting the dish and the tubes was
placed below, and could be raised to any desired height.   The
dishes were about 2·4 cms. in diameter, and the tubes 1·5 to
2 cms. in height.   Except in one case specially mentioned,
the dimensions were so proportioned that the cones, at all
distances, fell completely within the chamber.   The ionization
chamber was usually about 3 mms. deep : in curves shown in
any one of the figures given below, it was the same for all the
curves of one figure.

Fig. 2.—Ionization curve of dish prepared seven days before.
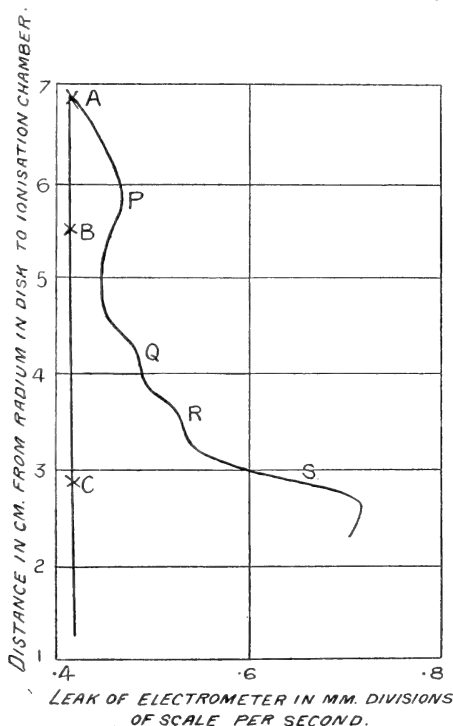
In fig. 2 is shown a curve obtained by the use of a dish
which had been prepared seven days previously from de-
emanated radium.   A set of seven tubes was used each 4 mms.

in diameter and 2 cms. high. The readings A, B, and C represent values of the ionization when a piece of paper was placed over the radium so as to cut off all α rays. The most part of the leak when this was done was due to natural leak of the electrometer. This may have been due to very small amounts of induced radioactivity deposited by emanation within the apparatus ; for when we carefully excluded all radioactive material from the working room, and carried out all preparations elsewhere, the leak was greatly diminished. Much of it was of course due to β and γ rays. It will be seen from the set of readings given later and from the curves drawn in the figures that the leak was very constant during any one set of experiments ; and it may be considered as certain that all to the right of the straight line drawn in the figures represents the ionization due to α rays.

Fig. 3.—Ionization curves of dish prepared (I.) 20 hours, (II.) 90 hours, (III.) 140 hours before.
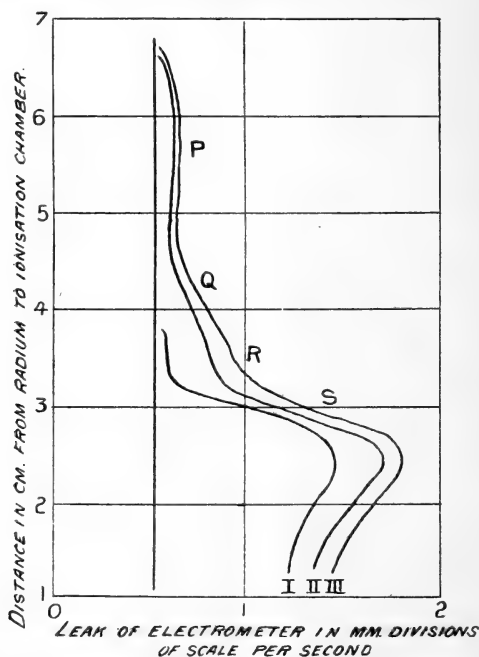
It is plain that the curve PQRS does very fairly approximate to the theoretical form. The gradual approach to this form is shown in fig. 3, where I., II., III. show the ionization of dishes at various times after preparation. In this case the portion P is not fairly represented because we employed

tubes which were only 1 cm. high, so that it might be possible to bring the radium close to the chamber : at the longer ranges the curves were therefore too wide to fall completely within the entrance to the chamber. The portion S represents the ionization due to the first change, and is always in excess of the others *. It might appear that after six days P, Q, and R ought to be more nearly of the width of S; but it is probable that the emanation which is formed does not all remain occluded in so thin a layer. We know, in fact, that it is always passing into the atmosphere. When a dish has been made red hot, the radium, as Rutherford has observed, retains its emanation better. In fig. 4 (p. 732) is shown the ionization curve of a dish five days after being so heated ; and it will be observed that P, Q, and R are now nearly equal to S.

The portions P and S are easily separable from the rest of the curve, and clearly represent different sets of rays. The portions Q and R are not so clearly distinguishable from each other. When the cones of rays are wide, as in the case of the experiments represented in fig. 2, the break between the two portions is not at all clear ; but when the curves are narrower, as was the case in fig. 1, the break is better shown. We have been over this portion of the curve many times, using different dishes and different sets of tubes, and we have always found the dip more or less pronounced.

When a dish is raised to a red heat the portions Q and R disappear, whilst P subsequently decays in great part. This is what should occur supposing that Q and R represent the rays from the emanation and the first induced activity,

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* We give a table of the figures from which one of the curves in fig. 3 was plotted : it would be tedious to give the figures in every case.

Distance from Radium to Chamber.		Leak per second.	Distance from Radium to Chamber.		Leak per second.
6·85	....	·535	Paper	....	·520
Paper	....	·520	3·65	....	·880
6·65	....	·546	3·45	....	·930
6·45	....	·560	3·25	....	·975
6·25	....	·615	3·05	....	1·125
6·05	....	·630	2·85	....	1·435
5·85	....	·630	......	....	1·425
Paper	....	·520	2·65	....	1·665
5·65	....	·640	2·45	....	1·770
5·45	....	·640	Paper	....	·540
5·05	....	·640	2·25	....	1·765
4·65	....	·620	2·05	....	1·690
4·45	....	·650	1·85	....	1·600
4·25	....	·700	1·65	....	1·535
4·05	....	·755	1·45	....	1·470
3·85	....	·800	1·25	....	1·430

whilst P represents the rays from the last induced activity.
Rutherford has shown that the rays from induced activities are
more penetrating than those from the radium itself. Moreover,
the rays that are represented by P go twice as far as those
represented by Q or R, and do twice as much ionization. In
the figure given on p. 262 of Rutherford's book, it is seen that

Fig. 4.—Ionization curve of dish five days after being raised to a red heat.
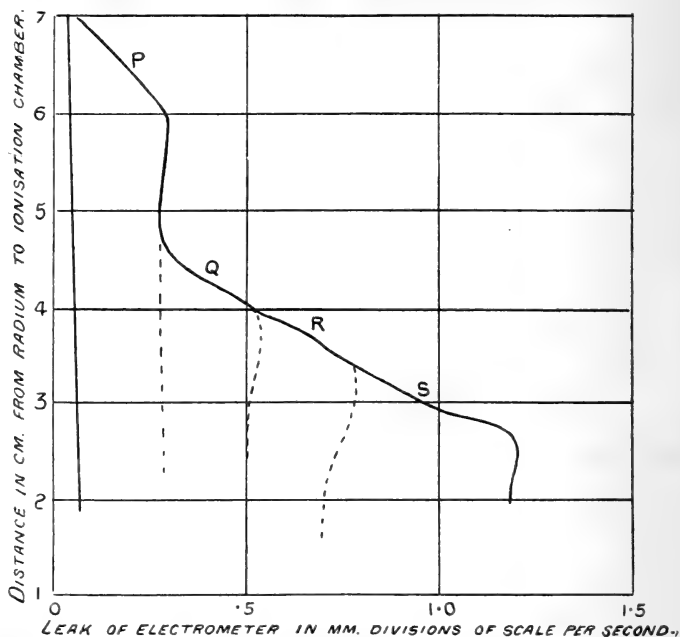
the area of the curve representing the total activity of the
second induced activity is much greater than that belonging
to the first. In fig. 5, I. represents the curve of a dish thirty
hours before heating: it must have moved somewhat to
the right before the operation. Curve II. shows the result
of heating; readings were taken downwards, and occupied
twenty minutes. Curve III. was obtained on going up
again, and the decay of P is clearly shown.

It is not so easy to separate Q from R, and to find which
is emanation and which the first induced change. Either
the red heat which drives off one drives off the other also,
or the induced activity dies away so fast when the emanation
is removed that it is hard to catch it before it is gone. We
cannot with our instruments take one reading in less than
thirty seconds; and this is too slow. We have, however,

received a long expected Dolezalek electrometer by this week's mail, and hope to work more quickly and accurately with its aid. Heating the dish more moderately seems to show that R is the emanation and Q the first induced activity.

It will be observed in most of these curves that there is a curious return towards the vertical axis: it is best shown in the lower portions of P and S. We do not see that there is any other than one possible explanation, viz., that

Fig. 5.
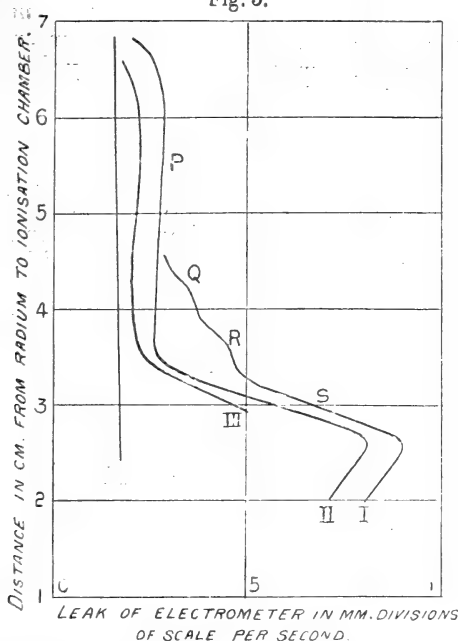
I. Curve 30 hours before heating. II. Curve after raising to red heat. III. Curve about 30 minutes later.

the α particle is more effective at the end of its course. This is not surprising. The disturbing influence of the α particle in its transit through an atom must become greater as the speed diminishes. The diminution is not likely to be great except at the end. This is indeed shown by the curves. Theoretical considerations based on a somewhat insufficient hypothesis show that the effect should be inversely proportional to the energy of the moving particle; but in the case of the β rays Durack has shown that the influence of change of speed is not so great as this. It is possible that it is only at the end, when the change of velocity is very great in proportion to what remains, that the influence of this cause is

perceptible. It is also conceivable that the particle, as its speed approaches the critical value below which it loses the power of penetration, may leave its rectilinear path and be buffeted about, causing a considerable amount of ionization without getting much further away from its source.

The experiments here described show, therefore, that the *a* particles from the first change have a range of about 3·5 cms. in air at ordinary pressures and temperatures: those from the second induced activity have a range of about 6·7 cms. in similar circumstances, whilst the rays from the other two changes have ranges of about 4·5 cms. and 4·0 cms. respectively.

These values may, however, be modified by the state of the air, by the humidity of the radium, and by the distribution of the radioactive products in the radium.

The emanation cannot be uniformly distributed through the radium ; it must be somewhat less at the surface than elsewhere, for it is always leaking into the atmosphere. The substances of " induced activity " are formed from the emanation, and their distribution must be affected by the distribution of the emanation.

To this cause we are inclined to attribute small irregularities in the curves which often appear at the heads of the various sections ; and which are not experimental errors because the same effects are sometimes found unchanged on going over the curve again.

In the paper referred to previously, a curve was given showing the ionization of a thick layer of radium, in radioactive equilibrium, at various distances from the source. Since the radium was not a thin layer, the curve ought to be a polygon with four corners.

The curve showed two well-marked corners and traces of probably two others.

We have repeated this experiment, using spherical ionization chambers. If a cone of rays from a small quantity of radium R is isolated by a suitable stop S, as in the figure, the upper boundary of the radiation is a portion of a spherical surface. If an ionization chamber of about the same radius is used, it is plain that the entry of the rays into the chamber and the consequent ionization within it will be more sudden and more marked than if the chamber is made of parallel plane sheets.
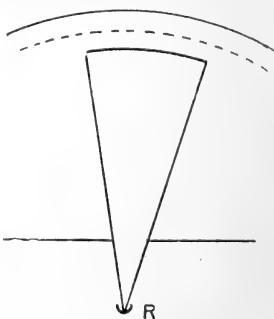
With a chamber of 6 cms. radius we obtained the curve

obtained in fig. 6, which shows well the first corner, at which the ionization due to the second induced activity makes its appearance.

Fig. 6.
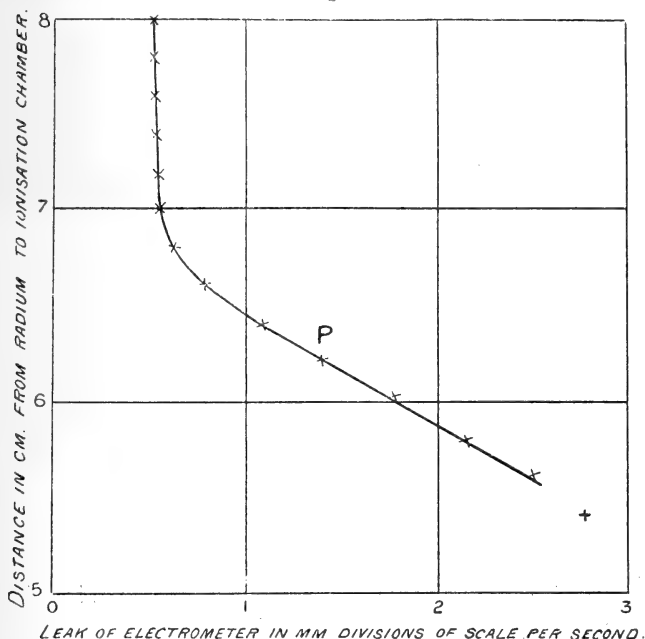
Using a chamber of 4 cms. radius we obtained results represented by the curves in fig. 7 (p. 736). For one of these the ionization chamber was shallower than for the other.

These show the two bottom corners, the lowest showing the point where the rays from the first break-down make their appearance, the upper probably representing the commencement of the emanation rays. The use of spherical chambers somewhat raises the points at which the various sets of rays make their appearance, as is to be expected. The higher values should be the more correct.

A very remarkable experiment has been made and described by Becquerel, which showed that the trace of fine sheet of α rays upon a photographic plate possessed less curvature, when deflected by a magnet, at greater distances from the source. If we seek an explanation on the basis of the hypothesis already referred to, we find at once that one side of the trace ought to show this effect.

3 E 2

Fig. 7.—Curves showing the two lowest corners : thick layer of radium bromide.
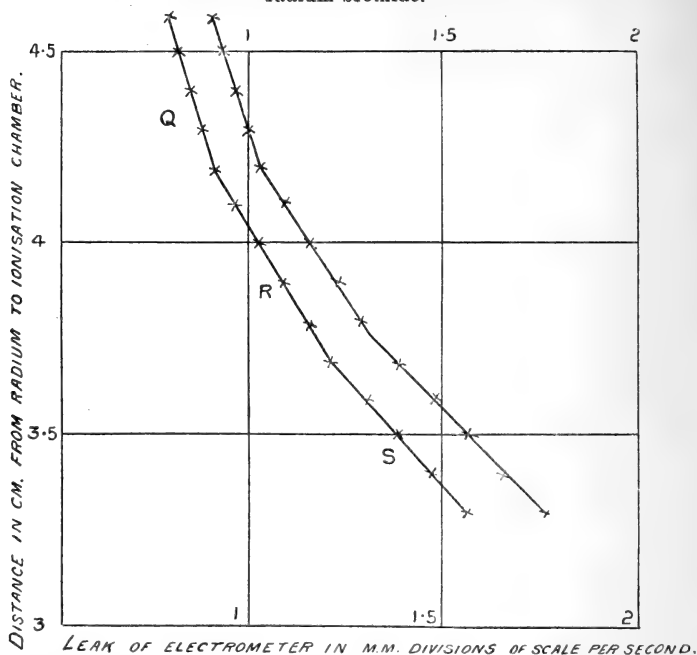
Becquerel isolated a very narrow sheet of rays by placing the radium in a narrow groove, and confining the rays by means of a fine slit. Suppose P to be one ray, that is to say the path of one set of particles of uniform speed, coming from the top of the radium. Then Q will represent the path of a similar set from a somewhat lower stratum, R a set from still lower, and so on. These paths are exactly alike in form except that they are shorter. If a wire were bent to the shape of PBA, and pushed downwards in the direction of its length, and kept always in contact with B and A, then PQR would be successive positions. Now the path of any one particle is more curved towards the end of its course, because its velocity diminishes. In the figure ABP is intended to be more curved at the upper end. But for this very reason the locus PQR is more curved at the lower end, as can be easily realized by means of the
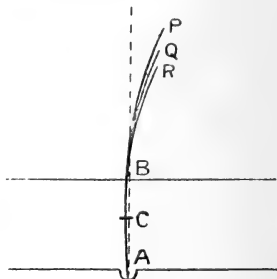
idea of the moving wire. The point P moves about an instantaneous centre at the intersection of the normals at B and A: approximately, therefore, about the centre of curvature of the wire at the point C. Thus the curvature of the locus PQR at the point P is therefore the curvature of the path of the ray at the point C, not at P. Hence if the locus is given, it is possible to calculate the curvature at different distances from the end of the path of a ray, which, it must be remembered, is the same for all α rays. Becquerel calculates the radii of the circles through PBA, QBA, RBA, and so on; but this is not exactly what we now require.

He gives, however, a table of most careful measurements of the various points on the trace: and it is therefore possible to make an approximate calculation of the curvature at various points of the path.

Let PAB, P'AB be two different paths. Let ρ be the radius of curvature at C, a point midway between A and B. Let s be the distance of C from P, and ψ the angle made with the tangent at any fixed point on the moving wire with the vertical AB. Let PM$=y$, AM$=x$.

Then approximately P'N$=$CP'. $\delta\psi$,

and
$$\rho = \frac{ds}{d\psi} = \frac{\text{PN}}{\text{P'N}} \cdot \text{CP'} = s\frac{\delta y}{\delta x}.$$

Becquerel states the following results * :—

Values of y.		Corresponding Values of $2x$.
·1	. . . .	·00719
·2	. . . .	·01489
·3	. . . .	·02293
·4	. . . .	·03160
·5	. . . .	·04028
·6	. . . .	·04973
·7	. . . .	·05928
·9	. . . .	·07864
1·1	. .	·09854

And AB$=2$ cms.

Hence when $s=1\cdot1$, $\delta x=\cdot0036$ cm., and $\delta y=\cdot1$ cm.

Thus $\rho=1\cdot1 \times \cdot1 \div \cdot0036 = 30$ cms. approximately.

* *Comptes Rendus,* cxxxvi. p. 1519 (1903).
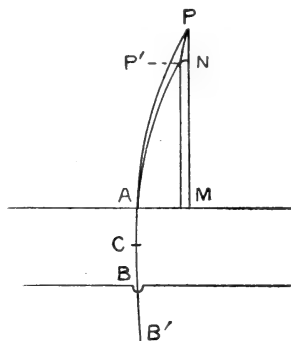
The other values are given in the following table.

Distance from end of Path.	Radius of Curvature.
1·1	30
1·2	31
1·3	31
1·4	33
1·5	32
1·6	35
1·8	37
2·0	40

The radius of curvature should be approximately proportional to the square root of the distance from the end of the path : and the agreement is quite as close as we could expect.

This argument applies of course to the outer edge of the trace only. But it may be argued that this edge should have more character than the other because we know that the rays end with precision. The inner edge of the trace is made up of the paths of rays which gradually decrease in intensity as we proceed from right to left in the figure. It is probable also that the photographic action, like the ionizing action, is more intense at the end of the path. The figures drawn greatly exaggerate the actual deflexions ; for in Becquerel's trace the length of AP is only 1 cm., and the deflexion at P only ·05 cm.

The University of Adelaide.
Adelaide, Sept. 8, 1904.

LXXV. *The Genesis of Ions by the Motion of Positive Ions, and a Theory of the Sparking Potential.* By JOHN S. TOWNSEND, *M.A., F.R.S., Wykeham Professor of Physics, Fellow of New College, Oxford*; *and* H. E. HURST, *B.A., Hertford College, Oxford* [*].

IN a paper on the " Genesis of Ions in a Gas " (Phil. Mag. Nov. 1903) the theory of ionization by collision in a uniform field of force was explained, and a number of experiments with air and hydrogen were given in illustration of the phenomena. The apparatus which was described in that paper was used for a further set of experiments with the same two gases in order to test the theories over a wider range of pressures.

* Communicated by the Authors.

The first set of experiments were made at pressures between ·5 and 2 mms. with air and from 1 to 8 mms. with hydrogen. The experiments which we have recently made include pressures up to 6 mms. with air and 20 mms. with hydrogen.

The principles which underlie the experimental investigations have already been explained. It was shown that when a number n_0 of ions are set free by the action of ultra-violet light from the negative electrode in a gas and travel in a uniform field to the positive electrode, the total number (n) of ions which arrive at the positive electrode is given by the formula

$$n = n_0 \frac{(\alpha - \beta)\,\epsilon^{(\alpha - \beta)d}}{\alpha - \beta\epsilon^{(\alpha - \beta)d}},$$

where α is the number of molecules ionized by a negative ion in going through a centimetre of the gas, β the corresponding number for a positive ion, and d the distance between the parallel plates which form the electrodes.

The values of α and β corresponding to given values of the pressure p and electric force X may be deduced from determinations of the currents between the plates at different distances apart when the force and pressure are kept constant.

The above equation shows that when the distance d is increased to an amount which is determined by the condition $\alpha - \beta\epsilon^{(\alpha - \beta)d} = 0$, the quantity n becomes infinite, and a current should continue to pass through the gas after the ultra-violet light has ceased to act on the electrode. Let a be the value of d which satisfies this condition so that the potential $X \times a$ should suffice to produce a spark in the gas when the distance between the electrodes a is given by the equation

$$a = \frac{\log \alpha - \log \beta}{\alpha - \beta}.$$

Having determined the quantities α and β for various forces and pressures, the corresponding quantities a and $X \times a$ were calculated and may be compared with the sparking potentials V determined experimentally for the distance a between the plates.

The following tables give the results of the investigations. The distance d between the plates is given in centimetres, and the currents q, determined experimentally for the different distances, with constant pressure and electric force are given in arbitrary units. The order of the currents generally used may be found in amperes by multiplying the values of q by the factor 10^{-12}.

The electric force X to which the currents q correspond is given in volts per centimetre. The numbers n are calculated from the formula $n = n_0 \dfrac{(\alpha - \beta)\,\epsilon^{(\alpha - \beta)d}}{\alpha - \beta\epsilon^{(\alpha - \beta)d}}$, α and β being chosen so that the formula should agree with the currents at three different distances, n_0 being taken as unity.

It will be seen that the formula is in agreement with the currents over the whole range of distances. The values of a obtained from the equation $a = \dfrac{\log \alpha - \log \beta}{\alpha - \beta}$ are given at the foot of the tables, also the quantities $X \times a$ and the sparking potential V determined experimentally, for the distance a between the plates.

Air, Pressure 2 mms.

d	·2.	·4.	·6.	·8.
X=525 q	4·67	22·2	110	800
$\alpha = 7\cdot7$ $\beta = \cdot0070$ n	4·67	22·1	110	820

$a = \cdot91, \quad a \times X = 481, \quad V = 488.$

Air, Pressure 4 mms.

d	·1.	·2.	·3.	·4.
X=1050 q	4·67	22·4	111	833
$\alpha = 15\cdot4$ $\beta = \cdot0142$ n	4·67	22·2	111	833

$a = \cdot454, \quad a \times X = 477, \quad V = 480.$

Air, Pressure 4 mms.

d	·2.	·3.	·4.	·5.	·6.	·7.	·8.
X=700 q	5·12	11·4	26·7	61	148	401	1500
$\alpha = 8\cdot16$ $\beta = \cdot0067$ n	5·13	11·6	26·5	62	149	399	1544

$a = \cdot871, \quad a \times X = 609, \quad V = 615.$

Air, Pressure 6 mms.

d	·1.	·2.	·3.	·4.	·5.
X=1050 q	3·47	12·1	42·5	163	880
$a=12\cdot42$ $\beta=\cdot0108$ n	3·47	12·1	42·8	163	872

$a=\cdot572$, $a\times X=601$,   V=604.

Air, Pressure 8 mms.

d	·1.	·2.	·3.	·4.
X=1400 q	5·2	27·5	158	1840
$a=16\cdot47$ $\beta=\cdot013$ n	5·2	27·6	158	1850

$a=\cdot431$, $a\times X=603$,   V=603.

Air, Pressure 8 mms.

d	·2.	·4.	·6.	·7.
X=1050 q	6·42	42·1	350	1400
$a=9\cdot3$ $\beta=\cdot0075$ n	6·42	42·5	338	1480

$a=\cdot765$, $a\times X=803$,   V=803.

In the previous experiments the hydrogen which was used was prepared by the electrolysis of potassium hydrate. The gas was carefully prepared and the air did not come into contact with the potassium-hydrate solution except while the solution was being prepared, so that any impurity which might have been introduced must have been extremely small. Some objections have, however, been raised to the use of potassium hydrate, since the carbonates which would be formed by contact with air are soluble and the hydrogen which is evolved by electrolysis may contain hydrocarbons. In order to obtain the gas as pure as possible, the hydrogen was prepared by the electrolysis of barium hydrate, which forms an insoluble carbonate. According to Mr. H. B. Baker, this is the best method of obtaining pure hydrogen, and we

are indebted to him for having given us some very pure barium hydrate. As in the previous experiments, the vessel containing the electrolyte was not open to the air, and was designed so that the hydrogen should be evolved without having any oxygen mixed with it.

The leak in the apparatus used for the determination of conductivities did not exceed $\frac{1}{300}$ of a millimetre in 24 hours; and as a fresh supply of dry gas was used for each set of experiments, the pressure of the air in the apparatus at any time during the experiments must have been less than $\frac{1}{3000}$ mm. This amount could not have had any appreciable effect on the experimental results.

In order to test the previous results some of the experiments were repeated. At 4 mms. pressure, when a force of 350 volts per centimetre was applied, the conductivities corresponding to distances between the plates of 6, 8, and 10 mms. were found to be proportional to the numbers 1, 2·94, and 11·9, the numbers previously obtained being 1, 2·93, and 11·9. It is evident that if there had been any impurity in the gas which was used in the previous experiments, it did not affect the conductivities to any appreciable extent.

The experiments at 8 mms. were repeated, as it was evident that in one case some error had been made, and there was one number which could not be well reconciled with a theoretical series of conductivities. All the numbers referring to that pressure which are given in the previous paper were verified except the number in which an error was suspected. This occurred in the ratio of the conductivities obtained with a force of 525 volts per centimetre corresponding to distances of 5 and 7 mms. between the plates, which was about 20 per cent. too large. The correct values of the conductivities at a pressure of 8 mms. when a force of 525 volts per centimetre is applied are given in the following table of results from experiments which were repeated several times :—

Hydrogen, 8 mms. pressure.

d	·3.	·5.	·7.	·8.
$X=525$ q	4·87	14·4	51·5	122
$a=5·2$ $\beta=·043$ n	4·85	14·6	53	124

$$a = ·927, \quad a \times X = 486, \quad V = 487.$$

The following experiments were also made :—

Hydrogen, 12 mms. pressure.

d............	·1.	·2.	·3.	·4.	·5.	·6.	·7.
X=1050 q	3·97	171	1160				
a=13·7 β=·114 n	3·97	171	1160				
X=700 q	...	3·71	7·17	14·2	29·5	67·0	204
a=6·51 β=·0388 n	...	3·71	7·19	14·4	29·7	67·6	205

X=1050, a=·353, $a \times$X=370, V=389.
X=700, a=·791, $a \times$X=555, V=561.

Hydrogen, 16 mms. pressure.

d............	·2.	·3.	·4.	·5.	·6.	·8.	1·0.
X=1050 q	8·2	26·7	126				
a=10·4 β=·086 n	8·4	26·7	124				
X=875 q	4·5	9·4	21·4	50·3	153		
a=7·46 β=·0384 n	4·5	9·7	21·4	51	149		
X=700 q	...	3·43	5·1	...	11·3	28·2	79
a=4·1 β=·0156 n	...	3·43	5·2	...	12·1	29·2	77

X=1050, a=·463, $a \times$X=485, V=490.
X=875, a=·710, $a \times$X=621, V=619.
X=700, a=1·36.

The greatest distance to which the plates could be separated in the apparatus which was used was 1·2 cms., so that the value of V for the distance 1·36 could not be determined.

Hydrogen, 20 mms. pressure.

d	·3.	·4.	·5.	·6.
X=1050 q	12·7	31·8	91·3	495
a=8·33 β=·040 n	12·7	31·7	91·0	495

$a=$·643, $a\times$X$=675$,   V$=675$.

The agreement between the values of the currents q found experimentally and the numbers n given by the formula $n=\dfrac{(a-\beta)\epsilon^{(a-\beta)d}}{a-\beta\epsilon^{(a-\beta)d}}$ shows that the theory affords a satisfactory explanation of the currents which are produced before sparking occurs.

The accompanying curves (fig. 1) give the connexion between $\dfrac{\beta}{p}$ and $\dfrac{X}{p}$ for the smaller values of the variables. The corresponding curves for the larger values of $\dfrac{\beta}{p}$ and the values of $\dfrac{a}{p}$ are to be found in the previous researches which have been published on this subject.

Fig. 1.
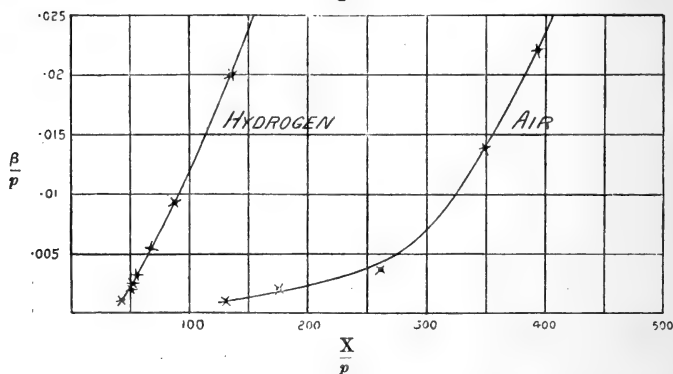
The values which have been found for the quantity β snow that the positive ions in hydrogen are more active in producing ionization by collision than the positive ions in air. This is due partly to the fact that the free paths of the ions are longer in hydrogen than in air, but it is impossible to account

for the large differences between the two values of β for the two gases by this effect alone.   If we suppose that the positive ion in a gas has approximately the same mass as a molecule of the gas, then the positive ions in hydrogen would acquire much larger velocities along the same paths than the positive ions in air, and large differences in the effects arising from collision might be expected.

Some confirmation of this theory is afforded by the results of experiments made with air containing a small percentage of hydrogen.   In this case, although only a small number of the positive ions in the mixed gas are derived from the hydrogen molecules, still, owing to the high velocities they acquire along the free paths, they would be more effective in ionizing other molecules than the positive ions derived from the air.   Consequently we should expect that the values of β for the mixture would be considerably greater than the values of β for air at the same pressure.

The following is the result of an experiment made with a mixture of air and hydrogen at a pressure of 4 mms., the partial pressure of the air being 95 per cent. and that of the hydrogen 5 per cent. of the total :—

d............	·2.	·3.	·4.	·5.	·6.	·7.	·8.
X $=700$ q	4·85	10·8	24·5	56	137	410	2170
$a=7\cdot91$ $\beta=\cdot0110$ n	4·88	10·8	24·4	55	132	390	2400

A comparison between this experiment and that made with air at a pressure of 4 mms. under a force of 700 volts. per cm. shows that the value of β is increased from ·0067 to ·011, and the value of α undergoes only a small change when the gas contains 5 per cent. of hydrogen.

In determining the currents in a gas at low pressure by means of an electrometer, we found it necessary to adopt certain precautions in the arrangement of the apparatus when the difference of potential between the electrodes exceeded the minimum sparking potential.   It was found that the electrometer was sometimes very unsteady when there were potential-differences of 400 or 500 volts between the electrodes.   The gas would insulate for some minutes and then apparently conduct, and a large electrometer deflexion would take place, although no glow appeared in the gas and the potential was obviously less than the sparking potential.   The cause of these irregularities was found to be due to an effect taking place along

the insulators, although there was perfect insulation for potentials of about 300 volts. The accompanying rough sketch shows the form of the apparatus, and will serve to explain the method which was adopted to get rid of the irregular deflexions of the electrometer.
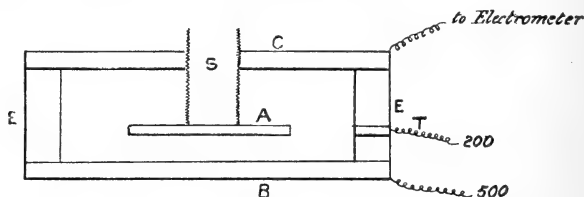
The metal plates B and C were held together by four ebonite pillars E, and the conductivity takes place between the plates A and B. The distance between the plates A and B was adjustable by means of the micrometer-screw S. The upper plate was in connexion with the insulated quadrants of an electrometer, and the lower plate B was in connexion with a terminal of a battery of cells and was thus maintained at any required potential V.

A little consideration will show that the insulation may be very irregular when the potential V is raised to 500 volts although there is good insulation for a potential of 340 volts. It would be almost impossible to ensure that the insulation of the supports E should be equally good along the whole length from B to C, so that a charge would creep gradually over the surface of the insulator and the electric force parallel to the surface would no longer be uniform. For potentials less than 340 volts this would produce a small uniform drift of the electrometer, but for higher potentials it would be quite possible for the surface charges to concentrate the electric force between two points on the surface of the insulators so as to give rise to a minute discharge in the gas.

The irregular disturbance of the insulation was attributed to small discharges arising from effects of this kind, and in order to insulate the plate B rings of tinfoil T were fixed round the supports E and maintained at a potential less than V, for example at 200 volts when the lower plate was 500 volts. The potential-difference between any two adjacent points on the surface of the insulator could not then attain a sufficient value to produce a discharge in the gas near the ebonite pillar.

It was found that the electrometer insulated perfectly when the lower plate was at 500 and the rings at 200 volts, but

irregular large deflexions took place at intervals when the tinfoil rings were insulated, or maintained at small potentials *.

In order to determine the sparking potentials, a battery of small cells was connected to the electrodes through a voltmeter having a resistance of 38,000 ohms and an adjustable resistance. The number of cells was increased until a glow appeared in the gas and the voltmeter gave a deflexion when a beam of ultra-violet light acted on the negative electrode. The intensity of the light used in these experiments was very small, being a thirtieth of that used in the experiments for the determination of the values of α and β, so that the ions set free from the negative electrode would constitute a current of the order $3 \cdot 10^{-14}$ ampere.

The values of the sparking potentials obtained by this method were very definite, and showed no irregularities depending on the length of time the gas is subjected to the electric force. The total external resistance which was used for the determinations which are given in the following tables was 48,000 ohms. It was found necessary to use a resistance of this amount so that too large a current should not pass through the gas when the sparking point is reached. Small variations occurred in the determinations when the external resistance or the intensity of the ultra-violet light was altered. When the intensity of the light was increased to thirty times the value which was used in the determinations, the potentials required to produce a spark were diminished by about three or four volts. When the resistance was increased by 20,000 ohms, it required a few more volts to produce a spark in the gas. There would probably be no variation in the potential due to a change in the external resistance if an electrometer was used to detect a discharge through the gas, but other effects give rise to irregularities in this case. These differences between the determinations of the sparking potentials are very small, and do not affect the agreement with the theory.

The following tables give the values of the sparking potentials V as found experimentally for the distance a between the electrodes. The numbers in the columns $a \times X$ are the values of the sparking potentials obtained theoretically; a being the sparking distance for the force X and pressure p given by the formula $a = \dfrac{\log \alpha - \log \beta}{\alpha - \beta}$.

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* Unless some such precautions as these are observed the sparking potential for pressures greater than the critical pressure cannot be found accurately by a sensitive electrometer, and we think it probable that the "lag" which has been noticed by some observers is due to the effect we have described.

The numbers corresponding to the lower pressures have been taken from the tables in the previous paper on this subject.

Air.—Sparking potentials.

X.	p.	a.	a×X.	V.
1050	8	·765	803	803
1400	8	·431	601	603
1050	6	·572	601	604
700	4	·871	610	615
1050	4	·454	477	480
525	2	·91	481	488
700	2	·575	403	407
350	1	1·13	395	398
437	1	·832	364	365
350	·66	·965	338	340
437	·66	·766	335	336

Hydrogen.—Sparking potentials.

X.	p.	a.	a×X.	V.
1050	20	·643	675	675
875	16	·710	621	619
1050	16	·463	485	490
700	12	·791	555	561
1050	12	·353	370	389
525	8	·927	486	487
700	8	·57	399	395
1050	8	·306	322	322
350	4	1·14	399	395
525	4	·613	322	323
700	4	·405	283	282
350	2	·810	283	287
525	2	·501	269	273
350	1	·806	282	289

The accompanying curves (figs. 2 and 3) represent the connexion between the sparking potential and the product

$p \times a$ for air and hydrogen. The points marked with small circles are those determined experimentally, and those marked with a cross are the theoretical values of the quantity $X \times a$.

Fig. 2.
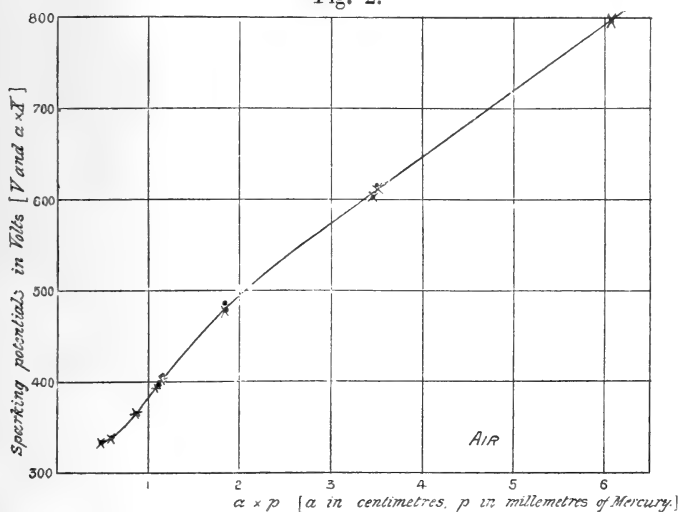
Fig. 3.
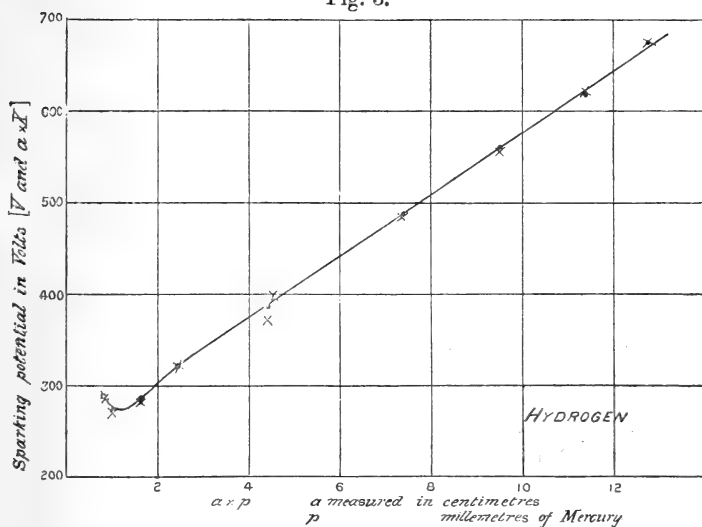
It has already been pointed out that the potential $X \times a$ is a function of the quantity of gas between the plates which s proportional to the product $p \times a$.

Note on the Potential required to maintain a Current in a Gas.
By Professor TOWNSEND.

It is interesting to investigate the phenomena which occur when the sparking point is reached, and to explain why it is that the potential required to maintain a current in a gas is much smaller than the sparking potential. The subject may be investigated on the lines which are here briefly indicated, but a fuller discussion of this application of the theory of ionization by collision must be reserved for a future paper as the necessary experimental investigations have not yet been carried very far.

According to the principles which have been already explained, a continuous current should pass through the gas when the values of α and β corresponding to the pressure and electric force satisfy the condition $a = \dfrac{\log \alpha - \log \beta}{\alpha - \beta}$, where a is the distance between the electrodes. The theory requires that the field of force should be uniform so that the current must be small. When the current is large, the charge carried by the ions disturbs the electric field in such a way as to facilitate the passage of the current. This effect comes into action gradually, and gives rise to the large fall of potential at the negative electrode when the current is sufficiently great. As the velocity of positive ions is smaller than that of negative ions, a positive charge accumulates in the gas. This charge * is greatest in the neighbourhood of the negative electrode since all the positive ions pass through that region, so that as the current increases the electric force becomes greater near the negative electrode than at other places in the gas. The ionizing power of the positive ions as shown by the values of β is much increased in passing through the field of strong electric force, so that the supply of ions required to maintain the current is kept up when the fall of potential between the electrodes is less than the sparking potential.

As an example of some experiments on the difference between the potential required to maintain a current and the sparking potential, we may take those made with air at a pressure of 8 millimetres and a distance of 4·31 millimetres between the plates. The sparking potential given in the

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* It was first suggested by Professor Schuster (Bakerian Lecture, Proc. Roy. Soc. xlvii. 1890, p. 541) that this variation of the electric field in the neighbourhood of the negative electrode is due to the difference between the velocities of the positive and negative ions.

above table was 603 volts. The battery was connected to the electrodes in series with a resistance of 10,000 ohms and a voltmeter having a resistance of 38,000 ohms. With this external resistance (48,000 ohms) the voltmeter gave a steady deflexion slightly over 200 volts, when the potential of the battery was 603 volts. The fall of potential along the 10,000 ohm resistance was over 52·5 volts, so that when the current ($\frac{1}{190}$ ampere q. p.) was flowing, the difference of potential between the electrodes in the gas was less than 351 volts. When the circuit was broken for an instant and a potential of 601 volts applied, no effect was produced, the gas in fact insulated ; but when the potential of the battery was raised to 603 volts the spark appeared again and the same current $\frac{1}{190}$ ampere passed through the gas. It was found possible to adjust the current to any smaller value by increasing the external resistance. The potential-difference at the electrodes required to maintain the smaller currents was greater than 351 volts, and for very small currents a difference of potential between the electrodes of 603 volts was required.

When the pressure was reduced to that which corresponds to the minimum sparking potential, it was found that the potential required to maintain a discharge did not differ much from the minimum sparking potential.

According to the theory the potential of 603 volts is required to maintain a current in a uniform field. When the current increases, a charge, as has been explained, accumulates in the neighbourhood of the negative electrode, and when the cathode fall of potential is fully developed the potential required to maintain the current is about 340 volts. The theory leads to the conclusion that the field of force changes gradually as the current increases, from a uniform distribution of force to one in which there is considerable polarization at the negative electrode which approaches to the value of the minimum sparking potential.

Some preliminary experiments on the field of force between two parallel plate electrodes have confirmed these conclusions.

Over a large range of values of the current there is a change in the potential-difference between the electrodes and in the field of force corresponding to changes in the intensity of the current. As the current is increased, the total fall of potential between the electrodes diminishes, and the intensity of the force increases at first in the neighbourhood of the negative electrode, and subsequently a similar but smaller effect appears

at the positive electrode. The polarization in the layers of gas near the electrodes gradually rises, and eventually the normal anode and cathode falls of potential are established.

Among other results, the experiments show that a current, accompanied by a glow in the gas, may be maintained when the fall of potential in the neighbourhood of the cathode is a small fraction of the normal cathode fall of potential.

These results show that some of the recent theories which have been proposed to explain the maintenance of a current are greatly at fault.

The phenomena which accompany the discharge of electricity in vacuum-tubes have usually been studied with small electrodes and large currents, so that experimenters have concluded that the cathode fall of potential is a constant and independent of the current.

It has been suggested that the normal cathode fall of potential is the potential required to impart a sufficient velocity to the positive ions in order that they may produce new ions on colliding with the cathode. If such a potential were required, it would be difficult to explain the maintenance of a current in cases where the fall of potential in the given layer near the cathode is much smaller than the cathode fall of potential.

A method has been given by Professor Thomson (J. J. Thomson, 'Conduction of Electricity through Gases,' p. 381) of calculating the spark-potential by finding the falls of potential along the different portions of the distance between the electrodes when the gas is carrying a current and the normal cathode fall of potential is developed. It is difficult to see why the potential so calculated should be the sparking potential, as it should represent the potential required to maintain a current which may be several hundreds of volts less than the sparking potential. Also, the calculated potential is independent of the current, whereas experiments have shown that the potential required to maintain a current depends on the value of the current as well as on the pressure of the gas and the distance between the electrodes.

This theory, therefore, does not give a satisfactory explanation of these potentials, as there is no distinction drawn between the sparking potential and the potential required to maintain a continuous current.

LXXVI. *Extract from a Letter dated Cleveland, Ohio, August 5th*, 1904, *to* Lord Kelvin *from* Profs. EDWARD W. MORLEY *and* DAYTON C. MILLER.

IN 1887 Michelson and Morley made an experiment on the relative motion of the earth and the luminiferous ether. They could detect no relative motion equal to one sixth of the earth's velocity in its orbit.

To explain this result, FitzGerald and Lorentz suggested that the stone slab on which the apparatus was constructed might have its dimensions changed by its drift through the ether.

There was a remote chance of detecting such an effect by repeating the experiment of 1887, but with different materials. If the FitzGerald-Lorentz effect exists, it may affect all materials to the same amount, independently of the nature of the material. But it is also possible that the effect is one which depends on the physical properties of the material, so that pine may be affected more than sandstone. In this case, if sandstone gives no displacement in an experiment like that of 1887, an apparatus supported by pine, which would be compressed more than sandstone, would give an effect of the sign opposite to that suggested by the original simple theory.

Such an experiment has now been made. We first made a structure of pine, which we floated on mercury as in 1887. While this structure was new, we obtained good observations with it. But after the wood had been affected by steam-heat for one season, it was not possible to maintain it in adjustment for five minutes. We therefore made a structure in which the distance of the mirrors depends on pine rods, but all other circumstances depend on steel. Two very stiff steel girders cross symmetrically, and are floated on mercury. Two holders, each carrying four mirrors, are fixed at the ends of two arms of this cross. Two other mirror-holders are suspended freely. Pine rods reach from the fixed holders to the free holders, and springs maintain stable contact. These pine rods are carried in brass tubes which constitute a sort of truss; the distance of the two sets of mirrors depends solely on these pine rods.

Observations were made by noting the place of the central black fringe on a kind of eyepiece micrometer. They were made at sixteen equidistant azimuths, and commonly at a rate of one revolution in little more than a minute, readings being made to the tenth of a wave-length. Two times were

selected in which the motion of the earth was in the plane of the apparatus. In the first part of July, the direction of motion at these two times apparently differed by 115° ; the morning and evening sets of observations were combined by taking into account this difference. In all, more than 250 turns were observed.

The velocity of the earth in July may be assumed to be 33·5 kilometres a second. The length of path in our apparatus was 32·2 metres, and the effect to be expected (on the original theory) is 1·4 wave-lengths.

We have established that, if there is any effect, it is not more than 0·015 wave-length.

LXXVII. *Notices respecting New Books.*

The Theory of Heat. *By* THOMAS PRESTON, *F.R.S.* *Second Edition,* revised by L. ROGERSON COTTER, M.A. (Dubl.). Macmillan & Co., Ltd. 1904.

IN is nearly ten years since the late Professor T. Preston wrote his ‘Theory of Heat.’ The work was written in a marvellously short time, taking into account its wealth of carefully compiled abstracts from recent sources as well as its fresh and interesting treatment of the classical literature of this branch of science. The book, in short, had merits which placed it at once among the best textbooks on Heat in this or in other languages. There were, however, omissions, felt to be such even then ; and, of course, these omissions have been growing with each important advance of the science.

The second edition of the textbook is now in our hands, and before offering any detailed remarks we heartily congratulate Mr. Cotter upon his editorial work as a whole. Mr. Cotter has added about 100 pages, not one of which is superfluous, to the work ; as well as many references which will be of value to students, both old and young. We proceed to make some very brief comments upon the additions.

In the first chapter we find a much needed exposition of Maxwell's law of the distribution of molecular velocities in gases. In this chapter Preston discusses atomic theories ancient and modern. It is a pity, for the sake of completeness, that the editorial work on this chapter was hardly recent enough to permit of a paragraph on the modern evidence in favour of a highly complex kinetic atom. In chapter II. on Thermometry, Mr. Cotter's additions on the Lag of Thermometers, Pressure Coefficients and Errors due to Capillarity or to Emergent Column, are excellent. In Pyrometry and Low Temperature Thermometry, a *résumé* of recent methods depending on thermo-electric effects, viscosity of

gases &c. is given. In chapter III. on the Dilatation of Solids, Liquids and Gases, the Editor adds the interesting observations of Tait on the properties of fresh and salt water at various pressures and temperatures. Here we are once again reminded of the classical experiments of Dulong and Petit and of Regnault on the absolute expansion of mercury, and may, perhaps, be permitted a brief digression as to the determination of this valuable constant in a less cumbersome manner. If the mercurial barometer is raised in temperature uniformly, the column lengthens proportionately to the diminishing density of the mercury and independently of the expansion of the envelope. Comparison with a standard barometer affords, then, the absolute expansion coefficient. In carrying out the experiment the vacuous extremity of the barometer is extended upwards or bent to one side and kept cool to avoid the correction for vapour-tension. The heating is done by a steam-jacket. The troublesome separation of the hot and cold mercury columns is here eliminated.

Several additions are made under the section treating of the dilatation of gases, such as Witkowski's experiments on the thermal expansion of air; Chappuis' on the pressure coefficient; Ramsay's and Travers' on the expansion and pressure coefficients of krypton, argon, and helium.

In the chapter on Calorimetry we find a very full account of Lussana's labours on the specific heats of gases at constant pressure by a modified Regnault method. The reference to the important subject of heat of combustion might with benefit be extended. In this chapter the consideration of the Dynamical Equivalent finds a place. The section is excellent, but we think, as perhaps the central fact in the study of Heat, it would have been better to have devoted to this subject a separate chapter. The Editor adds the large-scale experiments of Reynolds and Moorby, Griffiths' electrical experiments, as well as a short account of the work of Schuster and Gannon, of Callendar and Barnes, and finally a comparison of all. The section is further improved by a clearly worded discussion of the variation of the specific heat of water and the choice of a thermal unit.

The chapter on Change of State is enriched by articles on the modern methods for the liquefaction of gases and attainment of low temperatures; by Griffiths' experiments on the latent heat of steam, and by Lord Kelvin's beautiful calculation of vapour-pressure at curved surfaces of liquids. Aitken's experiments on the cloudy condensation of aqueous vapour produced by dust might with advantage be referred to here. We think, also, that M'Connel's very important observations on the plasticity of ice should be given a place in the body of the text and not be confined to a footnote. Erroneous references to this matter are common in current geological literature.

The principal feature of Mr. Cotter's editorial work is the treatment of the subjects of Radiation and Absorption. These sections are almost entirely his own, and we think they will be

adjudged to be some of the best parts of the book and show the Editor to possess a severe scientific training as well as an excellent scientific literary style : absolutely clear and to the point ; judicial without affectation. Here we find, naturally falling into their places in the orderly development of the subject, the laws of Kirchhoff and Stefan ; the experiments of Lummer and Pringsheim on the laws governing radiation within an enclosed space at uniform temperature, and on the distribution of energy in the spectrum of a black body ; also Boltzmann's proof of Stefan's law, Wien's general law and his displacement law. The ensuing section on the measurement of temperature by radiation is equally good, and Mr. Cotter's own. Poynting's interesting estimate of planetary temperatures based on Stefan's law, and W. E. Wilson's estimate of Solar temperature are in this section. By an oversight the paper in which Mr. Wilson gives his results is not referred to.

Chapter VIII. on Thermodynamics is not materially altered from the first edition. Alteration was not required. Callendar's method of correcting the gas-thermometer as well as Olszewski's determination of the temperature of inversion of the Joule-Kelvin effect for hydrogen are added here.

The reader of our own time of life will doubtless share our feeling that we had been fortunate had we possessed so comprehensive a textbook in our earlier student days. For equally comprehensive treatment we had to go abroad. In this way, indeed, we learnt some French and German. Perhaps the English student will be induced by such textbooks as the ' Theory of Heat ' to postpone his study of scientific French and German till a later time of life. Should evil arise from this, the blame is upon our school-system and not upon the expounder of Science who labours to remove a reproach from our scientific literature. If too late for our own young days we none the less welcome, as work of reference and for its pleasant reading, the Treatise on Heat which Mr. Cotter has now brought up to date.   J. J.

La Revue Electrique. Publiée sous la direction de J. BLONDIN. 1ʳᵉ Année. Tome I.—No. 1. Paris: Gauthier-Villars, 1904.

IN these days of keen competition and intense literary activity, it takes no small amount of courage to add to the mass of scientific an d technical periodicals already in existence. Yet the future of the above new publication appears to be full of promise, as the edit or has been fortunate in securing as contributors such wellkno wn experts as MM. Armagnat, Charpentier, Guilbert, J. Guillaum e, Maurain, Turpain, and others. The first number contains several interesting articles, and if the present high standard is maintained, there should be a useful future before *La Revue Électrique*

Recherches sur l'Effet Magnétique des Corps Electrisés en Mouvement.
Par M. N. VASILESCO KARPEN. Paris : Gauthier-Villars. 1904.
Pp. 114.

To all whose equanimity has been seriously disturbed by the
heretical announcements of M. Crémieu, directed against one of
the foundations of modern electromagnetic theory, we can cordially
recommend M. Karpen's monograph on the subject. In addition
to a very full account of the author's own interesting experiments,
it contains a connected historical account of the matter, and is
sure to be read with deep interest by all physicists.

Saggio di Una Teoria Generale dei Fenomeni Naturali. Di Dre
PARISINO PETTINELLI. Bertoletto e C. : Savona. 1904. Pp. 79.

*Un Nuovo Procedimento per Trovare Molte Relazioni Note ed Ignote
fra le Quantità Fisiche. Di* Dre PARISINO PETTINELLI. Fratelli
Isoardi : Cuneo. 1902. Pp. 70.

IN these two pamphlets, the author deduces a large number of
new and interesting relations between physical quantities by the
aid of dimensional equations. The simple manner in which some
of the results arrived at are obtained is very striking. Part of
the first pamphlet contains a section dealing with social, moral,
and even theological questions.

Leçons sur L'Électricité. Par ERIC GERARD. Tome Premier.
Septième Édition. Paris : Gauthier-Villars. 1904. Pp. xii
+882.

THE seventh edition of the first volume of this standard treatise,
so deservedly popular, fully maintains the reputation of its pre-
decessors. The work of revising and bringing it up to date seems
to have been carried out with exceptional thoroughness, and in its
present form it is an essentially modern text-book, in the best
sense of the term. The happy combination of a sound basis of
theory with technical detail is the most striking feature of the
book, and no doubt largely accounts for its phenomenal success.
The exposition is characterized by elegance and clearness, and the
reader's interest is maintained throughout ; there is further a
certain atmosphere of freshness about the book which is altogether
lacking in some rival treatises. In dealing with problems involving
more or less complicated calculations, the author seems to have
succeeded in choosing the simplest and most direct methods ; and
any reader who has mastered the elements of the differential and
integral calculus should be in a position to follow all the mathe-
matical developments contained in the book with the greatest
ease. It is a thoroughly practical book for practical men, and
exhibits that peculiar restraint, that instinctive appreciation of what
to retain and what to reject, which at once reveal the practised
touch of the expert teacher. The result is a work which, while

dealing with a subject of vast extent, contains everything that may be regarded as essential to the electrical engineer, and is nowhere overloaded with theoretical developments which from the practical standpoint present but little interest. There are few books which we could so confidently recommend to the student of electro-technology as Professor Gerard's admirable treatise.

Teoria Matematica dello Equilibrio dei Corpi Elastici. Di ROBERTO MARCOLONGO. Ulrico Hoepli: Milano, 1904. Pp. xiv + 366.

WITHIN the comparatively small compass of 366 pages of a pocket volume, Professor Marcolongo has succeeded in giving a fairly complete outline of the theory of elasticity. The first two chapters contain introductory mathematical matter, and deal with harmonic functions, Green's theorem, and the Newtonian potential function. In Chapter III. we are introduced to the general principles underlying the dynamics of a continuous body. Chapter IV. deals with the equations of equilibrium and motion of an isotropic elastic solid, Chapter V. with those of a non-isotropic solid, Chapter VI. with some general theorems, Chapter VII. with the problem of Boussinesq and Cerruti, Chapter VIII. with the deformation of an isotropic sphere, Chapters IX. and X. with St. Venant's problem of the deformation of a cylindric shaft and the complementary problem, and the concluding Chapter XI. with the deformation of a crystalline solid. To the advanced student of mathematical physics, Professor Marcolongo's little book should prove most useful.

Bibliography of Quaternions and Allied Systems of Mathematics. By ALEXANDER MACFARLANE, *D.Sc., LL.D., F.R.S.E.* Printed at the University Press, Dublin.

THIS important bibliography is published by the International Association for Promoting the Study of Quaternions and Allied Systems of Mathematics, of which Dr. Macfarlane is General Secretary. The matter is arranged under the names of the authors, and includes works, memoirs, and notes on quaternions and other systems of vector analysis, on Ausdehnungslehre, Universal Algebra, Complex Numbers, and Matrices. No pains have been spared by the compiler to make the list as complete as possible; and all workers in those difficult but growingly important departments of mathematics will find the booklet very useful for reference. To ensure accuracy and completeness every living author named received a proof of the bibliography, and Dr. Macfarlane had the gratification of getting corrections and additions from nearly all. Those who are interested in the history of this International Association will find the first steps chronicled in 'Nature,' vol. lii., in an appeal from Kimura of Tokyo and Molenbroek of Leiden.

CURVES OF ABSORPTION OF α RAYS OF RADIUM

ORDINATES. DISTANCES OF RADIUM FROM IONIZATION CHAMBER IN CENTIMETRES
ABSCISSÆ. IONIZATION ON ARBITRARY SCALE
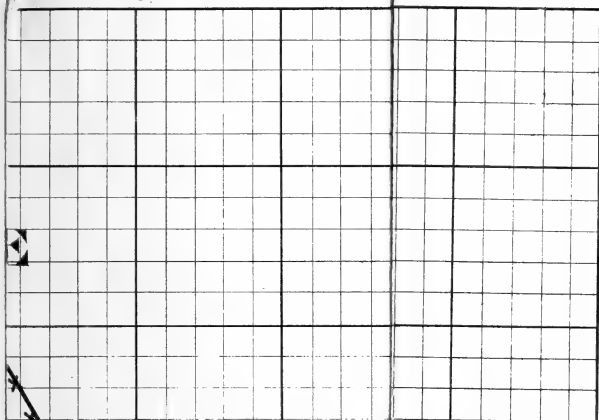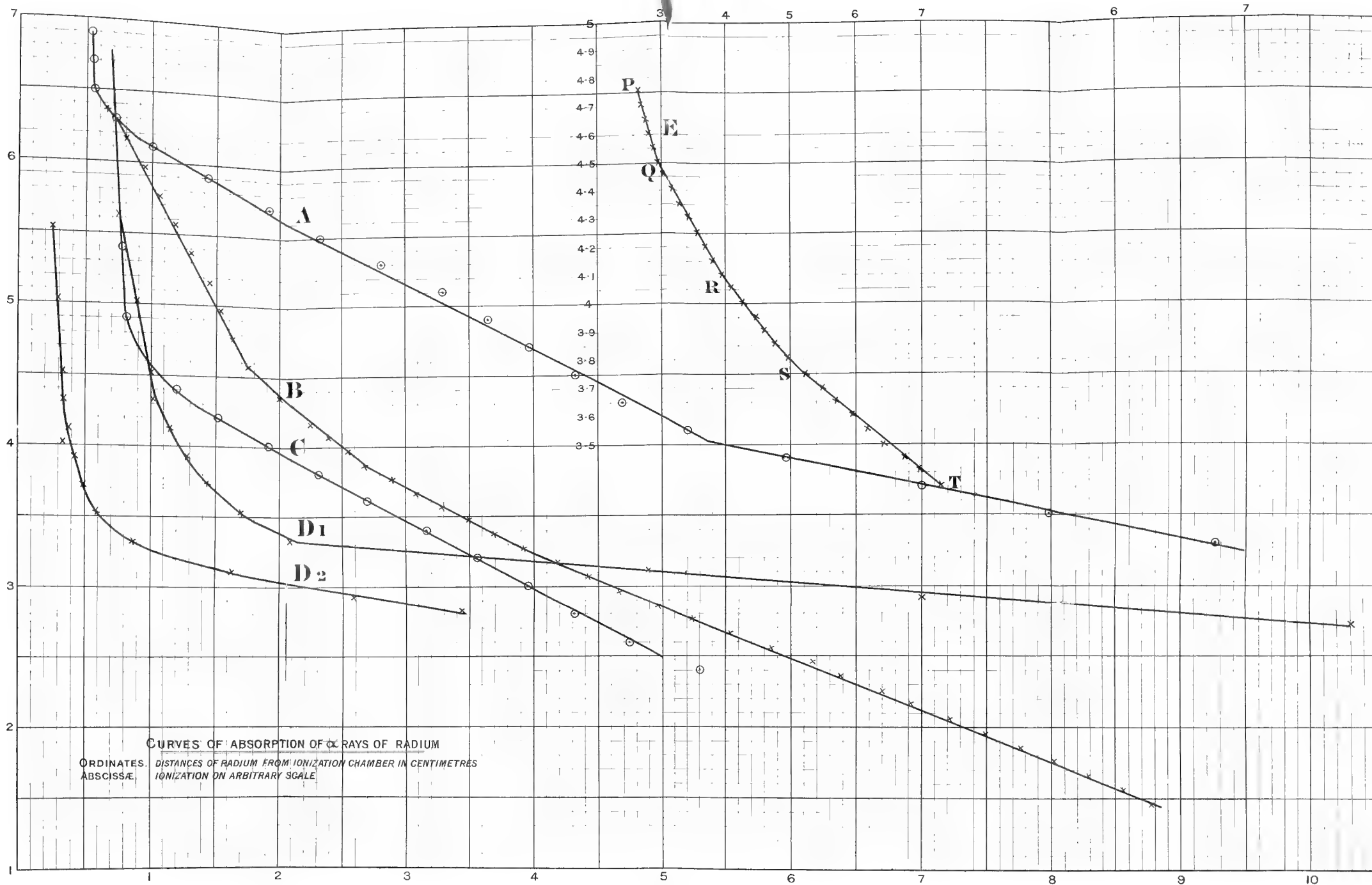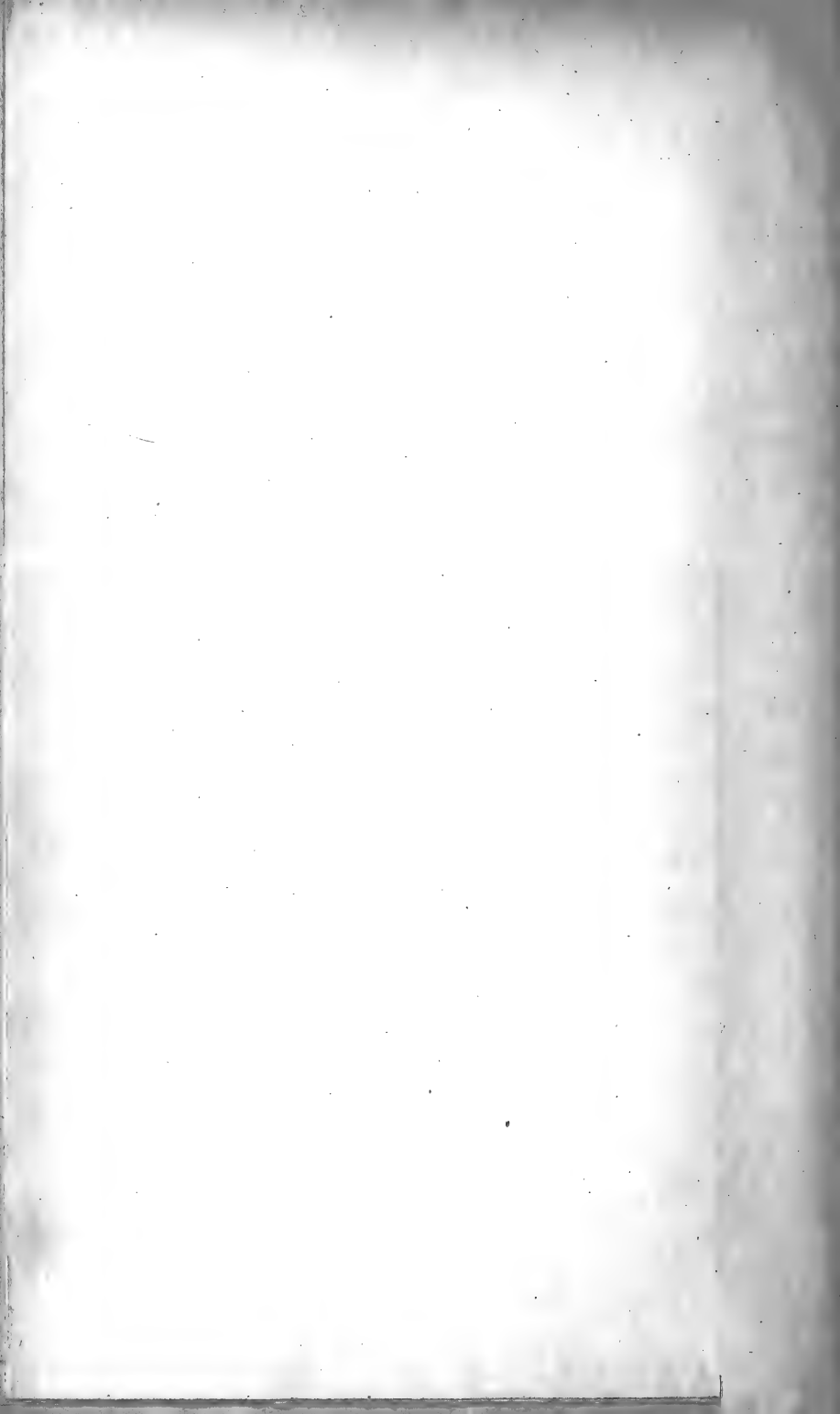
INDEX to VOL. VIII.

———◆———

α RAYS, on the absorption of, 719.

Actinium, on the decay of the excited radioactivity from, 373.

Æther, on the relative motion of the earth and, 716, 753.

Aichi (K.) on the theory of the rainbow due to a circular source of light, 598.

Alternating currents, on instruments for the measurement of, 91.

Anderson (Prof. A.) on the force on a magnetic particle in a magnetic field, 38.

Anomalous dispersion of sodium vapour, quantitative determination of the, 293.

Arnold-Bemrose (H. H.) on quartzite-dykes near Snelston, 415.

Atkin (A. J. R.) on the gold-deposits of Barkerville, 415.

Atmospheric friction on even surfaces, on the, 58.

Atomic weight of radium, on the, and on relationships between the atomic weights of the elements and their spectra, 279.

Atoms, on a plan of a combination of, to have the properties of polonium or radium, 528.

Austin (L. W.) on cathode disintegration in the discharge through gases at low pressures, 145.

Barrow (G.) on the Moine gneisses of the East-Central Highlands, 291.

Bars, on the lateral vibration of, 581.

Beilby (G. T.) on the hard and soft states in metals, 258.

Bismuth and tin, on the magnetic susceptibility of alloys of, 49.

Books, new:—Korn's Elektrische Fernphotographie und Ähnliches, 112; Poincaré's Théorie de Maxwell et les Oscillations Hertziennes,
112; Rutherford's Radioactivity, 284; Stokes's Mathematical and Physical Papers, 287; Schmidt's Die Kathodenstrahlen, 287; Kühling's Karl Heumanns Anleitung zum Experimentieren bei Vorlesungen über anorganische Chemie, 288; Reade's Evolution of Earth-Structure, 572; Curie's Untersuchungen über die Radioaktiven Substanzen, 572; Soddy's Radioactivity, 659; Jordan's Astronomical and Historical Chronology in the Battle of the Centuries, 660; Righi and Dessau's Die Telegraphie ohne Draht, 661; Hann's Jelineks Psychrometer - Tafeln, 661; Noyes's Text-book of Organic Chemistry, 662; Righi's La Moderna Teoria dei Fenomeni Fisici, 662; Loppé's Essais Industriels des Machines Électriques et des Groupes Électrogènes, 662; Carnot's Traité d'Analyse des Substances Minérales, 663; Preston's Theory of Heat, 754; Karpen's Recherches sur l'Effet Magnétique des Corps Électrisés en Mouvement, 757; Pettinelli's Saggio di Una Teoria Generale dei Fenomeni Naturali and Un Nuovo Procedimento per Trovare Molte Relazioni Note ed Ignote fra le Quantità Fisiche, 757; Gerard's Leçons sur l'Électricité, 757; Marcolongo's Teoria Matematica dello Equilibrio dei Corpi Elastici, 758; Macfarlane's Bibliography of Quaternions, 758.

Boulton (Prof. W. S.) on the igneous rocks of Pontesford Hill, 667.

Bowlker (T. J.) on the lengthening of the spark from an induction-coil by the aid of auxiliary wires, 487.

END OF THE EIGHTH VOLUME.